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for full contents see inside page

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CONTENTS OF VOLUME LXIX

ARTICLES

BOOK REVIEWS

CURRENT BOOKS

LETTERS TO THE EDITOR

ERRATA

THE AMERICAN JOURNAL OF

SOCIOLOGY

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JULY 1963

THE POLITICAL ACT AS AN ACT OF WILL

NORTON E. LONG

ABSTRACT

The problem of order is the central problem of politics. A political society must achieve a minimum degree of stability to survive. Equally, it must functionally adapt to change. The objective situation imposes constraints but does not provide determinate answers. Like the artistic tradition, the political tradition provides materials. The combination and recombination of these materials within the constraints of the objective situation is a creative act of political will. This act, by defining and redefining the situation, permits the actors to co-operate. It cuts the Gordian knot of the cognitive problem.

It was the great merit of Hobbes to raise the problem of order—to recognize that the existence of order is problematic. As contrasted with Locke, Hobbes emphasized the ever present immanence of anarchy, the latent tendency of society to dissolve into a warring heap of individuals. These two views of the problem of order—Locke's, which sees the ordinary customs and routines of society as making the possibility of anarchy highly marginal, and Hobbes's, which sees the problem logically if not existentially as salient and ever present—suggest radically different approaches to the key problem of politics: How is order maintained? For Locke, it is largely a gift of nature, a product of the common-sense functioning of the ordinary society requiring no special accounting for. For Hobbes, it is a living miracle that, despite the multiple forces working to cause the human particles to fly apart, they in fact successfully cohere. His explanation is in an act of will rather than reason or custom, in an act of power.

The emphasis on force in Hobbes has had the unfortunate effect of limiting our appreciation of the potential range of his argument. If we consider the problems of order and anarchy not just in the state but in organized activity generally, we get a better understanding of the thrust of Hobbes's position. When he says in the *Dialogue of the Common Laws* what is law but that as in whist if nothing else is turned up clubs are trumps, he insists on the sheer arbitrary nature of the initial norm that provides the ground of action. You can call a man for cheating when you have a game with rules. Until you have rules, you have no accepted standard. The creation of the game, the *creatio ex nihilo*, God's will—all have elements of the arbitrary fiat that provides the basis for the orderly and the rational. As St. Augustine says, the will is prior to the reason since it focuses the attention. *Crede ut ingellegas.*

The supreme and even the mediate political act is that which defines the situation and, by giving congruent definitions to the actors, permits their co-operation. As Professor Richard McCleery, of Antioch College, has pointed out, the essence of anarchy is the existence of incompatible and incongruous definitions of the situation among the actors. The essence of order is the reverse. For Hobbes, the objective situation provides

no assurance that the actors will arrive at appropriate definitions except through a political act of will backed by force.

One may be repelled by the emphasis on force as the *ultima ratio*. Perhaps one would be less repelled by the notion that what one was concerned with was an act of will, an act that is only arbitrary in the sense that politics is not a science, but an art in which those who disagree with you are not simply wrong on the facts but differ in their preferences. In Looke's realm of stable unquestioned values, the problem seems merely one of right reason, since the value premises of action are treated as non-problematic, and the fact premises as readily and commonly ascertainable. In fact, when value premises and fact premises are multiple and in doubt, anarchy can only be avoided by decision. There is a sociology of Locke and a sociology of Hobbes. In fact, the two prescriptions can coexist at different levels of the governmental hierarchy simultaneously.

In the lower level of the bureaucracy, the situation may seem as clear as daylight; use and wont prevail. It is almost a moral certainty that tomorrow will be like today, which was like yesterday. At the upper reaches, in the unstructured or only partially structured situation that there prevails, doubt may reign, doubt as to the applicable norms, doubt as to the facts, doubt as to what, if any, known principles apply. At one level, the morals of the structured situation prevail, at another, the jungle of anomie; at one level, the logic of Simon's "administrative man," at the other, the passion of his "uncertainty absorber" who must digest the realm of doubt and produce the affirmations of leadership.

The assumed easy applicability of a generalized common sense obscures the problematic character of decisional reality. The almost unconscious application of commonsense rules of interpretation makes the actor ordinarily assume he confronts a clear and unambiguous situation to which there is a clear and readily ascertainable answer. In fact, even when repeated experience has shown that the most common event of everyday life is difficult to reconstruct and account for with any degree of accuracy, the actor will, on most occasions, proceed as if the common-sense view, on which he reads and runs, gave him adequate grounds for his decisions. Two reasons would seem to account for this being the case: gross failure is normally not experienced, and, in all probability, a critically self-conscious scientific skeptic would go mad in his indecision.

Yet, the cognitive problems are not easy. Political scientists often act as if there were a political science available to the actors of politics, even though no such science is available in their writings. The number of tested, "if then," propositions in the literature to which one can turn for even factual guidance about uniformities of political behavior are meager in the extreme.

Conventionally in international relations, we expect the subject to be filled with doubt as to the intentions and capabilities of one's enemies, one's allies, and neutrals. Intelligence, in its military and other senses, is taken as a major part of the problem. In ordinary politics much the same cognitive problems obtain. They are obscured by the unstated assumption that the common-sense interpretation of the situation provides an adequate ground for action. While in particular case after case the adequacy may be found wanting, the general assumption of adequacy is a necessary condition for human action. The common-sense properties of the situation thus become the very ground of the social order. That they should be adequate is the implied contention of Locke. That they should be considered as radically contingent is the position of Hobbes. On the one hand, order may be regarded as the result of an ordinary application of intelligence to the readily ascertainable fact of an objective order independently given in the real world; on the other, order may be regarded as the result of a fiat imposing itself on a chaos of ambiguous possibilities.

Obviously, except in the logic of a Hobbesian state of nature, one is not presented with presocial or prepolitical man. In every real state that we confront, men are to some degree endowed with the language and habit of political action. But equally in every real

state, we can imagine and some times see the conditions under which order disintegrates and anarchy supervenes. The restoration of order depends not on a unique solution of the problem by reason, but on the willed selection from the possible of a course of action and its imposition. Two principal methods of solving the problem of order are exemplified in different stages of society: the traditional society with its use and wont, and bureaucracy with its hierarchy, routines, and indoctrination. The insufficiency of these methods lies in their incapacity to deal with novelty and change except by the gradual piecemeal increment of random experiment that they are largely designed to check. In fact, the system's capacity to restrain that which would alter it is in large measure what produces its essential stability. The two problems of any organization, that of securing sufficient stability to avoid the forces of disintegration that exist within it and beat upon it, and that of functionally adapting to change so as to survive in an altered environment, are the essence of the general problem of politics. The needs of innovation and the needs of stability frequently conflict.

Confucius in his sayings somewhere remarks, "Gravely the emperor Wu sat on his throne and stroked his beard. That was all. That was enough."

This is Simon's uncertainty absorber of the traditional society.[1] It resembles closely the naval pattern of a remote ceremonial skipper with an active mundane executive officer. The patterns of the bureaucratic head and the traditional society head resemble each other in the leadership task of personally representing and exemplifying the ceremonial cult of the conventional norms that order the society. In both cases, an innovative and creative role is marginal and even, to a degree, antithetical to the conduct that is publicly expected.

In the unstructured environment of the upper echelons of government, or in the unstructured environment of the society in which traditional norms have been undermined by outside impacts and the effects of change too rapid to be handled by piecemeal adaptation, positive leadership imposing a redefinition or a new definition of the situation is called for to stabilize the common premises of action. There is a continuum from the leadership of an organization caught in change to the leadership of a society whose unconsciously accepted rules of conduct and folk common sense no longer suffice to give it direction and coherence. As Simon has remarked, the innovative role is probably the most significant of those of the top executive.[2] This is not incompatible with, but differs in emphasis from, Selznick's view which emphasizes the function of the executive in preserving the character of the organization, an almost Aristotelian conception that the preservation of the constitution of the organizational polity is the supreme political act.[3] Given the Greek view of the primary problem of politics as stasis and of stability as almost the highest value, such an emphasis is neutral. Given a modern Darwinian view of natural selection and the survival of the fittest, a view that emphasizes the constant necessity to adapt to survive, concern with organizational capacity to innovate functionally is equally understandable.

Except where the traditional society or socialization into an established bureaucracy solves the problem of providing the value premises of action, Simon's administrative man and political man are bedeviled with the problem of valuation.[4] The problem of choice cannot be reduced to logical analyses or scientific inquiry. It entails a choice among values and, all too often, an act of will that seeks amid value and factual uncertainties to assert the affirmations that constitute Simon's value premises and even

[1] James G. March and Herbert A. Simon, *Organizations* (New York: John Wiley & Sons, 1958), p. 165.

[2] See Simon's essay in Sidney Maillick and Edward Van Ness, *Concepts and Issues in Administrative Behavior* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1962).

[3] Philip Selznick, *Leadership in Administration: A Sociological Interpretation* (Evanston, Ill.: Row, Peterson & Co., 1957).

[4] March and Simon, *op. cit.* pp. 137–42.

many of his fact premises on which action in a confused, time-bound world must depend. The failure to realize this has vexed many a group of civic and business notables who, having hired their research team of experts and academics, expected the facts to provide the answers and save them the political heat and perplexity of choice. The reduction of the political problem to a scientific problem is a natural result of the confusion of propositions of value and propositions of fact. It is also a result of the human desire to escape the sheer anguish of creative decision. Leadership is concerned with the transformation of doubts into the psychological grounds of co-operative common action. The resolution of doubts does not mean the resolution of the leader's doubt, only his capacity to manifest the external role, verbal and otherwise, that permits and engenders co-operative functional behavior on the part of others.

The interpenetration of the realms of certainty and uncertainty is subtle and must, for effectiveness in action, be unconsciously or consciously and resolutely ignored. This has been previously referred to in the case of the problematic nature of common-sense reality which, for almost all ordinary purposes, must be treated simply and as non-problematic and unequivocally given. Professor McCleery's very suggestive study of prison society has pointed to the source of the old cons' power in their capacity as myth-makers to provide an acceptable explanation of the random system of terror designed by the guards to cow the inmates. That the myths were myths did not prevent their functionality for reducing the unbearable burden of the unknown. Populating the unknown with some plausible explanation of events has been the function of priests, leaders, and philosophers from the beginning of time. The freezing voids of a scientific world are uninhabitable except as warmed by the creative poetry of the imagination backed by the will to give it credence. One would not suggest that a Hopi Indian rain dance was superior to irrigation as an agricultural technique. What is suggested is that the rain dance and its equivalent in modern society are functional for the reduction of tensions that permits ordinary life to go on. Much of political life consists in blasting locusts with bell, book, and candle in the absence of DDT. The social placebo given with an air of medical wisdom and assurance is no less curative for collective than individual neuroses. Even without the buildup that Erving Goffman finds so necessary to the act, a forceful affirmation can infuse a group with the confidence the leader must exhibit while masking his inner uncertitudes.

How profoundly uncertain the knowledge of everyday life is, is hidden by the rarity of reflection concerning the adequacy of common sense, the rationalization of the course of events, and the literary conventions of the plausible narrative. The abyss beneath one's feet must be ignored if sanity and balance are to be maintained amid the depths of the multiple unknowns, the informational voids, the normative uncertainties that surround us but of which we are routinely blissfully ignorant. Only those who have toyed with the world of the psychiatrist and the psychologist or those with the artistic imagination, appreciate the thin line of sanity that seems so certainly and monotonously to be neither thin nor in danger. The doings of the OAS, the stolid English capacity for deteriorative violence in Kenya seem alien to the well-adjusted norm of the ordinary rather than the contingent savage Hobbes saw in all of us.

The actors in the world of politics are neither the rational calculators of economic man nor the uncultured savages of Hobbes's state of nature but are born into a political culture, albeit frequently an ambiguous one, and are socialized to a range of response patterns that may be invoked by diverse stimuli. With this equipment, they confront a reality that seems, to each public, one-dimensional common sense but, in fact, through the differing glasses that it is viewed, presents widely differing perspectives. The actors possess a repertory of responses that are articulated into roles and interacting patterns of roles much like the repertory of plays of a stock company.

Given what seems to each the appropriate stimulus, a given play is called for on the stage in which persons perceive themselves as having a defined role or, as members of the audience, have an expectation as to how the performance on the stage will be presented. The existence of these patterned sets of roles is part of the technology of the political culture and permits the actors to function with the same ease as a ballplayer playing his position.

Ambiguity lies in the differing stimuli that are simultaneously present, differing interpretations of the same stimuli because of differences in the equipment and perceptions of the actors and the fit of the plays to the objective situation.

A governor confronted with the necessity of deciding whether or not to call a special session of the legislature to raise taxes to meet a fiscal emergency might use a decision-making model in which he would "game" the actors to see how they might rationally weigh the ranges of possible and probable outcomes. While such a calculus is not without value, it is in most cases less relevant than a consideration of the repertory of plays with which the actors are equipped and those they can be made to consider, the occasion, and the audience demands. Thus, there are objectively a number of possible pieces of theater that range from "let's choose up sides and clobber each other and the governor," to "let's rally round the flag, boys, and rise to the statesman-like task of saving the state." At critical points, there is considerable leverage for leaders to manage the stimuli that they are publicly expected to produce so that they can determine the play considered appropriate by both actors and audience. Leaders are not working with empty-headed malevolent savages or disembodied calculators, but with culture-bound actors whose political-art tradition is rich in materials to which the ordinary imagination is conditioned.

The most significant political act is that of innovation through which the existing materials are recombined and developed in in such a way as to unlock new human potentialities and overcome problems in physical and human nature. For example, the alteration of value patterns, operative political philosophy, is a major task in the political job of racial accommodation in a swiftly changing environment.

Were Hobbes's act of will, or the innovative, creative act of political art, writing new plays for the actors to perform, a constant necessity, rather than an intermittent and contingent one, the psychic cost might well be more than human nature and its systems of action could bear. Fortunately, custom, habit, and bureaucratization provide a framework of systemically functional routine that gives society a built-in automatic pilot that permits relaxation into common sense, and the segmental rationality of the highly structured role and atmosphere of the laboratory and the office. Indeed, for the most part the innovative and adaptive process by which the human system solves its problem is not through the self-conscious role of leadership, but through the random responses of individual and institutional actors to challenges in the environment which, when successful like the wheel and the union, are imitated and diffused within the common culture.

While one can and must lay great stress on the working of the social and political system as a system, as an ecology of institutions and actors where interaction produces unintended but systemically functional outcomes, one must not ignore the voluntary elements that maintain the culture. In the most dramatic case, missionaries undermine the native beliefs and, unsuccessful in replacing them with others, produce a condition that results in even biological decline. One may know this as a matter of scientific knowledge without possessing any cookbook recipe for the creation of an effective religion to meet social requirements. But quite beneath the great questions of political theory, the moral architecture, constructed like regions to give form and warmth and co-operative possibility to the life of men, there are the everyday acts of political will that signal the traffic of affairs and permit it to move.

President, governor, and mayor are called

on to provide programs, acts of will, that focus the attention of public and legislature on some of the myriad things to which they might attend. They are called on to select from the currently acceptable diagnoses of social problems or, more rarely, to create their own and present them to the public. Their definitions of the situation, ranging from the fertility problems of prosperity and depression to the hostility problems of peace and war, not only create the conditions for co-operation among the bureaucracies of government but among the public at large.

In this broad field of symbolic manipulation exist deep-rooted public and individual behaviors going back in history to prehistory and to tribal patterns, and in childhood development to the deeply etched patterns of juvenile behavior.

Responses are ready to be triggered, and while, even from the special vantage points of political leadership, many stimuli fail to connect, these vantage points are the publicly expected sources of direction. Even the opposition requires a definition of the situation from the leadership in power to permit it to effectively play its role of adversary co-operation. Press and public, elite and mass, need a score card against which to follow the game. In the absence of some sufficient lead in defining the situation, the complex co-operation of the actors that rests on a sufficiently common definition of the situation for even adversary co-operation breaks down into stalemate and anarchy.

This is an ever present danger where use and wont and the great stabilizing force of bureaucracy have not replaced the unstructured or partially structured situation with a realm of commonly accepted, largely unquestioned definitions. But the very motor of change is the unstructured, partially structured, structuring situation where possibilities are many and things are "up for grabs." Uncertainty, risk, amorality, and profit coexist and powerfully motivate the actors to explore an expanding and radically unknown and unexplored universe. The universe becomes known as the game to be played, emerges, is created, and its rules are known, enforced, and accepted. At any time the players may fall out, become bored, wish to innovate, feel the pinch of its inadequacy, and in a thousand ways present difficulties to the maintenance of a smoothly functioning order. Socialized habits and acts of political will both are necessary to preserve the system.

In the everyday routine of life, the problematic nature of reality is made up of a multiplicity of potentially applicable norms cutting in different ways, a fragmentary state of information, an absence of any relevant substantial amount of scientific knowledge and a pressure of time flooding by constraining decision on the most precarious definition of the situation. No certain rule exists as to whether to invoke the norms of loyalty to the chief, personal loyalty of friends, loyalty of institutional identification, professional loyalty, loyalty to a conception of the public interest with all its ramifications of city, state, national, and international publics and their variant interpretations, loyalty to geographic community, loyalty to party and part of party, loyalty to race or nationality, loyalty to religion or religious groups or many more.

The multiplicity of norms, as well as the fragmentary and multiple nature of the perceived facts, contributes to the widely varying definitions of the situation that determine what Lipmann called the pictures in our heads. The cognitive problem can at best be partially solved in its own terms. As St. Augustine pointed out, the will focuses the attention and the norms invoked determine the interpretation of the relevant reality. The selection of the norms and the creation of a stable definition of the situation out of the manifold uncertainties of the reality masked by common sense is an act of political will. It cannot be escaped by resort to science or the expert. It is the problem of choice, the choice of a possible order in a world that provides no substitute for the human act of moral will.

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KNOWLEDGE, DANGER, CERTAINTY, AND THE THEORY OF MAGIC[1]

LIONEL S. LEWIS

ABSTRACT

Three variables, knowledge, danger, and certainty, have been logically isolated by social scientists as conditions that lead to the utilization of magical behavior. This paper reports an empirical study in which these variables are shown to relate differently to three subcategories of behavior that are generally lumped together under the generic term "magic." It is suggested that a reformulation of "the theory of magic" is necessary so that a delineation can be made between different types of behavior in which means are inappropriate for an intended end.

In the formulation of "the theory of magic," three different conditions have been designated as antecedent to the utilization of magical behavior. This paper reports an empirical study in which the place of each of these conditions in "the theory of magic" was examined.

Magic, first of all, has been viewed as being bad science. Persons holding this position believe that magic is an applied pseudo-science, dealing with basically incorrect cause-and-effect relationships that are a consequence of inadequate knowledge. Second, magic is seen as an emotional reaction of individuals who feel that their life is endangered. In this instance, magic is said to develop as a form of control to reassure persons who feel less than omnipotent in the face of natural events. Third, magic is said to flourish in situations in which individuals feel unsure. When persons see the world as unstructured, they attempt to create order through the use of magic. Thus, a lack of knowledge, perceived danger, and feelings of uncertainty are three conditions that have been logically isolated as independent variables in "the theory of magic." The purpose of this paper is to suggest how each of these relates to the utilization of magical beliefs and practices. First a brief discussion of the ideas touched upon above will be presented.

Tylor and many writers after him have been of the opinion that magic is nothing more than a fallacious philosophy.[2] It was maintained by Frazer and his students that magic is merely bad science, that people utilize magic because of ignorance or error. Frazer argued that magic is the application (or misapplication) of the principle of association of ideas and the transposition of this into a theory of natural processes. The two principles that underlie and guide most magical beliefs and practices are (1) that like produces like and (2) that things once in contact continue to act on each other at a distance. It is the *post hoc, ergo propter hoc* fallacy, Frazer argued, that lies beneath a substantial portion of magical behavior.[3]

On the other hand, Malinowski hypothesized a relationship between magic and the dangerous and uncontrollable.[4] The development of magic occurs, he maintained, from the realization by man that the powers of his body and his mind are limited.[5] He wrote:

> The function of magic is to ritualize man's optimism, to enhance his faith in the victory of

[1] This paper is based on an analysis reported in the author's "Arationality in Human Behavior: A Study of Health Practices" (unpublished doctoral dissertation, Yale University, 1961).

[2] E. B. Tylor, *Primitive Culture* (2 vols., 6th ed.; New York: G. P. Putnam's Sons, 1920), I, 135.

[3] Sir James Frazer, *The Golden Bough: A Study in Magic and Religion* (1-vol. ed., abridged; New York: Macmillan Co., 1958), pp. 12–14.

[4] Bronislaw Malinowski, *Magic, Science, and Religion* (Garden City, N.Y.: Doubleday & Co., 1955), chap. v (also see *Coral Gardens and Their Magic* [London: George Allen & Unwin Ltd., 1935], I, 444).

[5] *The Foundations of Faith and Morals* (Riddell Memorial Lectures [London: Oxford University Press, 1936]), p. 5.

hope over fear. Magic expresses the greater value for man of confidence over doubt, of steadfastness over vacillation, of optimism over pessimism.[6]

Talcott Parsons, following this lead, holds that magic is one mechanism for coping with strains that arise in social systems.[7] In fact, many scholars have commented on the relationship between danger or anxiety and activity characterized by supernatural means or ends. For example, Parsons states, in a more recent publication, that the focal point for the development of religious patterns are unforeseen events or events of strong emotional investment that are nonetheless uncontrollable or non-understandable.[8] Ruth Benedict also noted that a strong development of occultism may reflect extreme psychological insecurity.[9] Robin Williams makes this same point when he states: "Interest in religion is a drastically changing variable, linked with such factors as social stress."[10]

Finally, somewhat akin to the first association he observed in his classic study of the Trobrianders, Malinowski remarked on the relationship between uncertainty and magic. He observed that magic was practiced before the men set out on deep-sea fishing trips, but was not necessarily associated with shallow-water lagoon fishing. In the former there is little certainty of success of either a plentiful catch or a safe return. In the latter, both of these events, particularly a safe return, are almost assured.[11] In *The American Soldier*, Brewster Smith commented on the relationship between prayer, magic, and fatalism, and unpredictable circumstances.[12] Parsons argued that the strains which give rise to magic are rooted in uncertainty factors or especially acute adjustment problems.[13] Evon Vogt also maintains that ritual and magical divination are responses to emotional anxiety and cognitive frustration in a situation of uncertainty.[14] In all of these formulations, the connection between uncertainty and magic appears to be indirect. The reasoning seems to be that uncertainty leads to anxiety, which in turn leads to the use of magical practices.

Thus the relationship between a lack of knowledge, perceived danger, and the perception of a situation as being uncertain have been asserted as conditions that lead to the use of magical behavior.[15] That is, they have each been delineated as independent or explanatory variables in "the theory of magic." The question that then arises is: How does each fit into a theoretical model? The investigation reported in this paper suggests that each relates differently to the separate subcategories of magical behavior; and that the seemingly contradictory postulations are the consequence of not delineating these subcategories in the conceptualization of magical behavior.[16]

[6] *Magic, Science, and Religion*, p. 90.

[7] *The Social System* (Glencoe, Ill.: Free Press, 1951), pp. 303–4.

[8] "Religious Perspectives in Sociology and Social Psychology," in W. Lessa and E. Vogt (eds.), *Reader in Comparative Religion* (Evanston, Ill.: Row, Peterson & Co., 1958), pp. 118–24.

[9] "Magic," in E. Seligman (ed.), *Encyclopaedia of the Social Sciences* (New York: Macmillan Co., 1937), p. 43.

[10] Robin M. Williams, *American Society: A Sociological Interpretation* (New York: Alfred A. Knopf, Inc., 1957), p. 326. For an additional analysis of the relationship between danger and magic see Clyde Kluckhohn, *Navaho Witchcraft* (Papers of the Peabody Museum, Vol. XXII, No. 2 [Cambridge, Mass. 1944]).

[11] Malinowski, *Magic, Science, and Religion*, chap. v.

[12] Samuel A. Stouffer *et al.*, *The American Soldier: Combat and Its Aftermath* (Princeton, N.J.: Princeton University Press, 1949), II, 188.

[13] *The Social System*, pp. 303–4.

[14] "Water Witching: An Interpretation of a Ritual Pattern in a Rural American Community," in Lessa and Vogt (eds.), *op. cit.*, pp. 327–41.

[15] It should be noted that we are not attempting to explain the origin of magical beliefs and practices but are only interested in understanding the situations in which they are utilized. However, implicit in the distinction between remissive and operative magico-religion made in this paper is that there are differences in the origin and cultural legitimacy of arational acts.

[16] Although we are primarily concerned with the place of these three independent variables in "the theory of magic," multivariate analysis with standard sociological background variables was carried out to see if factors could be uncovered that would interpret or explain the relationships. For the most

THE SAMPLE

One hundred and four lower class females,[17] mothers of children who in 1959 had been ill with one of four illnesses (poliomyelitis, measles, viral pneumonia, or tuberculosis), were interviewed. The respondents were selected from records found in the office of the Commissioner of Health in New Haven, Connecticut, or in the Winchester Tuberculosis Clinic at the Grace-New Haven Community Hospital. The twenty-six respondents with a child who had poliomyelitis represented the total number of mothers that could be reached for an interview in the summer of 1960. The respondents for the other three illness groups were matched by ecological area with this group. The interview, which lasted approximately one hour, was conducted in the respondent's home.

MAGICAL BEHAVIOR

The type of magical behavior on which this study focuses is single-ended action that, from a scientific point of view, employs means totally inappropriate for an intended end. This type of action, hereafter referred to as arational behavior, falls into two major categories. The first is behavior resulting from faulty knowledge not obtained from scientific sources; this will be called non-rational behavior. The second category of arational behavior, magico-religious, includes action in which the means are characterized by a non-empirical element. The means of magico-religious behavior are not technically relevant for the end to which they are directed, in that those in a position to judge these means (see below) would find them outside the range of behavior prescribed by the tradition of the area in which the actor is operating.

Magico-religious behavior is further divided into remissive magico-religion and operative magico-religion. The former includes use of and/or belief in the efficacy of formally accepted religious practices, while the latter includes activities that, although they resemble the former, are theologically extraneous in the Judeo-Christian tradition. Operative magico-religion differs further from remissive magico-religion in that the individual may not only be appealing for help but appears to be actively trying to do something to bring about an event. Arational beliefs and practices taken together are called "arationality."

A measure of arationality was obtained by asking each respondent how she treated the child who had been ill the previous year. Both open-ended questions and check lists were designed so that all facets of treatment would be considered.[18] In the quantification of arationality, each reported practice was given a value of *1*, so that the total number for each or all respondents was additive.

Examples of the type of practices assigned to the subcategories of arationality follow:

1. *Remissive magico-religion.*—Reading from a Bible, praying, going to church, using a healing shrine, religious self-castigation, and using sacred relics or medals.

2. *Operative magico-religion.*—The use of non-religious charms and trinkets, the changing of residence, and all types of "magical" or "superstitious" acts. The behavior in this subcategory ranged from the tying of a family heirloom around a child's neck to the practice of astrology.

3. *Non-rational behavior.*—The use of ineffective or harmful foods, herbs, patent medicines, alcohol, raw foods, and various compounds such as milk and honey, vaseline and sugar, turpentine and sugar, kerosene

part, this analysis, possibly because of the limited sample, revealed few significant results, Social class position was inversely related to the utilization of non-rational behavior and operative magico-religion. However, other variables such as education, religious affiliation, and racial identification were not associated with any of the three subcategories of arational behavior.

[17] Using Hollingshead's Two-Factor Index of Social Position to stratify the sample, the class distribution was Class III, 14.4 per cent; Class IV, 32.7 per cent; Class V, 52.9 per cent (see A. B. Hollingshead, *Two Factor Index of Social Position* [New Haven, Conn.: Privately printed, 1957]). Further, 61 per cent of the sample were non-white, only two-thirds were living with their spouse at the time of the interview, and 63 per cent had not completed high school.

[18] See Lewis, *op. cit.*, Appendixes A and C.

and sugar, onion syrup (onions and sugar), and rabbit tobacco (white plantain).

The rationality or arationality of behavior was established by utilizing present-day medical knowledge as represented by three physicians, pediatricians on the faculty of the Yale Medical School. The medical practices that the respondents reported they used on their child or children were put on lists and given to each expert for independent judgment. The three physicians were in agreement on the rationality or arationality of the items 90 per cent of the time. For the remaining items, the agreement of two out of three judges was accepted.

ANALYSIS AND FINDINGS

A three-way analysis of variance was carried out in order to examine the simultaneous effects of the three independent variables in "the theory of magic" on the three subcategories of arationality. The measures of the three variables were as follows: (1) for danger, whether or not the respondent viewed the illness which her child had as dangerous; (2) for knowledge, the respondent's score on a twelve-item medical knowledge list;[19] (3) for certainty, the length of time the child was ill. The rationale behind the use of this latter item was that it would be expected that the treatment process for an illness of a short duration would be more structured and would appear more manageable than one for an illness of a longer or unspecified duration. A rationality/arationality percentage was computed for each of the twelve combinations in the three tables.[20] These percentages were then converted into angles by arc sine transformations. The standard statistical procedure for analysis of variance was employed except in the computation of the error term. The error term used as the denominator in computing the F-ratio was $1/N$.[21] The F-ratio for the independent variables and their interactions for each of the three types of arationality are presented in Table 1. Though this analysis has not definitively sorted out the amount of variance explained by each factor or combination of factors because of the significant second order interactions, we can still tentatively conclude from the F-ratio values that:

1. Certainty-non-certainty is the variable most affecting the use of remissive magico-religion. Its influence is confounded, however, by the other two variables, particularly knowledge.
2. Certainty-non-certainty is the variable most affecting the use of non-rational behavior. Its influence is confounded by the other two variables.
3. Level of knowledge is the variable most affecting the use of operative magico-religion. This factor is not confounded by the other two variables.

The most notable findings are what appear to be the determinate relationships between certainty-non-certainty and remissive magico-religion, and level of knowledge and operative magico-religion. That is, supplicative practices are utilized when persons see a situation as unstructured and perhaps disquieting; and manipulative practices are used when persons would like to solve a problem but lack the knowledge of how this might be done. This finding is in no way surprising. This combination of uncertainty and arational behavior is the phenomenon Malinowski and those of his tradition have called magic.[22] Moreover, the combination of a lack of knowledge and arational behavior is precisely what Frazer called magic.[23] It would appear that these men were not observing the same behavior and attributing

[19] Knowledge was measured by administering a twelve-item list of true-false statements to each respondent. The item concerned possible types of treatment for the illness with which the respondent had experience. The lists were checked for validity by the three medical judges (see *ibid.*, Appendix B).

[20] Before the independent variables were run against rationality-arationality, danger and certainty were dichotomized and knowledge was trichotomized. This left twelve columns in each table for which percentages could be computed.

[21] The author is indebted to Professor Robert P. Abelson, of the Department of Psychology, Yale University, who suggested this method for the computation of the analysis of variance.

[22] *Magic, Science, and Religion*, chap. v.

[23] *Op. cit.*, chap. iii.

different causal factors to it. The behavior patterns that each observed and called magic, though superficially the same insofar as the absence of logical means-ends connection, were in reality different.

When persons utilize remissive magico-religious practices that are in reality religious practices, they are acting under conditions perceived as unstructured. That is, they are unable to see clearly the situation to which their means will be applied. It would not really be expected, then, that they would utilize means characterized by empirical elements in an event that they do not see as concrete. Nor would it be expected then that their magico-religious behavior would be more than supplicative. They combine the religious practices they use with non-rational practices that are technically relevant for the treatment of illness but also arational. Though adequate data are lacking, there is some evidence to conclude that the non-rational practices are first used, and that remissive magico-religion is employed as a final method by persons who feel they have "nothing to lose" by its utilization. If persons do not see a situation as structured, it would not be expected that they would have enough information to select rational means to cope with it.

On the other hand, persons who have inadequate knowledge about a situation, though they have no more basis for making a decision in the selection of means than those who feel uncertain, may not see themselves as having inadequate knowledge and consequently try to actively change the situation. Such persons, therefore, employ operative magico-religion. It would seem that their insufficient knowledge coupled with their attempt to manipulate their environment leads to the use of behavior that, although characterized by non-empirical elements, shows less resignation to natural events than remissive magico-religion.

TABLE 1

RESULTS OF ANALYSIS OF VARIANCE

	Degrees of Freedom	Remissive Magico-religion*		Non-rational Behavior†		Operative Magico-religion	
		F-Ratio	Significance	F-Ratio	Significance	F-Ratio	Significance
Danger	1	<1	$p > .05$	<1	$p > .05$	3.09	$p > .05$
Certainty	1	25.41	$p < .05$	10.20	$p < .05$	2.97	$p > .05$
Knowledge	2	3.73	$p < .05$	2.10	$p > .05$	13.72	$p < .05$
Danger×certainty	1	6.39	$p < .05$	7.08	$p < .05$	<1	$p > .05$
Danger×knowledge	2	<1	$p > .05$	2.78	$p > .05$	1.69	$p > .05$
Certainty×knowledge	2	4.15	$p < .05$	1.28	$p > .05$	<1	$p > .05$
Danger×certainty×knowledge	2	2.44	$p > .05$	3.39	$p < .05$	1.70	$p > .05$

* Since two first-order interactions are significant, testing the main effects of these same variables against the error term (estimated residual) is not statistically sound. However, each test of significance is reported so that the power of each F-ratio can be shown.

† Since the second-order interaction is significant, testing the first-order interactions and main effects of these same variables against the error term (estimated residual) is not statistically sound. See n. *.

DANGER AS AN EXPLANATORY VARIABLE

In Table 1 the variable of danger by itself does not explain any of the variance. Though the effects of both knowledge and certainty are not distinct from themselves and from the variable of danger as evidenced by the interaction effects, in two out of three cases each accounts for a significant amount of variance. In a separate multivariate analysis, taking two independent variables at a time while holding a third constant, this same pattern is seen. In the latter analysis, in three out of three cases, only under the condition of low knowledge, does danger sig-

nificantly account for the variance in the utilization of each of the three measures of arationality. It seems, then, that it is the combination of the perception of a situation as dangerous and a lack of knowledge that is critical in motivating persons to act arationally. Only when one has low knowledge does the perception of a situation as dangerous or not very dangerous explain the use of arational behavior. The status of danger in "the theory of magic" could be best described as a precondition rather than as an explanatory variable.

CONCLUSION

The analysis completed in this study would indicate that a reformulation of "the theory of magic" is needed. The most obvious problem centers on the use of the generic term "magic" to refer to a category of behavior in which means are inappropriate for a desired end. If only the view of an omnipresent observer, and not also that of the actor, is considered by the researcher, then the establishment of the rationality or arationality of behavior would not require the development of a more finite classification scheme.[24] However, such an approach, as Max Weber clearly pointed out, misses the essence of all that is social in human behavior. How would we know, for example, if an actor's selection of what an omnipresent observer believed to be rational means was accidental?

In fact, if Weber's caution that the point of view of the actor must always be considered had been heeded, then social scientists would have long ago clearly seen the impasse to which their reasoning led, and would have discarded the general concept "magic." A more refined classification would have served to advance the development of "the theory of magic."

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[24] Although the rationality or arationality of behavior in this study was determined primarily by omnipresent observers (medical judges), the orientation of the individual was considered in the basic decision of submitting a practice for judgment (see Lewis, *op. cit.*, chap. ii).

AFFILIATIONS: STRUCTURAL DETERMINANTS OF DIFFERENTIAL DIVORCE RATES[1]

CHARLES ACKERMAN

ABSTRACT

This paper argues that a structural determinant of the incidence of divorce is the pattern of the spouses' affiliations. Research into the rate of divorce in sixty-two primitive societies indicates that, when the spouses maintain predominantly common affiliations, the incidence of divorce is low; that, when the spouses maintain predominantly separate affiliations, the incidence of divorce is high.

Differential divorce rates have been the object of both anthropological and sociological research. Separate complexes of hypotheses have been formulated in the two disciplines: anthropological research has emphasized various aspects of kinship structure, and sociological research has been largely concerned with homogamy and assortative mating. The purpose of this paper is to provide a framework for interdisciplinary research. Selected structural correlatives of high and low divorce rates in sixty-two primitive societies are presented along with explanatory concepts and hypotheses. The paper is concerned only with such correlatives and not at all with whatever psychodynamics may be operative.

The conceptual framework of anthropological research was fixed and phrased succinctly by Gluckman. In a comparative analysis of Zulu and Lozi kinship systems he writes: "I suggest that the divorce rate is a reflex of the kinship structure itself."[2] The aspects of kinship structure that he cites as most important are the nature of the descent system and the locus of jural authority over what he calls "the woman's procreative power." When such authority is located within the agnatic lineage of the husband, as in patrilineal societies, "divorce is rare." In matrilineal societies "the woman produces for her own matrilineal line [and] divorce is frequent." In bilateral societies "divorce is frequent, for the woman produces for her own line as well as for her husband's."[3]

Several modifications of Gluckman's hypotheses have been suggested. All retain his emphasis upon the kinship structure. Fallers finds "common corporate group memberships" associated with marital stability,[4] and Mitchell suggests that it will make a difference in the divorce rate whether the woman's procreative power is held by a corporate group or by an individual.[5] Fortes suggests that "divorce is correlated with the degree to which a person has jural status that is independent of his or her status as spouse."[6] Neither Gluckman's original hypotheses nor the subsequent suggested modifications have been tested cross-culturally.

Sociological research has been mostly concerned with the twin concepts, "homogamy" and "assortative mating." The pioneer

[1] I am indebted to Florence Kluckhohn, Evon Vogt, and John Whiting for their criticisms of an earlier version of this paper. I want especially to thank George C. Homans, who argued with me throughout the research.

[2] Max Gluckman, "Kinship and Marriage among the Lozi of Northern Rhodesia and the Zulu of Natal," in A. R. Radcliffe-Brown and Daryll Forde (eds.), *African Systems of Kinship and Marriage* (London: Oxford University Press, 1950), p. 166.

[3] Gluckman cites also types and quantities of marriage payment, the sex ratio, and relative marriage ages as examples of other factors that, he believes, may be associated with the incidence of divorce.

[4] Lloyd Fallers, "Some Determinants of Marriage Stability in Busoga: A Reformulation of Gluckman's Hypotheses," *Africa*, XXVII, No. 2 (1957), 108.

[5] J. C. Mitchell, "Social Change and the Stability of African Marriage in Northern Rhodesia," in Aidan Southall (ed.), *Social Change in Modern Africa* (London: Oxford University Press, 1961), pp. 316–29.

[6] Meyer Fortes, "Descent, Filiation and Affinity: A Rejoinder to Dr. Leach," *Man*, LIX (1959), 206.

study of Burgess and Cottrell[7] and the research of Goode[8] and of Zimmerman[9] have established the crucial importance of (*a*) homogeneity in the background and interests of the spouses and (*b*) homogeneity in the network of affective affiliations surrounding the marriage. The more alike the spouses are in such *antecedent* factors as socioeconomic status, ethnicity, region of origin, education, and religion, the less the probability that the marriage will result in divorce. Further, as Zimmerman establishes, the more traits ego-families have in common with close friend-families, the less the incidence of divorce among ego-families: *subsequent* homogeneity in affective affiliations is important.

Zimmerman's findings in particular have important structural implications. The "family-friend groupings" whose homogeneity he analyzes are collectivities, albeit informal ones, and as collectivities they possess, on a minor scale, norm and value sets analogous to the norm and value sets of formal collectivities. If we can grant that the behavior and expectations of members of collectivities are affected by the norms and values of the collectivities, it is clear that when the affiliations of both spouses are *conjunctive*, that is, overlapping or identical, the behavior and expectations of both spouses are affected by the same norm and value sets. The affiliations of the spouses can, on the other hand, be *disjunctive;* that is, each spouse may maintain membership in different collectivities. In such a situation, the behavior and expectations of the spouses are affected by different norm and value sets.

Zimmerman's "family-friend groupings" are, by definition, conjunctive affiliations. He cites the effect on marital stability of greater and lesser homogeneity within such affiliations, and he finds, as has been noted, that homogeneity within the group is associated with a low incidence of divorce, increasing heterogeneity with an increasing incidence of divorce. Can Zimmerman's findings be interpreted as having indicated an association of low divorce rates with norm and value *consensus*, higher divorce rates with an increasing lack of such consensus? If this be the case, we may wonder, what may be the effect on the stability of marriage of disjunctive affiliations? In such a situation it seems reasonable to expect a high incidence of divorce. The more disjunctive the affiliations of the spouses, and the more disparate the norm and value sets of their affiliations, the higher the probability of divorce.

PATTERNS OF AFFILIATIONS

We introduce, then, two structural concepts into the analysis of differential divorce rates. We hypothesize that the *pattern* of the network of affiliations surrounding the marriage is a structural determinant of the probability of divorce. Specifically, we expect the divorce rate to be low when marriages are imbedded in a network of conjunctive affiliations, high when the spouses maintain disjunctive affiliations. From this point of view, assortative mating and homogamy are seen as conditions that make it likely that the spouses will establish conjunctive affiliations, and the association of these antecedent factors with a low divorce rate is seen as dependent upon the establishment of conjunctive affiliations.

In order to test these hypotheses cross-culturally, indexes of "conjunctive" and "disjunctive" affiliations must be selected. The whole anthropological tradition warns us that differences among kinship structures must be taken into account. The question is: *At what level* must they be taken into account? Is there nothing that can be said generally? This research was guided by the assumption that general statements can be formulated. We shall see to what extent that assumption was justified.

[7] E. W. Burgess and L. S. Cottrell, *Predicting Success or Failure in Marriage* (New York: Prentice-Hall, Inc., 1939).

[8] W. J. Goode, *After Divorce* (Glencoe, Ill.: Free Press, 1956).

[9] Carle Zimmerman, "The Present Crisis," in Carle Zimmerman and Fr. Cervantes, S.J., *Marriage and the Family* (Chicago: Henry Regnery Co., 1956); see esp. p. 111.

SAMPLE AND STRATEGY

The sample societies were drawn from the more than 150 societies documented in the Human Relations Area Files (HRAF) at Harvard University in May, 1962. Each of the societies described in the HRAF was examined for an assessment of the incidence of divorce (HRAF Index No. 586). Only those societies whose divorce rate is unambiguously assessed by the ethnographers are included in the sample. It was required also that each society in the sample be included in Murdock's "World Ethnographic Sample,"[10] which categorizes societies with respect to several structural aspects relevant to the hypotheses.

Sixty societies in the HRAF meet these two requirements. In addition to these sixty, two other societies are included in the sample, the Zulu and the Lozi; it was comparative analysis of these that suggested to Gluckman his hypotheses. The total sample includes thirty-two bilateral, twenty-one patrilineal, six matrilineal, and three double-unilineal societies.

The author must admit that the data used in the research have many faults. For the most part, ethnographers have stated only that divorce is "low," "common," "infrequent," etc. Rarely has any ethnographer justified his assessment of the rate by any statement of the actual incidence of divorce. Barnes points out the peculiar difficulties of assessing divorce rates among primitive peoples.[11]

It is also true that the comparability of the assessments is somewhat doubtful. Since the ethnographers have not necessarily had reference to a common baseline, it is impossible to assert with confidence that one society's "low" is exactly equivalent to another society's "low" rate.

There is no way of dealing with these faults. The researcher can only use the data as given—or stop his research. This paper is built on the hope that the ethnographic data are neither wholly meaningless nor wholly incomparable.[12] One wishes that an adequate number of primitive societies could be redescribed and that the ethnographers would be guided by identical protocols.

Early in the research, the author decided he would not himself make any interpretive *decisions* regarding the relative importance in a society of conjunctive and disjunctive affiliations. The reason for this decision is obvious: Any researcher may selectively perceive among data only those supporting his hypothesis. It was decided to categorize whole societies as either emphasizing conjunctive or disjunctive affiliations; such an either/or categorization obscures, of course, the fact that there are probably always both. Many deviant cases were expected, because of the grossness of the indexes used; fortunately there are few; the selected indexes seem to be quite discriminating. With regard to each deviant case, the author will *argue* for a predominance of either conjunctive or disjunctive affiliations; such arguments are based on his personal assessment of the data and may be biased.

THE BILATERAL SOCIETIES

What structural aspect may be specified as an index of conjunctive affiliations in a bilaterally organized primitive society? *Endogamy* seems a likely index. Whereas community exogamy unites spouses from different communities, community endogamy unites spouses who have a common community background. It seems fairly apparent that the probability of conjunctive affiliations is greater in the latter case. Further, whereas consanguine exogamy unites spouses whose kin affiliations must be different, con-

[10] George Peter Murdock, "World Ethnographic Sample," *American Anthropologist*, LIX (August, 1957), 664-87.

[11] J. A. Barnes, "Measures of Divorce Frequency in Simple Societies," *Journal of the Royal Anthropological Institute*, LXXIX (1949), 37.

[12] Many of the societies in the present sample are included by George Peter Murdock in the sample used by him in "Family Stability in Non-European Cultures," *Annals of the American Academy of Political and Social Science*, CCLXXII (1950), 195-201. Although this does not, of course, establish the reliability of the ethnographic data, it indicates that I am not alone in my decision to accept them as meaningful and comparable.

sanguine endogamy unites spouses with overlapping kin affiliations. Three hypotheses are indicated:

1. Low divorce rates are associated with community endogamy, and high divorce rates are associated with community exogamy.
2. Low divorce rates are associated with consanguine endogamy, and high divorce rates are associated with consanguine exogamy.
3. The expected associations should be *strengthened* when, on the one hand, community and consanguine endogamy are combined and, on the other, community and consanguine exogamy are combined.

TABLE 1*

	Assessed Divorce Rate	
	Low	High
Demes	Inca Thai Pawnee Burmese Siriono Kaingang Macassarese	Cuna Comanche Tarahumara
Agamous	Iban Pomo Aztecs Andamanese Tubatulabal	Ifugao Samoans Chukchi Marquesans Nambicuara
Exogamous	Nyakyusa	Lozi Yurok Semang Papago Nootka Amhara Alorese

* Degree of community endogamy "unknown": Abipon (high); Callinago, Cayapa, and Koryak (low).

In Table 1 the bilateral societies are distributed according to assessed divorce rate and degree of community endogamy.[13] The association predicted by the first hypothesis is found: twenty-four of the societies conform with expectation and there are four exceptions. Endogamous communities have a low divorce rate and exogamous communities have a high divorce rate. The coefficient of association of these dichotomous extremes is $Q = .94$, with $p = .024$ by Fisher's Exact Test. The agamous communities, which reveal no tendency toward either endogamy or exogamy, reveal no tendency toward either low or high divorce rates; they divide in half.

Among bilateral societies, norms of consanguine endogamy and exogamy involve permitting or prohibiting marriage with cousins of varying degrees of consanguine relationship. Murdock uses twelve classifications of such norms that for our purposes can be collapsed into three: (1) marriage permitted with a first-degree cousin; (2) marriage prohibited with a first-degree cousin but permitted with a second-degree cousin; and (3) marriage prohibited with any cousin within two degrees of consanguinity.[14] We expect (from hypothesis 2 above) that societies that permit marriage with a first-degree cousin will have low divorce rates and that societies that forbid marriage with any second-degree cousin will have high divorce rates. The expected association is found. It is, however, a weaker association than that found in Table 1: $Q = .72$ and $p = .067$ by Fisher's Exact Test. Whether this is a *significantly* weaker association is problematic. It should be noted also that the probability fails to achieve an .05 rejection level. There are, nevertheless, fewer than seven chances in one hundred that the association results from chance.

The indexes, community and consanguine endogamy, may be combined. Thus, we examine the divorce rates in those societies that, by combining community endogamy with consanguine endogamy, achieve a maximum of conjunctive affiliations and in those which, by combining community exogamy with forbidding second-degree cousin marriage, achieve a maximum of disjunctive

[13] The cited categories are defined in Murdock's "World Ethnographic Sample," *op. cit.*, p. 669. "Demes" are "communities which reveal a marked tendency toward local endogamy"; "agamous" communities are those "without any marked tendency toward local exogamy or local endogamy"; "exogamous" communities are those which reveal "a marked tendency toward local exogamy"; and *unknown* are communities for which relevant data are lacking.

[14] Murdock, *ibid.*, p. 672.

affiliations. Unfortunately, only eleven of our societies combine these various criteria. Table 2 shows the distribution of these societies with respect to the divorce rate. The expected association is found and, as expected, it is stronger than the previous associations: $Q = 1.0$ and $p = .013$ by Fisher's Exact Test. Of course, a sample composed of only eleven societies constitutes a very tiny sample indeed; alone, these results would be of little interest. Taken in the context of the research, however, the results are meaningful: as the network of conjunctive affiliations "tightens" around the spouses, marital stability increases.

A DEVIANT CASE

A single tribe resists firmly all expectations—the Nyakyusa, a Bantu-speaking people of northern Nyasaland. Clearly, they are a problem case. Their communities are exogamous, and they forbid marriage with any second-degree cousin: *Why* is their divorce rate low?

The Nyakyusa do not in fact constitute an exception to any of the three hypotheses. (1) Although Nyakyusa communities are largely exogamous, they could hardly be otherwise, as they are composed initially of *male age mates*. Each generation of male adolescents founds a new village near that of their parents. From where do these male age mates bring their wives? From that parental village. Spouses are, therefore, from the same natal community and have been raised together into adolescence. (2) It is obvious from the ethnographer's account[15] that the Nyakyusa forbid marriage with any second-degree cousin. What is a preferred marriage among the Nyakyusa? As one of the informants puts it, "I may take one of my brothers by a different woman to my mother's brother's place and say to my brother, 'There is the girl for you.'" The Nyakyusa practice polygyny; brothers may have different mothers; and Ego's *brother* marries Ego's first-degree cousin—surely a conjunctive affiliation. (3) The Nyakyusa combine the conjunctive affiliations of community and consanguinity, as has been argued.

THE LINEAL SOCIETIES

What structural aspect may be specified as an index of conjunctive affiliations in lineally organized primitive societies? Initially, the author assumed that in these societies, as in the bilateral ones, endogamy would constitute an adequate index. He was mistaken. The nature of the descent system must be taken into account, Gluckman warns us; and the warning is justified. No association is found between the assessed divorce rate and any kind or combination of kinds of endogamy in lineal societies.

TABLE 2

	Assessed Divorce Rate	
	Low	High
Demes Permitting Marriage with a First-Degree Cousin	Thai Inca Siriono Burmese Kaingang	
Exogamous Communities Forbidding Marriage with a Second-Degree Cousin	Nyakyusa	Lozi Yurok Papago Nootka Amhara

Nevertheless, conjunctive and disjunctive affiliations remain the structural determinants of the divorce rates. It is the indexes chosen, not the concepts, that are no longer adequate. Within the lineally organized primitive community the various lineages themselves are bases for corporate disjunctive affiliations. Within the bilaterally organized community no such bases exist; hence, the endogamous lineal community is not structurally analogous to the endogamous bilateral community. We must take into account the lineal principle, which introduces a new criterion of affiliations.

How may the conjunctive affiliations found within the endogamous bilateral community be achieved within a lineal com-

[15] Monica Wilson, "Nyakyusa Kinship," in A. R. Radcliffe-Brown and Daryll Forde (eds.), *op. cit.*, pp. 111-39.

munity? Two mechanisms may be postulated:

1. The affiliations of each spouse may be extended so as to include those of the other.
2. One spouse may be severed from prior affiliations, incorporated into those of the other.

It is in terms of either *extension-inclusion* mechanisms or *severance-incorporation* mechanisms that a hypothesis can be specified to lineally organized societies.

TABLE 3*

	Assessed Divorce Rate	
	Low	High
Levirate Present....	Zulu Bhil Lolo Gond Kazak Azande† Yoruba Lepcha Afghan	
Levirate Absent......	*Vedda* *Mbundu* Atayal Kikuyu Tikopians Vietnamese	Lau *Ila* *Yao* Fang *Toda* *Zuni* Omaha Hausa *Khasi* *Bemba* Mandan‡ Azande† Ojibwa§ Somali Siwans Mundurucu

* Matrilineal and double-unilineal societies are italicized.

† The Azande no longer practice the levirate; divorce rate is high. When they did, divorce rate was low.

‡ Among the Mandan the levirate is permissive and rarely observed.

§ There is no assessment of incidence of divorce when the Ojibwa practiced the levirate. They no longer do so, and their divorce rate is high.

Perhaps the most immediately obvious index of one of these mechanisms is the "levirate." This institution so completely severs the wife from her prior affiliations and incorporates her into her husband's affiliations that even upon her husband's death she is not released: she is taken as wife by [illegible] dead husband's brothers or some other lineage mate.[16] The levirate is a mechanism of radical severance-incorporation, producing conjunctive affiliations.

Certainly, if our general formulation be a valid one, the existence of the levirate should be a sufficient condition for a low divorce rate. However, a low divorce rate may also be characteristic of societies not practicing the levirate, as it is only a single index of one of the two postulated mechanisms: conjunctive affiliations may be achieved by other means. We expect, therefore, that societies with the institution of the levirate will be societies with a low divorce rate; that some societies will be characterized by low divorce rates without the levirate; and that in these latter societies some other mechanism, either of severance-incorporation or of extension-inclusion, exists. Societies with high divorce rates will be societies lacking such mechanisms and will reveal disjunctive affiliations.

In Table 3 the lineally organized primitive societies are distributed with respect to the presence/absence of the levirate[17] and the assessed divorce rate. The levirate is a sufficient condition for low divorce rates: the coefficient of the expected association is $Q = 1.0$ and $p = .0002$ by Fisher's Exact Test. Within the six low-divorce-rate societies lacking the levirate, we shall expect to find evidence of either some other severance-incorporation mechanism or some extension-inclusion mechanism.

Although an argument can be made for the existence of the cited mechanisms within at least four of the six low-divorce-rate societies lacking the levirate, it should be noted that the author is interpreting the ethnographic data. Interpretation is a

[16] The nature of the evidence available to us and the small size of our sample do not permit us to discriminate among specific types of leviratic institutions.

[17] The levirate is so salient an aspect of marriage customs that I doubt that any ethnographer describing those customs would ignore it if it were present. I have, therefore, assumed the levirate to be "absent" if it is unmentioned. It is generally dangerous to assume that because something is not mentioned in an ethnography it is not present in the society.

chancy thing. It is fortunate for the nonproblematic corroboration of our general hypothesis that the mechanism of the levirate is so widespread and so visible to the ethnographers.

The Kikuyu and the Vietnamese present relatively unambiguous evidence of severance-incorporation. (1) Kikuyu girls possess age-set affiliations and lineage affiliations; both can constitute affiliations disjunctive to those of her husband. Kikuyu symbolize nicely the rupture of both these affiliations. At the end of a period of premarital seclusion the bride is led "as though blind" to visit her parents *by a girl of her husband's family*, thus demonstrating that her personal affiliations are mediated by her husband's. As a part of the marriage ceremony, a mourning song is sung signifying the breaking-away of the girl from her age set. (2) Among the Vietnamese, the wife's presence is necessary for the rituals of family worship and in them she occupies the same rank as her husband. More appositely, upon the death of the husband, the widow assumes the ritual and judicial roles of the husband, remains with his people and has the right to name his successor to cult functions.

Mechanisms of extension-inclusion exist among the Tikopians and the Vedda. (1) Co-operation of the Tikopian husband with the wife's family is a matter of course and "will go on all his life." Co-operation is not restricted to economic affairs: the wife accompanies her husband in the performance of his clan rituals and "if it be of the wife's group the husband takes part in fulfillment of his specific duty." (2) Among the Vedda bilateral cross-cousin marriage is preferred, and, consequently, the affiliations of the spouses are largely coterminous at marriage.

The data are more ambiguous concerning the Atayal and the Mbundu. Although the Mbundu practice bilateral cross-cousin marriage and the proper marriage is one "within the circle of kin," the ethnographer also states that marriage "profoundly alters the relationship of those who enter into it, that is, the two families." The nature of this alteration is not clear. We are given little clear data about Atayal marriage, although "the men of the two families form a real alliance" and divorce entails "a rupture between the two families."

What of the societies in Table 3 with high divorce rates? Excepting the Ojibwa, they are all societies in which postmarital disjunctive affiliations are obvious. Indeed, in many patrilineal cases the ethnographers have commented specifically upon the disruptive influence on the marriage of the wife's continued affiliations with her lineage. In matrilineal societies marital instability is widespread, and Richards' comment concerning the Bemba seems generally applicable: "there are several sources of tension in this type of kinship system . . . the ties of loyalty which bind a man to his matrilineal descent group clash to a certain extent with the obligations he owes to his local group, his wife's extended family."[18]

The Ojibwa marriage is anomic in the classic sense. Among the Ojibwa there is little evidence of *any* affiliations; their marriages seem wholly isolated and exposed, with few ties of any sort to the wider community. This is a structural possibility that was not included in the formulation of the hypotheses.

DISCUSSION

It has been admitted already, but it bears repeating, that the data by which these societies have been categorized as ones of "high" or "low" divorce rates are in several respects unsatisfactory. Although interesting and clear-cut associations have been established within the sample primitive societies, these associations are no firmer than the data upon which they are based. For this reason, the author has defined the purpose of this paper as exploratory, that is, it is intended to provide a *framework* for the further research that may be stimulated.

What light do these findings throw on the Gluckman hypotheses and on the various suggested modifications of those hypotheses? It can be said that bilateral societies are *not* characterized by high divorce rates gen-

[18] A. I. Richards, *Chisungu: A Girl's Initiation Ceremony among the Bemba of Northern Rhodesia* (London: Faber & Faber, 1956), p. 41.

erally. Further, patrilineal societies are not characterized by low divorce rates generally. Reference to the descent system does not seem to account for the different rates. It is also apparent that reference to "the woman's procreative power" does not account for the variance found. Nevertheless, Gluckman's emphasis on the importance of the descent system has been corroborated by our need to change indexes in midanalysis.

The modification suggested by Fallers is especially productive. Our formulation is quite similar to his. He asserts the desirability of "socially absorbing" the inmarrying spouse, hypothesizing that "common corporate group memberships tend to reinforce the marriage bond, different corporate group memberships to work against it."[19] The present author arrived at his formulation independently, but is wholly in agreement with Fallers so far as lineal societies are concerned. Emphasis upon corporate kin-group affiliations may be somewhat inhibiting, however; only with great difficulty can such a concept be specified to bilateral societies. It seems reasonable to assert that Fallers' common corporate group memberships are a subtype of conjunctive affiliations, which may or may not be corporate and may or may not be kin-based, namely, Zimmerman's family-friend groupings.

We suggest anthropological research into the various forms of conjunctive affiliations, the mechanisms by which they may be achieved, and the relationships between them and disjunctive affiliations. Each society probably has both conjunctive and disjunctive patterns, and the precise forms of these patterns will vary with cultural differences. We may note, as examples, such variant forms of conjunctive affiliations as the Incan *ayllu* and the castes of India. The *ayllu* had deme-like structure, permitted marriage with first-degree cousins, and reinforced these close bonds economically, as land was held and worked communally. The *ayllu* were, of course, characterized by low divorce. The *gotras* ("clans") of India constitute disjunctive affiliations *within* the conjunctive caste, and normally the levirate is absent precisely where divorce is probably least common, among the *gotras* of the highest castes. Here, what are the mechanisms producing low divorce? Is one possibly the very fact of *gotra* exogamy, which in combination with the localization of the *gotras* means that wives are brought from afar, severed from their affiliations by that simplest of all mechanisms—distance? The *ayllu* and the *gotra* are both localized, landholding kin groups characterized by low divorce rates, but the one is bilateral, the other is lineal, and the one is endogamous and the other exogamous. What are the specific structural mechanisms involved?

Finally, what can be said of the sociological relevance of these findings? Obviously, it is the relationship of affiliational patterns to homogamy and assortative mating that is most problematic and most in need of further research. Can homogamy be interpreted as a special case of conjunctive affiliations, appropriate to a highly differentiated society? Or is homogamy an antecedent factor that makes likely the establishment of conjunctive affiliations? The author is inclined to the latter interpretation.

It is worth noting in this regard that Zimmerman's ego-families may themselves be quite homogamous; yet in spite of such homogamy, the incidence of divorce increases with heterogeneity in the surrounding affective affiliations. This would seem to indicate that the important factor is those affiliations, not the homgamy. Worth noting also is the fact that, although there is no reason to assume that homogamy is any less characteristic of lineal endogamous communities than of bilateral endogamous communities, the divorce rates of these two types are different: it is only the bilateral endogamous community that is characterized by low divorce rates.

It may be, however, that both the pattern of the affiliations and the degree of homogamy "carry" variance. Certainly, there is nothing in the findings of this research that could deny this possibility. This is a question that, like the previous ones, can be answered only by further research.

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[19] Fallers, *op. cit.*, p. 121.

MANAGERIAL SUCCESSION AND ORGANIZATIONAL EFFECTIVENESS[1]

OSCAR GRUSKY

ABSTRACT

A negative correlation is found between (1) rates of managerial succession and effectiveness and (2) change in succession rate and change in organizational effectiveness among sixteen professional baseball teams examined over two time periods, 1921–41 and 1951–58.

A set of ten variables from organization theory is applied to the analysis of team performance and administrative succession. A number of illustrative propositions are presented.

The major purpose of this study was to test two related hypotheses: (1) that rates of administrative succession and degree of organizational effectiveness are negatively correlated, and (2) that a change in the rate of administrative succession is negatively correlated with a change in organizational effectiveness.[2] The hypotheses are deliberately stated so as not to attribute causality solely to either succession or effectiveness. We assumed that the variables induce reciprocal effects. High rates of succession should produce declining organizational effectiveness, and low effectiveness should encourage high rates of administrative succession.

To obtain anything resembling an adequate field test of these hypotheses required a substantial number of formal organizations that, ideally, were identical in official goals, size, and authority structure. If the objectives of the organizations were not similar, then obviously it would not be feasible to compare their relative effectiveness, since this concept refers to the extent to which an organization is able to move toward the accomplishment of its official aims. We know that for business organizations and certain public agencies, and perhaps for other kinds as well, rates of succession are positively related to organizational size.[3] Therefore, we sought a sample of organizations of similar size.

There is some evidence, although it is highly limited, that organizations with different types of authority structures respond in very different ways to personnel changes at top levels in the hierarchy.[4] Hence, organizations with similar types of structures of authority were desirable.

In addition, a relatively "clean" field test of the hypotheses demanded reliable and valid measures of rates of administrative succession and organizational effectiveness.

[1] A number of people have contributed to this study. I am grateful to Judith Kairath for doing the coding and to John Vincent and Jerry King for computational work. The Helms Athletic Foundation was most gracious in permitting use of its library and records. I am also indebted to the members of the Department of Sociology, University of California, Davis, for their numerous helpful comments when an earlier version of this paper was presented at a seminar. Professors Mayer Zald, Charles R. Wright, and Fritz J. Roethlisberger and an anonymous reviewer gave much constructive advice. This is an expanded version of a paper read at the annual meetings of the American Sociological Association in Washington, D.C., 1962.

[2] This hypothesis was discussed in my "Administrative Succession in Formal Organizations," *Social Forces*, XXXIX (December, 1960), 105–15.

[3] See my "Corporate Size, Bureaucratization, and Managerial Succession," *American Journal of Sociology*, LXVII (November, 1961), 261–69, and L. Kriesberg, "Careers, Organization Size, and Succession," *American Journal of Sociology*, LXVIII (November, 1962), 355–59. For a comprehensive discussion of other variables related to size see T. Caplow, "Organizational Size," *Administrative Science Quarterly*, II (March, 1957), 484–505.

[4] D. M. Sills, *The Volunteers* (Glencoe, Ill.: Free Press, 1957); W. A. Lunden, "The Tenure and Turnover of State Prison Wardens," *American Journal of Corrections*, XIX (November-December, 1957), 14–15; and A. Etzioni, "Authority Structure and Organizational Effectiveness," *Administrative Science Quarterly*, IV (June, 1959), 43–67.

Since the sixteen organizations selected for study, professional baseball teams, met all the relatively stringent requirements described, a second objective of this research was to illustrate some of the potentialities of sports organizations as objects of sociological investigation.

METHODS AND FINDINGS

All data for this study were gathered by means of secondary analysis of published documents.[5] Baseball teams and, in fact, most professional sports clubs offer the research advantages of public records of team personnel and team performance. This fact, as we shall see, also has important implications for the behavior of the organization.

Two time periods, 1921–41 and 1951–58, were selected for study. It was deemed wise to skip the World War II and immediate post-World War II periods.

The structure of baseball organizations is such that ultimate responsibility for the performance of the team is almost always fixed on one position, that of field manager. At the same time, official authority is generally concentrated in this position. Therefore, it was clear that personnel changes among field managers rather than club presidents, general managers, or team captains were central to the study. The number of managerial changes for each time period or the average length of managerial tenure constituted the rate of succession for each team.

The measure of organizational effectiveness was team standing, based on the number of games won and lost at the completion of the season. This might be considered analogous in some respects to productivity in industrial organizations. Georgopoulos and Tannenbaum's study of thirty-two similar suborganizations or stations demonstrated significant correlations between their various measures of organizational effectiveness: expert assessment of station effectiveness, productivity, intragroup strain, and flexibility.[6] It would certainly be safe to say that, among baseball experts, team standing is the most widely accepted criterion of effectiveness. Financial profit is also an important criterion. It would appear that the profitability of a baseball club is highly related to its team standing. Consistent with this assumption, we found a strong positive correlation between team standing and yearly attendance.[7]

Table 1 presents the basic data of the study. The data for Periods I and II taken separately or together strongly supported the hypothesized negative correlation between rates of managerial succession and organizational effectiveness. The correlations were considerably greater in the second time period, 1951–58, than in the earlier one. Rates of succession and team standing correlated −.40 in the first period and −.60 in the second. One team that contributed to the lower correlation in the earlier period was the Philadelphia Athletics. Despite the fact that the team consistently finished in the second division between 1921 and 1941, no managerial successions took place during this period. Undoubtedly, manager Connie Mack's ownership of the club assisted his long tenure. The Athletics experienced frequent managerial succession during 1951–58 with the departure of Mack from the scene.

In contrast, the Yankees, as Table 1 suggests, contributed to the magnitude of the correlation in both time periods. Not only

[5] H. Hurkin and S. C. Thompson, *The Official Encyclopedia of Baseball* (2d rev. ed.; New York: A. S. Barnes & Co., 1959); H. Johnson, *Who's Who in Baseball* (New York: Buston Publishing Co., 1953); F. Menke, *The Encyclopedia of Sports* (2d rev. ed.; New York: A. S. Barnes & Co., 1960); *1958 Baseball Guide and Record Book* (St. Louis, Mo.: Sporting News, 1958); T. Spink and Son, *Baseball Register*, compiled by T. Spink and P. Rickart (St. Louis, Mo.: Sporting News, 1940–41, 1951–58).

[6] B. S. Georgopoulos and A. S. Tannenbaum, "A Study of Organizational Effectiveness," *American Sociological Review*, XXII (October, 1957), 534–40.

[7] Profitability, attendance, and effectiveness are related in part because prolonged increases in profits tend to yield increases in organizational control over the market for new talent and therefore tend to produce a more effective farm system. Interpretation of the correlation between team standing and attendance should be approached cautiously. Attendance may also be a function of variables such as the total population of the metropolitan area, its particular age and sex distribution, and, of course, the number of professional baseball teams in the community.

were they highly effective, but they also experienced few managerial changes.

The second hypothesis was tested by examining the relationship between changes from Period I to Period II in the average length of time a manager retained his position with a team and changes in the team's standing. That is, we wanted to see if teams that kept their managers for shorter periods (experienced more succession) in Period II than they had in Period I were less effective in the later period and vice versa. In fact, the average tenure for managers declined in Period II for all but two clubs. As Table 2 demonstrates, our hypothesis was again strongly supported.[8] All eight teams that increased considerably their rate of managerial succession over that of the earlier period experienced a decline in average team standing. Moreover, the two clubs that decreased their rate of succession increased their effec-

TABLE 1

MEASURES OF SUCCESSION AND EFFECTIVENESS FOR SIXTEEN PROFESSIONAL BASEBALL ORGANIZATIONS OVER TWO TIME PERIODS*

TEAM	No. of Successions			Average Team Standing†		
	Period I (1)	Period II (2)	Periods I and II (3)	Period I (4)	Period II (5)	Periods I and II (6)
Phillies......	7	3	10	7.2	4.8	6.5
Giants.......	1	1	2	2.7	3.4	2.9
Cardinals....	10	4	14	3.0	3.8	3.2
Braves......	7	3	10	6.3	6.9	5.3
Pirates......	6	3	9	3.2	6.9	4.2
Cubs........	8	3	11	3.5	6.2	4.4
Dodgers.....	4	1	5	4.9	2.2	4.2
Reds........	7	3	10	4.9	4.9	4.9
Athletics.....	0	4	4	4.8	6.6	5.3
Nats........	6	3	9	4.2	6.8	4.9
Yankees.....	2	0	2	1.8	1.2	1.6
White Sox...	8	2	10	5.6	2.9	4.9
Red Sox.....	8	2	10	6.0	3.9	5.4
Indians......	6	1	7	3.9	2.6	3.6
Browns (Orioles)...	9	5	14	5.6	6.8	5.9
Tigers.......	4	4	8	3.9	5.4	4.3

* Period I, 1921–41; Period II, 1951–58. Rank-order correlations (Kendall's tau) and one-tail p values are: cols. (1) and (4), −.40 ($p < .02$); cols. (2) and (5), −.60 ($p < .001$); and cols. (3) and (6), −.43 ($p < .001$).

† A numerically high team standing meant low effectiveness.

TABLE 2

RELATIONSHIP BETWEEN CHANGE IN AVERAGE LENGTH OF MANAGERIAL TENURE AND AVERAGE TEAM STANDING FROM PERIOD I TO PERIOD II FOR FIFTEEN PROFESSIONAL BASEBALL TEAMS*

CHANGE IN AVERAGE MANAGERIAL TENURE	CHANGE IN AVERAGE TEAM STANDING	
	Increased Effectiveness	Decreased Effectiveness
Tenure longer..........	2	0
Tenure about same†....	4	1
Tenure much shorter....	0	8

* $P = .0014$ by Fisher's Exact Test if the categories "Longer tenure" and "Tenure about same" are combined. One team (Reds) that did not change its average team standing was excluded.

† Defined as a decrease of 0.3 year or less.

[8] We realize some of the interpretative limitations of utilizing team averages as measures of succession. A study comparing the "effectiveness" and length of tenure of the successor and his managerial predecessor is in progress. In this investigation the object of study is the manager and not the team. Some limitations in our measure of effectiveness also should be noted. Team standing may not reflect perfectly the ability of the team, just as fielding and

tiveness. However, it was evident that those teams that had experienced frequent and infrequent succession in the original period needed to be analyzed separately. Therefore, we controlled for average length of managerial tenure in Period I (a control for average team standing in Period I also would have been desirable, but we did not have a sufficient number of cases). The hypothesis was supported when the relationship was examined separately for teams that were below and above the median with respect to rates of succession in the first period (Table 3). Moreover, it should be noted that the single deviant case in Table 3 (the St. Louis Cardinals) was the team with the *lowest managerial tenure of any team in Period I.* This low rate remained about the same in Period II, although team effectiveness declined somewhat. We might speculate that perhaps (1) the very slight alteration of the club's policy of frequent succession was not above the threshold necessary to raise the organization's effectiveness, and/or (2) the slight decrease in the club's effectiveness did not encourage the owners to alter their policy of frequent succession.

TABLE 3

RELATIONSHIP BETWEEN CHANGE IN AVERAGE LENGTH OF MANAGERIAL TENURE AND AVERAGE TEAM STANDING FROM PERIOD I TO PERIOD II FOR FIFTEEN PROFESSIONAL BASEBALL TEAMS, CONTROLLING FOR AVERAGE LENGTH OF MANAGERIAL TENURE IN PERIOD I*

CHANGE IN AVERAGE MANAGERIAL TENURE	CHANGE IN AVERAGE TEAM STANDING		ONE-TAIL p LEVEL†
	Increased Effectiveness	Decreased Effectiveness	
A. Short tenure in Period I (below median):			
Tenure longer or about same‡..	3	1	.11
Tenure much shorter.......	0	3	
B. Long tenure in Period I (above median):			
Tenure longer or about same...	3	0	.018
Tenure much shorter......	0	5	

* One team (Reds) that did not change its average team standing was excluded.

† By Fisher's Exact Test.

‡ "About same" was defined as a decrease of 0.3 year or less.

batting averages are not ideal measures of individual performance. E.g., a team may improve over the course of a season and because of a poor start finish only second, although it is the best team by other standards. And the bias of the official scorer has a lot to do with the players' fielding and batting averages.

The findings of this study may be compared with a recent laboratory investigation by Trow.[9] Using Leavitt's Common-Symbol problem and the five-position chain organizational network, Trow found no significant linear relationship between mean rate of succession and long-run organizational performance. He did find that the mean performance of the twelve teams with the lowest replacement rates was significantly superior

[9] D. B. Trow, "Membership Succession and Team Performance," *Human Relations*, XIII, No. 3 (1960), 259–68. An immediate problem in making such a contrast is the critical difference in the objects of study. Trow applies his findings to "self-organizing" groups and points out several limitations of the experimental situation relevant to generalizing the findings. Formal organizations typically possess properties that laboratory organizations such as Trow's do not possess, such as: a formal system of authority, at least three levels of authority, and planned task differentiation. Moreover, when laboratory investigations have attempted to manipulate some of these differentiating variables, important results have been indicated. Hence, H. H. Kelley found that the existence of a hierarchy influenced communication ("Communication in Experimentally Created Hierarchies," *Human Relations*, IV, [1951], 39–56), and I. D. Steiner and W. I. Field found that the assignment of roles to persons in laboratory groups affected persons' perceptions of and reactions to one another ("Role Assignment and Interpersonal Influence," *Journal of Abnormal and Social Psychology*, LX, No. 2 [1960], 239–45). Of course, there are outstanding examples of experimental studies that have attempted to establish structures which legitimately could be called formal organizations. See, e.g., W. M. Evan and M. Zelditch, Jr., "A Laboratory Study on Bureaucratic Authority," *American Sociological Review*, XXVI, No. 6 (1961), 883–93.

to the mean performance of the twelve teams with the highest rates of succession. Trow discovered that *variability* in the rate of succession was a more important factor in team performance, noting that "whatever the average rate of succession, an increase in the rate, i.e., a temporal clustering of succession, tends to bring about a decrease in the level of organizational performance." In addition, he found that ability of the successor was a major factor in organizational performance. Thus, despite considerable differences between the techniques of secondary analysis and contrived experimentation, the findings of the two studies appear to be consistent at least with respect to the second hypothesis.

SUCCESSION AND EFFECTIVENESS

It is apparent that theoretical explanations for the findings of this study may be pursued from two opposite directions; it may be assumed that either effectiveness or succession functions as the primary independent variable. Our data demonstrate only the existence of an association, not its cause. Logic or common knowledge will not permit us to decide the issue. However, there is no intrinsic reason why a particular variable, such as rate of succession, could not be *both* a cause and an effect of effectiveness. This may very well be so in this instance.

A common-sense explanation for our results might suggest that effectiveness alone is the cause. The manager is fired because the team performs badly. Not only is the simplicity of this explanation appealing, but the negative correlation between succession and effectiveness is fully consistent with it. However, if taken by itself, this approach possesses all the deficiencies properly attributed to orientations that rest only on common knowledge: they typically do not stimulate careful empirical test; they typically do not suggest additional propositions which might be worthy of examination; they typically do not fit in systematically to a comprehensive body of generalizations in the field of interest. Naturally, we prefer explanations that can meet these and other criteria described by Nagel somewhat more adequately.[10]

If we assume that effectiveness and succession influence each other by contributing to managerial role strain, it is possible to formulate an alternative explanation for the major findings, one that ties in with a growing body of theory and research. It was this assumption that originally provoked this study. Succession, because it represents a universal organizational process, and effectiveness, because all formal organizations tend to strive toward the attainment of their official objectives, are strategic concepts for studying organizations within a comparative framework. Numerous studies conducted in the laboratory as well as in the field suggest that these variables produce reciprocal effects. For example, both Gouldner's and Guest's field research as well as Trow's experiment indicate that succession influences organizational effectiveness.[11] On the other hand, Hamblin's laboratory study suggests

[10] Ernest Nagel in a recent book provides an excellent discussion of the elements of the scientific and common sense approaches. He observed that "the sciences seek to discover and to formulate in general terms the conditions under which events of various sorts occur, the corresponding happenings. This goal can be achieved only by distinguishing or isolating certain properties in the subject matter studied and by ascertaining the repeatable patterns of dependence in which these properties stand to one another. In consequence, when the inquiry is successful, propositions that hitherto appeared to be quite unrelated are exhibited as linked to each other in determinate ways by virtue of their place in a system of explanation" (*The Structure of Science* [New York: Harcourt, Brace & World, 1961], p. 4).

[11] A. Gouldner, *Patterns of Industrial Bureaucracy* (Glencoe, Ill.: Free Press, 1954); R. H. Guest, *Organizational Change* (Homewood, Ill.: Dorsey Press, 1962); and Trow, *op. cit.* See also W. F. Whyte, "The Social Structure of the Restaurant Industry," *American Journal of Sociology*, LIV (January, 1949), 302–10; C. R. Christiansen, *Management Succession in Small and Growing Enterprises* (Boston: Graduate School of Business Administration, Harvard University, 1953); E. Dale, "Du Pont: Pioneer in Systematic Management," *Administrative Science Quarterly*, II (June, 1957), 26–30; O. Grusky, "Role Conflict in Organization: A Study of Prison Camp Officials," *Administrative Science Quarterly*, III (March, 1959), 463–67; and R. H. McCleery, *Policy Change in Prison Management* (East Lansing: Michigan State University, 1957), pp. 10–27.

that the ineffectiveness of the group contributes to high rates of succession among the leaders. When the leader could not solve a crisis problem confronting the group, he was replaced.[12] Accordingly, the relationship between rates of succession and organizational effectiveness was analyzed within the context of a conceptual scheme that focused on their interrelationships with a number of other variables: managerial (or executive) role strain, expectation of replacement, style of supervision, subgroup stability, morale, clientele support, degree of discrepancy between managerial authority and responsibility, and availability of objective assessment of organizational performance.

Figure 1 presents the proposed network of interrelations of the variables. The arrows indicate the direction of influence. Key propositions discussed below are followed by a numerical reference to the relevant variables. Of course, no attempt was made to exhaust the logical possibilities in the formation of propositions.

FIG. 1.—Organizational factors in team performance

The magnitude of managerial role strain is a general factor conditioning the nature of the relationship between succession and effectiveness.[13] By role strain is meant the extent to which role performance produces stress for the occupant of a position that cannot be fully relieved by institutionally legitimated means. Hence, this concept refers to the amount of tension with which a

[12] R. L. Hamblin, "Leadership and Crisis," *Sociometry*, XXI (December, 1958), 322-35.

[13] Position or office refers to a category that is located in the formal social structure of an organization. In a formal organization the category is defined in terms of its relationship with other positions that in turn are organized around the official objectives of the system. By role is meant a "set of evaluative standards applied to an incumbent of a particular position" (see N. Gross, W. S. Mason, and A. W. McEachern, *Explorations in Role Analysis* [New York: John Wiley & Sons, 1958], p. 60). Role strain is viewed in the present study as a more inclusive concept than role conflict. The latter is limited to situations of strain produced by incompatible expectations. W. J. Goode defines role strain as "the felt difficulty in fulfilling role obligations." Our definition differs in that it does not require a perfect association between perceived and objective role strain ("A Theory of Role Strain," *American Sociological Review*, XXIV [August, 1960], 483-96).

person is confronted as a result of occupying a particular office in an organization. The sources of strain will vary, of course, with the nature of the organizational setting, rank of the position, experience of the person, and so on. In general, organizational effectiveness should be inversely related to strength of managerial role strain (*1* and *3*); high levels of effectiveness (of the manager's unit) should be associated with low managerial strain and low levels of effectiveness correlated with high managerial strain. Perhaps, however, some optimum level of managerial strain is associated with maximum organizational effectiveness. At the same time, again assuming all else equal, the magnitude of managerial role strain should be positively correlated with rates of succession (*3* and *4*). Low strain defines a position as desirable. If the strain is too high, the manager searches for opportunities elsewhere, or redefines previous opportunities as attractive and eventually leaves the organization. Once again a simple monotonic relationship may be an oversimplification; too little strain may indicate a lack of "challenge" to the manager and thereby also stimulate turnover.

If rates of succession in a position have been high, and expectation of replacement arises, this, in turn, should contribute to managerial role strain. All else being equal, the stronger the expectation of replacement, the greater the role strain (*2* and *3*). Organizational effectiveness should be inversely related to strength of expectation of replacement; if the organization is performing well, the manager would not normally expect to be replaced (*1* and *2*). Strength of managerial role strain should also be related to style of supervision. All else being equal, the greater the role strain, the greater the likelihood that supervision will be close (*3* and *5*).[14] There are numerous studies relating closeness of supervision, morale, and organizational effectiveness (*1* and *7*; *1* and *5*).[15] Judging from Guest's study, we would expect that the greater the rate of succession in an organization, the greater would be the tendency to supervise closely (*4* and *5*). There is evidence suggesting that closeness of supervision is associated with degree of internal organizational stability (*5* and *6*).[16]

Two special sources of strain seemed pertinent to the analysis of the managerial role in baseball organizations: (1) the discrepancy between official responsibility and authority (we assumed that, in general, the greater this discrepancy, the greater the role strain [*3* and *10*]) and (2) the availability of objective assessment of managerial and team performance to the organization's clientele and higher levels of authority. We would expect that when objective assessment is available, the negative correlation between effectiveness and managerial role strain should be higher than when such objective assessment is not available (*8* and *9*). The first attribute concerns primarily the nature of the internal structure of baseball organizations, the second the relationship between the organization and its interested public.

Many of the role strains of the field manager emerge from the fact that he alone is acknowledged to be officially responsible for team performance. Therefore, it is defined as illegitimate for him to delegate ultimate responsibility for results either upward to the "Front Office" or downward to the coaches and individual players. At the same time, however, he depends, particularly over the long run, upon the front office for assistance by providing a strong farm system and advantageous trades, and, at all times, upon the quality of performance of the lower-level members of the hierarchy, the players. If they perform well, his position is secure; if they do not, it is in jeopardy.

However, although other managerial positions, such as those in business, often carry responsibility for results, what distinguishes the baseball manager is the fact that not only is he acknowledged to be responsible,

[14] Guest, *op. cit.*, chap. iii.

[15] Many of these studies are discussed in P. Blau and W. R. Scott, *Formal Organizations* (San Francisco: Chandler Publishing Co., 1962), pp. 140–64.

[16] Gouldner, *op. cit.*, and R. O. Carlson, *Executive Succession and Organizational Change* (Chicago: Midwest Administrative Center, University of Chicago, 1962).

but his superiors have objective data with which they can readily evaluate his performance. Unlike the typical business executive, for whom few clear standards of performance tend to exist, the baseball manager is exposed continually to seemingly unassailable comparative measures of effectiveness.[17] Moreover, the effects of many of the manager's daily decisions are a matter of public record. This means that every managerial decision that turns out to be an unfortunate one for the team, such as substituting in a key situation a mediocre left-hand hitting pinch batter for the team's star right-hand hitting slugger, is immediately "second-guessed" by the players, coaches, the front office, and the fans. The manager is constantly open to criticism, public and private.

The relationship between the field manager and his subordinates, upon whom he depends so heavily, is also influenced by the availability of public and objective measures of performance. Outstanding performance on the part of individual players insures their remaining with the club, and, importantly, the evaluation of this performance rests *not* with managerial subjective judgment as it does frequently in business firms, but instead is based largely on relatively objective standards of performance. Hence, the player in many respects is independent of managerial control. Where the ballplayer tends to resemble the traditional entrepreneur, the manager resembles the bureaucrat.[18] But the manager is a bureaucrat stripped of many vital bureaucratic controls. For example, the typical manager in a bureaucracy possesses power because he can limit the access of his subordinates to higher positions. However, the average ballplayer does not anticipate upward mobility in the ordinary sense within the structure of the professional baseball organization. In his case, upward career mobility applies primarily to the income and popularity rank systems; the players' major sources of reward are external to managerial control.

To a certain extent each manager develops his own inimitable way of handling players. After a while, the players feel comfortable with this style, and the younger ones in particular may feel that no successor can quite measure up to the standard (Willie Mays's reported fondness for Leo Durocher is a case in point). A managerial change inevitably upsets old patterns of behavior. New organizational policies, a different style of leadership, perhaps new players, and the addition of new coaches produce changes of great magnitude in the internal structure of the team. Members are forced

[17] See F. X. Sutton, S. E. Harris, C. Kaysen, and J. Tobin, *The American Business Creed* (New York: Schocken Books, 1962), pp. 336–38. In this study the authors point out how the *lack* of clear standards of performance can contribute to role strain. It should be pointed out that the skill of the baseball manager also is greatly affected by subjective judgments. As Sutton *et al.* point out, it often matters not that the effectiveness of the organization was in fact unrelated or only slightly related to the behavior of the manager. It is typically assumed in business, in baseball, and elsewhere, that a strong correlation exists between organizational effectiveness and the performance and ability of the manager in charge.

[18] Recently (April 1, 1962), sports columnist Frank Finch of the *Los Angeles Times* reported the difficult problem of control confronting Coach Pete Reiser of the Dodgers. Although Reiser is referred to as Howard's "father-confessor," it appears that the player refused to alter his batting stance to fit the coach's demands. "I'd like to have the authority to tell Frank to hit the way I say or else not play," the exasperated Reiser is quoted as saying, "but in baseball you just don't order people to do anything in a certain way." Of course, the fact that Howard in 1961 hit fifteen home runs and batted .296 contributed to his independence. Howard's reported point of view in this dispute parallels the individualistic spirit of the traditional entrepreneur: "I appreciate advice, and I accept it if it will help me, but Frank Howard, and nobody else, is going to help Frank Howard hit the ball on the button. When you step into the batter's box you're on your own. . . . This is a game made up of individuals." It would be a gross exaggeration to assert that Howard's attitude is universally found among ballplayers. Obviously, co-operation is common, perhaps especially among infielders who tend not to have high batting averages. Their skills are more likely to lie with the kinds of plays that require smooth co-ordination and not with the bat. For this and other related reasons, we hypothesized in another study that infielders and catchers would be more likely than outfielders and pitchers to become managers.

to adapt not only to the successor's new ways of doing things but also to the new informal coalitions that inevitably develop. The recruitment of the successor from the present staff or from outside the organization may be an important factor affecting the degree of instability created by succession.[19] Moreover, a high rate of managerial succession on a team tends to generate expectations, especially during a losing streak, that the current manager's job is in danger. This may encourage dissatisfied players to challenge the manager's authority and increase even more the felt discrepancy between his responsibility and authority. The result is greater managerial role strain.

In addition to the internal sources of tension, constant pressure on both the manager and the ordinary team members emanates from the organization's clientele. Unlike many other kinds of organizations, professional baseball teams must deal with a clientele that is both highly committed and highly informed. The strong emotional identification of the fan with "his" professional baseball or football club is often a part of the resident's identification with his local community. In some locales, such as Los Angeles, comprised of a large number of suburban subcommunities, it probably represents one of the more important integrative symbols. In the Green Bay area, the Green Bay Packers football team is referred to as a "regional religion."

Not only are the clientele strongly committed but in addition, as we suggested, they can readily and continually evaluate the effectiveness of the team since performance criteria are public knowledge.[20] In other types of organizations, the clientele cannot evaluate the effectiveness of the system with comparable precision. Consumers of an industrial corporation's products, for example, typically possess neither the propensity nor the knowledge to compare objectively the quality of the products they purchase or the "efficiency" of the corporation's employees. Accordingly, public relations and advertising men are probably able to manipulate the image of the corporation and its products much more effectively than can professional baseball teams.[21] Not even the best advertising men could have undone the damage to the Philadelphia Phillies' game attendance between 1934 and 1941 when they finished in last or next to last place every year.

Clientele support is critical because of its close relationship to morale and team effectiveness (*8* and *7; 8* and *1*); it is important in two ways. First, attendance is ostensibly highly related to profitability, and a drop in profitability produces strong pressures for managerial change. Second, high rates of attendance, by raising team morale, may contribute to team effectiveness as well as being affected by it. Our data revealed a strong correlation between effectiveness based on team standing and ranked yearly attendance. The zero-order correlations, by Kendall's tau, were as follows: for Period I, $T = .60, p < .0007$; for Period II, $T = .44, p < .009$; for Periods I and II combined, $T = .58, p < .001$. These data, of course, do not allow us to separate cause and effect. Mosteller's statistical study of the effects of playing "at home" and "away" upon winning World Series games found no significant differences in performance under the two conditions.[22] However, as he pointed out, outcomes of regular season games might still be influenced by this factor. He noted: (1) Baseball teams are often tailored to the home park because half the games are played there. Perhaps league champions are more skilful hitters and therefore less limited

[19] See Carlson, *op. cit.*

[20] Manager Freddie Hutchinson of the Cincinnati Reds once rather grimly described baseball as "the only sport in the world where everybody thinks he is an expert." No wonder several managers feel, as does Hutchinson, that fans are much too preoccupied with baseball statistics: "Now every club has to have a statistician, ours included. The statistician gives his figures to the newsmen and the broadcasters and now he's got everybody conscious of them."

[21] I am indebted to Professor R. J. Murphy for this observation.

[22] F. Mosteller, "The World Series Competition," *Journal of the American Statistical Association*, XLVII (September, 1942), 355–80.

by the dimensions of a particular park. (2) Fatigue from excessive traveling may disadvantage the away team to a greater extent during the regular season than during the World Series. Still another possibility is that clientele support is less critical for team performance during World Series competition than during the day-in-day-out play of the regular season. The extensive publicity and assured popular interest in the World Series generates sufficient enthusiasm on the part of the player whether he is playing at home or away. We suspect that the home crowd can exercise considerable influence on player performance during a regular season game. Under these conditions, enthusiastic support from the crowd may stimulate the player to "put out" more in the same way that a responsive audience can help produce scientillating dramatic performances on the stage. The ineffective team is less likely to receive this added inducement to perform well.

To summarize briefly: Our orientation focused on a set of ten variables. Analysis of the situation of the ineffective team may be used illustratively. If a team in ineffective, clientele support and profitability decline. Accordingly, strong external pressures for managerial change are set in motion and, concomitantly, the magnitude of managerial role strain increases. A managerial change may be viewed in some quarters as attractive in that it can function to demonstrate publicly that the owners are taking concrete action to remedy an undesirable situation.[23] The public nature of team performance and the close identification of community pride with team behavior combine to establish a strong basis for clientele control over the functioning of the team. These external influences tend to increase the felt discrepancy between managerial responsibility and actual authority. Since the rewards of popularity are controlled externally, individual rather than team performance may be encouraged. Similarly, the availability of objective performance standards decreases managerial control and thereby contributes to role strain. The greater the managerial role strain, the higher the rates of succession. Moreover, the higher the rates of succession, the stronger the expectations of replacement when team performance declines. Frequent managerial change can produce important dysfunctional consequences within the team by affecting style of supervision and disturbing the informal network of interpersonal relationships. New policies and new personnel create the necessity for restructuring primary relationships. The resulting low primary-group stability produces low morale and may thereby contribute to team ineffectiveness. Declining clientele support may encourage a greater decline in team morale and performance. The consequent continued drop in profitability induces pressures for further managerial changes. Such changes, in turn, produce additional disruptive effects on the organization, and the vicious circle continues.

Our findings demonstrating a negative correlation between rates of succession and effectiveness and a positive correlation between clientele support and effectiveness constitute only two connections of the chain depicted in Figure 1. The methodological weaknesses of studies such as the present one, based wholly on official documents, should not be underestimated. Clearly, such inquiries are not adequate substitutes for

[23] Although officially the manager may be held responsible for a team's poor showing, the fact that managers frequently are hired later by other clubs would suggest that their alleged ineptness is partly a screen. It is not easy for the front office to resist public pressures even if they might feel that the decision to replace the manager is unwise. The case of Mike Higgins and the Boston Red Sox is instructive, for it is one where the owner really did not want to fire the manager but did so anyway. Yawkey, the owner, and Higgins, the manager, were the best of friends. Yet a few years back when the Red Sox were doing very poorly, Yawkey gave in to public criticism and replaced Higgins. However, he kept Higgins on in the rather vague position of "troubleshooter." When the team still did poorly under Billy Jurges, Higgins was rehired. A new manager at least provides the fans with some hope for the coming season. Professor Gerard Brandmeyer kindly provided this example.

well-designed field and laboratory investigations. Systematic research examining, for example, the nature of the relationship between morale and effectiveness in baseball teams (morale and productivity studies of industrial organizations have produced contradictory findings[24]), morale and strength of clientele support, and managerial role strain and team effectiveness, would be highly desirable.

Several years ago Herbert A. Simon pointed out that the problem of organizational effectiveness was essentially an empirical one. He observes: "What is needed is empirical research and experimentation to determine the relative desirability of alternative administrative arrangements."[25] In addition, he emphasized two canons of research design: "First, it is necessary that the objectives of the administrative organization under study be defined in concrete terms so that results, expressed in terms of these objectives, may be accurately measured. Second, it is necessary that sufficient experimental control be exercised."[26] As an approximation to these principles, this study, by means of secondary analysis of published documents, has, in effect, compared the performance of professional baseball teams operating under contrasting administrative arrangements, the conditions of frequent and relatively infrequent managerial succession.

UNIVERSITY OF CALIFORNIA
LOS ANGELES

[24] E.g., R. L. Kahn and N. C. Morse, "The Relationship of Productivity to Morale," *Journal of Social Issues*, VII, No. 3 (1951), 8–17; D. Katz, N. Maccoby, and N. C. Morse, *Productivity, Supervision and Morale in an Office Situation* (Ann Arbor, Mich.: Institute of Social Research, 1950); D. Katz, N. Maccoby, and L. G. Floor, *Productivity, Satisfaction and Morale among Railroad Workers* (Ann Arbor, Mich.: Institute of Social Research, 1951); and N. C. Morse, *Satisfactions in the White-Collar Job* (Ann Arbor, Mich.: Institute for Social Research, 1953); H. Wilensky's paper in C. Arensberg *et al.* (eds.) *Research in Industrial Human Relations: A Critical Appraisal* (New York: Harper & Bros., 1957), pp. 25–50.

[25] *Administrative Behavior* (2d ed.; New York: The Macmillan Co., 1958), p. 42.

[26] *Ibid.*

THE CONCEPT OF BUREAUCRACY: AN EMPIRICAL ASSESSMENT

RICHARD H. HALL

ABSTRACT

The concept of bureaucracy is viewed as a series of dimensions, each in the form of a continuum. When each continuum is measured, no concomitant variation is found among the dimensions. It is suggested that the bureaucratic concept is more empirically valid when approached in this manner, rather than assuming that organizations are totally bureaucratic or non-bureaucratic. The suggested approach is demonstrated through the application of the model to ten organizations.

Students of organizations from the time of Weber to the present have used the bureaucratic model as the basis for conceptualizing the system of interrelationships in organizations. This acceptance of the bureaucratic model has served as the point of departure for studies of the development and modification of organizational structure, the place of the individual within such a structure, and various associated problems. This paper examines the bases of the bureaucratic model, the dimensions of organizations that are characteristically cited as bureaucratic attributes by measuring the degree to which these dimensions are present in a variety of organizations.

There has been an unfortunate lack of sophistication in the use of the bureaucratic concept. All too often organizations have been labeled "bureaucratic" for purposes of study when little evidence has been presented that they are in fact bureaucratic. Alvin Gouldner has pointed out that the bureaucratic model has been used as a finished tool rather than as a set of hypotheses to be verified by empirical findings.[1] In a similar vein, Udy has suggested that the Weberian ideal-typical attributes be recast as variables in order to determine their empirical interrelationships.[2] Upon closer examination, the characteristics or dimensions that are typically ascribed to bureaucracy appear to be variables that can be systematically measured to demonstrate the degree to which organizations are or are not bureaucratic.

BUREAUCRATIC DIMENSIONS

Max Weber, in his formulative work on bureaucracy, described bureaucratic organizations from the dimensional perspective.[3] That is, he listed a series of organizational attributes that, when present, constitute the bureaucratic form of organization. These dimensions, including division of labor, hierarchy of authority, extensive rules, separation of administration from ownership, and hiring and promotion based on technical competency, have served as the basis for subsequent delineations of bureaucratic structure.[4]

Carl J. Friedrich, commenting on Weber's work, incorporates six of Weber's characteristics into his own formulation.[5] Michels, although writing in another context, similarly uses the dimensional approach.[6] Contemporary sociologists have either directly relied upon the Weberian

[1] "Discussion," *American Sociological Review*, XIII, No. 4 (August, 1948), 396.

[2] Stanley H. Udy, Jr., " 'Bureaucracy' and 'Rationality' in Weber's Organization Theory: An Empirical Study," *American Sociological Review*, XXIV (December, 1959), 792.

[3] *The Theory of Social and Economic Organization*, trans. A. M. Henderson and Talcott Parsons (New York: Oxford University Press, 1947).

[4] *Ibid.*, pp. 330–34.

[5] "Some Observations on Weber's Analysis of Bureaucracy," in *Reader in Bureaucracy*, ed. R. K. Merton *et al.* (Glencoe, Ill.: Free Press, 1949), p. 29.

[6] Robert Michels, *Political Parties* (Glencoe, Ill.: Free Press, 1949), pp. 33–34.

formulations in their discussions,[7] or have used selected dimensions based upon the Weberian model.[8]

Those students who have used this theoretical model as the basis for empirical research or theoretical development have typically made the assumption that the dimensions are present in the first case or would be present in the latter case in the organizations under study or consideration.

This assumption was the source of Gouldner's concern noted above. In a later publication, he more specifically questions much of the current usage of the bureaucratic model:

> It is instead an *ideal type*, in which certain tendencies of concrete structures are highlighted by emphasis. Not every formal association will possess all of the characteristics incorporated into the ideal-type bureaucracy. The ideal type may be used as a yardstick enabling us to determine in which particular respect an organization is bureaucratized. The ideal-type bureaucracy may be used much as a twelve-inch ruler is employed. We would not expect, for example, that all objects measured by the ruler would be exactly twelve inches—some would be more and some would be less.[9]

Stated in other terms, Gouldner, and later Udy, imply that bureaucracy is a condition that exists along a continuum, rather than being a condition that is either present or absent. This point may be expanded to state that bureaucracy is a form of organization which exists along a number of continua or dimensions.

[7] E.g., Talcott Parsons, *The Structure of Social Action* (New York: McGraw-Hill Book Co., 1937), p. 506, and Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1949), pp. 151–52.

[8] E.g., Marshall E. Dimock, *Administrative Vitality* (New York: Harper & Bros., 1959), p. 5; Ferrel Heady, "Bureaucratic Theory and Comparative Administration," *Administrative Science Quarterly*, III, No. 4 (March, 1959), 516; Peter Blau, *Bureaucracy in Modern Society* (New York: Random House, 1956), p. 19; and Morroe Berger, *Buraucracy and and Society in Modern Egypt* (Princeton, N.J.: Princeton University Press, 1957), p. 48.

[9] Alvin Gouldner, *Studies in Leadership* (New York: Harper & Bros., 1950), pp. 53–54.

In regard to the dimensionality of bureaucracy, Udy's findings are relevant. In his study of formal organizations in non-industrial societies, Udy utilized seven characteristics of bureaucracy. These characteristics, subdivided into "bureaucratic" and "rational" elements, were also treated as dimensions.[10] Using a present versus absent dichotomy for each characteristic, he found a variation among the associations in the pattern of presence versus absence for the seven dimensions. The characteristics were not either all present nor all absent in any one organization. Instead, some had certain configurations of present versus absent characteristics, while other organizations had other configurations. These findings support the contention that the bureaucratic model is best approached from the dimensional perspective.

In order to study this dimensionality further, specific dimensions had to be delimited. A review of the relevant literature yielded the dimensions noted in Table 1. The author citing each dimension is also listed.

Six dimensions were chosen for use in this study on the basis of frequency of citation and theoretical importance. They are:

1. A division of labor based upon functional specialization
2. A well-defined hierarchy of authority
3. A system of rules covering the rights and duties of positional incumbents
4. A system of procedures for dealing with work situations
5. Impersonality of interpersonal relations
6. Promotion and selection for employment based upon technical competence

In the ideal-type bureaucracy all of these dimensions would be present to a high degree, while non-bureaucratized or simple organizations would ideally have a low degree of all the dimensions present. Stated more directly, a highly bureaucratized organization would be characterized by an intricate division of labor; a multilevel, closely followed hierarchical structure; ex-

[10] *Op. cit.*, pp. 791–95.

tensive rules governing on the job behavior; well-developed and systematically followed work procedures; interpersonal behavior, both between organizational members and toward outsiders, governed by norms that stress the importance of the positional rather than the personal basis for interaction; and the importance of successful performance as opposed to sentiment as a basis for both hiring and promotion practices. The non-

TABLE 1

CHARACTERISTICS OF BUREAUCRACY AS LISTED BY MAJOR AUTHORS

Dimensions of Bureaucracy	Weber	Friedrich	Merton	Udy	Heady	Parsons	Berger	Michels	Dimock
Hierarchy of authority......	*	*	*	*	*	*	*	*	*
Division of labor...........	*	*	*	*	*	*	......	*	*
Technically competent participants................	*	*	*	*	......	*	*	......	......
Procedural devices for work situations...............	*	*	*	......	*	......	*	......	*
Rules governing behavior of positional incumbents.....	*	*	*	......	......	......	*	*	......
Limited authority of office...	*	......	*	......	*	*	......	......	......
Differential rewards by office.	*	......	......	*	......	......	......	......	......
Impersonality of personal contact..................	......	......	*	......	......	......	......	......	......
Administration separate from ownership...............	*	......	......	......	......	......	......	......	......
Emphasis on written communication.................	*	......	......	......	......	......	......	......	......
Rational discipline..........	*	......	......	......	......	......	......	......	......

* Cited by author.

bureaucratic organization would be characterized by a relatively flat and often by-passed hierarchy, simpler division of labor, and so on.

The question that immediately arises, based on the contributions of Udy and Gouldner, is: Do actual organizations conform to either of these ideal extreme types? As has been noted, many observers have assumed that they do, but even from a common-sense approach an opposite assumption appears to be more defensible. In order to delineate more accurately the actual structural characteristics of organizations in relation to the bureaucratic dimensions, and to test the proposition that bureaucracy actually exists in degrees along the six dimensions, the following hypothesis was tested: The bureaucratic characteristics described above are not highly intercorrelated; thus, organizations that are highly bureaucratized on any one dimension are not necessarily so on the other dimensions.

This hypothesis assumes that the bureaucratic dimensions do in fact exist in the form of continua and that these continua are measurable. Both of these assumptions will be demonstrated.

METHODOLOGY

There appeared to be two possible methods of dimensional measurement. One alternative was to observe the actions of organizational participants, noting the incidence of the various types of activities associated with each dimension. Besides being extremely time-consuming in multi-organizational analysis, this method would require prolonged and complete observation in any one organization.

The second alternative was the use of interview responses from organizational participants. The responses for this method would necessarily be directed to the dimensions studied. This second method was selected on the basis of its greater simplicity

and equal reliability.[11] Interview responses can be ordered by the use of carefully prepared scales for this purpose.

It is recognized that the perceptions of participants of their organization may well be at variance with the officially prescribed structure. The official structure, however, is only as important as the degree to which it is adhered to. If the actual organizational structure is a replica of the formal structure, then the formal structure is the significant structural component. On the other hand, the degree of variation from the formal structure is the actual significant structure for organizational operation. Accurate measurement of participant perceptions should therefore yield a reliable and valid representation of the actual organizational structure.

Scales were constructed for measurement of each of the six dimensions. The format of the Likert internal-consistency technique was employed together with other methods.[12] Preliminary items, in the form of descriptive statements related to one of the six dimensions, were collected and edited. The items were placed in preliminary scales on the basis of the "logical" fit of the items. The scales were then pretested to eliminate irrelevant items and, perhaps even more importantly, to eliminate any interdependency among the scales.[13]

The six scales that were developed on the basis of these considerations were comprised of items to which organizational respondents were to respond. The responses were based on a five-point scale designed to indicate how closely each statement corresponded to an accurate description of the organization. Examples of items from each scale are:

1. Hierarchy of authority scale: "A person can make his own decisions without checking with anyone else"
2. Division of labor scale: "One thing people like around here is the variety of work"
3. System of rules scale: "The time for coffee breaks is strictly regulated"
4. System of procedures scale: "We are to follow strict operating procedures at all times"
5. Impersonality scale: "We are expected to be courteous, but reserved, at all times"
6. Technical competence scale: "Employees are periodically evaluated to see how well they are doing"

In their final form, the scales were quite brief; five contained ten items each, while the sixth contained twelve items. In spite of their brevity, all the scales were reliable with the corrected split-half correlation coefficients ranging between .80 and .90.

Validity of the scales was more difficult to establish. Inspection led to the conclusion that a high degree of face validity was present. More significantly, the use of external criterion groups indicated that all scales were valid. For each scale the organizations that scored at both extremes were examined to determine whether the scale scores corresponded with the relevant organizational characteristics. The absence of other measures of the dimensions made such a determination difficult, but observational and inter-

[11] Each of the measurement methods outlined involves the possibility of perceptual variations. In the first case the observer may bring a particular biasing viewpoint with him (e.g., antilabor or antimanagement). In the latter two cases reliance upon participants' perceptions could be a problem if sufficient care is not exercised in sample selection.

[12] A complete discussion of the methods of scale development is included in Richard H. Hall, "An Empirical Study of Bureaucratic Dimensions and Their Relation to Other Organizational Characteristics" (unpublished Ph.D. dissertation, Columbus, Ohio State University, 1961).

[13] Maximal elimination of interdependency was accomplished by analyzing each scale item of each scale in its relationship to the other five scales. Those items that were discriminating in more than one scale were eliminated from consideration for the final scales. It should be noted that total independence of scales was not achieved. Some scale items had partial relevance in other scales. It appears to be almost impossible to eliminate all such interdependence since the dimensions under study are in fact parts of a whole, the organization. Nevertheless, there is a high degree of independence in the scales. Furthermore, any interdependence that does exist among the scales would lead to higher, rather than lower, intercorrelations among the dimensions. As will be indicated below, the degree of intercorrelation among the dimensions in actual organizations is relatively low.

view data did indicate a high degree of validity. For example, on the hierarchy of authority dimension, the most bureaucratic organization studied was a large hotel that had rigid separation into levels of authority, even in the smaller subdivisions. On the other hand, the least bureaucratic organization was a stock brokerage firm, not included in the final organizational sample because of its small over-all size, in which there were few hierarchical levels. Once licensed, a "representative" had equal authority in comparison to the other "representatives." It was a very flat organization in terms of its hierarchical structure. Similar comparisons were made on the other five scales to establish their validity.[14]

The scales were designed to be administered to the personnel of the organizations selected for study. The organization was assigned a score on each dimension based on the mean of all responses from that organization. The mean scores yield an ordinal score for each organization.[15]

Data were collected from the employees of ten organizations.[16] In each organization a systematic random sample of employees was selected that was designed to include members of both "management" and "worker" categories in order to reduce bias from either perspective. Respondents from the various internal segments of the organizations were similarly included to avoid any departmental bias.

The selection of organizations for study was purposive to the extent that organizational variety in terms of type, age, and size was desired in order to demonstrate the relationship between the several dimensions. If there is concomitant variation in the magnitude of the dimensions, it should be present when a series of organizations are compared. On the other hand, if interorganizational comparisons indicate that the dimensions do not vary concomitantly, then the dimensional nature of bureaucratic structure will be further demonstrated.

The final sample included organizations that performed a number of different functions. The organizations ranged in size from 65 to 3,096 employees and in age from four to sixty-three years.[17]

FINDINGS

When the mean scores on each dimension were tabulated, a range of scores for the ten organizations was found (Table 2). It should be noted that these scores do not correspond to the possible range of scores for each scale. The possible range is from 12 to 60 on the hierarchy dimension and from 10 to 50 on the other five dimensions. As Table 2 indicates, the scores tend to cluster within a more limited range of the total possible range. It may be possible to locate organizations that would approximate either extreme on any one dimension, but this appears somewhat unlikely. Since the scales are ordinal, the range of the actual scores for the organizations will not approximate

[14] A more complete description of the methods of establishing validity is found in Hall, *op. cit.*

[15] It must be noted that the use of data from organizational members was done with the realization of the frequently made distinction between the official and unofficial structures of organizations (formal-informal structures). It was recognized that the unofficial relationships that develop in work situations can modify any officially prescribed arrangement. This frequently made observation should not be overstressed, however, for as William F. Whyte has indicated, the formal or official structure sets limits on the shape of the unofficial structure ("The Social Structure of the Restaurant," *American Journal of Sociology*, LIV, No. 4 [January, 1949], 308). The *actual* organizational structure is thus the pattern of interrelationships that exists. It is a result of both the official and unofficial structures.

[16] The use of multiple organizations in testing these hypotheses will allow any generalizations proceeding from the findings to be more widely applicable. Although some would argue that the concept of bureaucracy can only be applied within one organization at one time, it is this writer's belief that the utility of the concept must be demonstrated within a range of organizations. These organizations are comparable in that they reflect contemporary Western culture. Cross-cultural comparisons, such as Udy's, require the assumption that such an organizational structure, or at least some parts of it, approach universality as organizational components.

[17] Pertinent information on organizational selection and descriptions of the organizations selected are included in Hall, *op. cit.*

the theoretically possible range. The significant point is that a range does exist.

From Table 2, it is evident that the assumptions made regarding the nature of the bureaucratic dimensions are upheld; the degree to which each dimension is present ranges along a continuum, rather than existing in a present-absent dichotomy. Bureaucracy in general thus may be viewed as a matter of degree, rather than of kind.

The major hypothesis was tested by use of the Spearman rank-order correlation method. A rank was assigned to each organization on each dimension. Rank-order coefficients between each dimension and every other dimension were computed and are shown in Table 3.

None of the coefficients reach the .05 level of significance, upholding the central hypothesis. In the organizations studied, there is no concomitant variation in the presence-absence patterns among the dimensions. The relatively small number of organizations included in this study indicates that caution should be exercised in the interpretation of these findings. The results of this analysis are suggestive, however, of the variability with which the degree of bureaucratization can exist.

Analysis of the matrix of rank correlation coefficients in Table 3 suggests that the "hierarchy" dimension may be the central dimension in the determination of the overall degree of bureaucratization. This can only be looked upon as a tentative proposition, however, because the data from this research do not yield significant conclusions in this direction.

TABLE 2

MEAN SCORE OF EACH ORGANIZATION ON EACH DIMENSION OF BUREAUCRACY*

Rank of Organization	Sample N	Dimension					
		Hierarchy of Authority	Division of Labor	Rules	Procedures	Imperson-ality	Technical Qualifications
1	35	33.1	29.0	27.6	26.5	21.9	22.5
2	26	33.3	32.3	36.0	29.4	25.0	20.8
3	37	33.4	34.7	22.2	28.9	23.9	20.8
4	36	34.5	34.3	28.7	26.7	25.6	23.2
5	32	36.7	36.8	26.3	29.4	24.8	16.3
6	28	36.5	31.5	31.0	28.6	21.4	20.2
7	45	36.7	32.7	23.9	31.0	31.0	19.0
8	26	37.0	28.6	28.3	28.1	26.3	22.0
9	26	38.3	36.3	36.9	32.7	30.6	25.5
10	26	38.9	35.9	33.2	33.2	27.7	19.3

* High score = less bureaucratic; low score = more bureaucratic. Difference between extreme scores on all dimensions significant at .05 level.

TABLE 3

RANK-ORDER INTERCORRELATION COEFFICIENTS BETWEEN DIMENSIONS*

(N = Organizations)

	Hierarchy of Authority	Division of Labor	Rules	Procedures	Imperson-ality
Division of labor	.419				
Rules	.594	.134			
Procedures	.660	.678	.167		
Impersonality	.678	.266	.194	.624	
Technical qualifications	.032	.300	.627	−.303	.170

* The ranking of each organization was from most bureaucratic to least bureaucratic on each dimension.

Another possibility that should be considered is that organizations of similar types may be found to have similar configurations of dimensional magnitude, with the corresponding concomitant variation among the dimensions. Two organizations, both marketing divisions of large national firms, which were *not* included in the final organizational sample due to inadequate sample size, were found to have quite similar dimensional configurations. Although the data were insufficient for analysis, they are suggestive of the possibility of such common configurations.

TABLE 4

SIZE AND AGE OF ORGANIZATIONS CORRELATED WITH MEAN SCORE ON BUREAUCRATIZATION DIMENSIONS*

(N = 10 Organizations)

Dimension	Rank-Order Correlation Coefficients	
	Size	Age
Hierarchy of authority	.227	−.409
Division of labor	.362	.423
Rules	.398	.411
Procedures	−.201	−.171
Impersonality	.133	.001
Technical qualifications	.403	.368

* Organizations ranked 1–10 from most bureaucratic to least bureaucratic. Size and age ranked 1–10 from largest and oldest to smallest and youngest.

Although there may be similar configurations of bureaucratization among organizations of the same type, evidence obtained during the course of the research indicated that the commonly noted factors of organizational size and age were not related to the degree of bureaucratization on the six dimensions. As Table 4 indicates, none of the rank-order correlation coefficients indicated any significant relationship between age or size and degree of bureaucratization.

The fact that the size and age of the organizations studied were not highly related to the degree of bureaucratization should not be taken as conclusively ruling out the effects of these factors. If a larger organizational sample were taken, covering a wider range of age and size, the importance of these factors could be more easily determined. It may well be, however, that these factors have been overestimated as the important determinants of bureaucratization. From the evidence here, the type of organizational activity appears to be of greater importance.

It must be stressed, however, that the organizations studied did not yield any significant patterns either in the interrelationships among the dimensions or in over-all organizational configurations.

These findings raise some serious questions about bureaucratic theory as it has been commonly formulated, and about many empirical studies done in "bureaucratic" settings. First, the findings indicate that what is commonly approached as a totality (bureaucracy) is not such an integrated whole in reality. The configurational nature of the degree to which the dimensions are present suggests that organizations are indeed composed of the commonly ascribed dimensions, but these dimensions are not necessarily all present to the same degree in actual organizations. The bureaucratic concept would appear to be more descriptively accurate if it were rephrased in these dimensional terms, with an emphasis on the continual nature of the dimensions. This would allow continuity in the over-all discussions of "highly" bureaucratized organizations, but in a more sophisticated manner. It would also allow other organizations to be studied from the bureaucratic perspective with less conjecture as to their degree and type of bureaucratization.

Second, many of the empirical studies of conditions within the bureaucratic setting and of the setting itself might be reexamined. There is little empirical evidence presented in many studies that yields any reliable estimate of the degree to which the organizations are bureaucratized along the dimensions cited by the authors themselves. Indeed, some research in this area has simply stated that the organizations under investigation are bureaucratic without any

attempt to demonstrate their degree of bureaucratization. From the evidence presented in this study, a reassessment of certain assumptions included in past and contemporary research is warranted. A more exacting delineation of the degree to which the organizations being studied are bureaucratized would aid in the over-all systematization of findings from the field.

Third, the direction of the relationship between the "technical qualifications" dimension and three of the other dimensions is negative, as Table 3 indicates. This finding, while not conclusive, does raise the question of the appropriateness and utility of the inclusion of the dimension in the bureaucratic model. If technical competence is taken to encompass a general high level of training and ability, then it may not be an appropriate dimension. In a highly bureaucratized situation (along all dimensions) the highly competent person might not be able to exercise the full range of his competence due to specific procedural specifications, limited sphere of activity, limited authority due to hierarchical demands, etc. The generally competent person in these terms could easily manifest the symptoms suggested by Merton and others.[18] If, however, this dimension were rephrased to specify that technical competence is required only to the degree necessary to fulfil each job requirement within the hierarchical structure, the dimension might have more verifiable utility.

These findings are similar to those of Udy in certain regards. In distinguishing between the "bureaucratic" and "rational" elements of organizational structure, he notes that there are high interrelationships among the attributes of each element. There was also, however, a negative association between the attributes of the two elements.[19] The dimensions of the present study, while essentially on the "bureaucratic" side of Udy's distinction do include one dimension which could be termed as "rational."

The "technical qualifications" dimension can be viewed as a "rational" aspect of the over-all concept of bureaucracy used here. As noted above, this dimension was generally negatively associated with the other dimensions. The findings of Udy and this study thus concur on this point. As Udy suggests, the distinction made may be operative in contemporary society as well as in the non-industrial societies that he analyzed.

CONCLUSION

This research has raised some serious questions about the theoretical and empirical application of the bureaucratic model of organizational structure to actual organizations. The model itself was not questioned, nor was the possibility of finding organizations that would approach, in all dimensions, the polar extremes of the ideal type. However, through the use of measuring devices, it was shown that, in the organizations studied, the bureaucratic dimensions existed independently in the form of continua.

While additional evidence is needed to demonstrate conclusively the validity of the findings reported in this paper, certain conclusions can be drawn. First, the bureaucratic dimensions are meaningful organizational structural attributes. Second, when measured quantitatively, the dimensions exist in the form of continua rather than as dichotomies. Third, the magnitude of the dimensions varied independently in the organizations studied.

Factors related to the degree of bureaucratization along each dimension were examined. From the data available, it appears that the type of organizational activity may be highly related to the degree of bureaucratization. Additional research in this area may indicate that certain organizational activities are related to particular degrees of bureaucratization on one or several dimensions. For example, organizations that regularly deal with a large volume of customers or clients may develop a high degree of impersonality. At the same time, a rather low

[18] See Merton, *Social Theory and Social Structure*, and P. Brown, "Bureaucracy in a Government Laboratory," *Social Forces*, XXXII (1954), 259–68.

[19] Udy, *op. cit.*, p. 794.

degree of procedural specificity could be present if there is much variation in the type of interaction involved. While the activity factor appears to be related to the degree of bureaucratization, the factors of age and size did not emerge as important factors in this study.

These findings suggest some additional directions that research in this field could pursue. Studies of conditions that are thought to be associated with a bureaucratic structure could utilize this framework to specify more accurately the situations in which such conditions arise. Certain problems that are typically ascribed to bureaucratic structures may in fact only exist when a particular configuration of the dimensions is present. Some "problems of bureaucracy" could be more concretely studied if the exact nature of the setting were better understood.

The use of this dimensional approach could also lead to a more accurate delineation of the organizational form that is most "rational" for the pursuit of particular organizational goals. For instance, an intense emphasis on procedures may be very useful in one type of organization but not in other types. It is within the realm of possibility that there are optimal or most rational forms of organization for particular organizational activities. Bureaucracy, as it has commonly been used, may not be that rational form described by Weber, but particular configurations of the bureaucratic model may be the most rational form for particular activities.

If human life is increasingly becoming organizational life, a better knowledge of the organizational structures in which such lives are led may allow a more realistic confrontation of the problems associated with such a life. The mere fact that the "organization man" is a symbol of modern life for some people is not enough for the understanding of the organizational forces that produce such a man. Inasmuch as all organizations are not equally bureaucratic, there are undoubtedly a variety of organizational factors that contribute to a variety of different types of "organization men." More adequate knowledge of such factors is thus essential to the understanding of the overall significance of organizations for contemporary life.

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ALTERNATIVE QUESTIONNAIRE APPROACHES TO THE MEASUREMENT OF INFLUENCE IN ORGANIZATIONS[1]

MARTIN PATCHEN

ABSTRACT

Employees in a manufacturing company and in a set of auto dealerships were asked to make global judgments about the distribution of influence in their organizations; they were also asked about influence in specific decision areas. An index based on reports of influence in specific areas shows better reliability among non-supervisory respondents than does the global influence measure. There is, however, poor agreement across organizational levels (supervisory versus non-supervisory) in responses to either influence measure. Neither measure shows clear superiority as a predictor of employee morale.

For the measurement of organizational characteristics (power structure, communication patterns, methods of supervision, etc.), researchers have often relied on the reports of organization members. Such reports may be obtained from selected informants or from a sample survey. In either case, a further choice of method must be explicitly or implicitly made by the researcher. Should he use global questions that ask the respondent to generalize about such matters as the power structure, the communications structure, and the methods of supervision? Or should he ask relatively specific, detailed questions of the respondents and make the generalizations himself on the basis of the detailed answers? Both these techniques have been used to measure, among other things, the distribution of power and influence within organizations. This paper will attempt to assess the relative usefulness of these two methods of measuring influence. This evaluation will be based on data from two business sites—a manufacturing company and a set of sales organizations.

It is useful, first, to look briefly at some instances in which the "global" or the "specific-item" method of measuring influence have been used, and at some of the advantages that might be claimed for each approach. Arnold Tannenbaum and his associates have used the technique of the "control graph" in a number of organizational studies.[2] In this approach, a global question of the following sort is asked (of union members in this instance): "In general how much do you think the membership has to say about how things are decided in this local?" Five fixed alternative responses range from "They have a great deal of say" to "They have no say." Parallel questions are asked concerning the influence of such other hierarchical levels as the international union, the local president, and the plant bargaining committee.[3] Measures of the "slope" of the control curve—that is, of the relative power at different levels—and of the total height of the curve have been found by Tannenbaum to be related to organizational efficiency and/or to member satisfactions.[4]

Lazarsfeld and Thielens in their study of the "academic mind" also used a global question to assess the power situation in

[1] Work for this paper was supported by the Air Force Office of Scientific Research of the Air Research Development Command, under contract No. AF 49(638)-1032. Costs of data collection were paid by the companies at which the research was done.

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[2] A. S. Tannenbaum and R. L. Kahn, "Organizational Control Structure: A General Descriptive Technique as Applied to Four Local Unions," *Human Relations*, X, No. 2 (1957), 127–40; and A. S. Tannenbaum and R. L. Kahn, *Participation in Union Locals* (Evanston, Ill.: Row, Peterson & Co. 1958).

[3] *Ibid.*, pp. 252–53.

[4] A. S. Tannenbaum, "Control and Effectiveness in a Voluntary Organization," *American Journal of Sociology*, LXVII, No. 1 (July 1961), 33–46.

universities.[5] They asked a sample of faculty members: "If you had to choose one, who would you say has the most powerful voice here on this campus, in determining the degree of academic freedom that exists here —the trustees, the president, the deans, the heads of departments, the faculty, the students, or who?" A measure based on these answers was strongly related to the over-all quality of the university.

A somewhat different approach, using more detailed information, has been used in surveys of the American Association of University Professors, in an attempt to measure the authority of university faculties.[6] Informants were asked to report on such relatively specific questions as whether the faculty was consulted on certain appointments and on various budget matters; whether there is an academic senate; and whether there is a definite plan for an exchange of ideas between faculty and trustees.

Another example of the use of more detailed questions is that of a study of union influence by Chalmers and his associates.[7] They asked organization members not for an over-all judgment of union influence but instead about union influence in such specific areas as hiring and firing, grievance procedures, setting piece rates, efficiency, and technical change. Using an index based on these specific areas of power, they found that greater union influence is related to greater satisfaction by the union but to lesser satisfaction by management.

We have, then, two techniques by which organization members may be asked about power (and other) relationships—one by obtaining over-all global judgment, the other by getting information about a list of specifics. The global judgment method has the advantage of brevity and simplicity. It takes less of the respondent's time. It demands less knowledge about the organization by the researcher because he does not have to know precisely what specifics to ask about. One might argue that it also gives as valid, or more valid, information because the respondent makes an over-all judgment on the basis of intimate knowledge of the organization rather than on the basis of a handful of perhaps arbitrarily chosen specifics.

An advocate of the more concrete approach might argue, however, that the global assessment approach has relatively poor reliability and validity. Each respondent may be thinking of different specifics when he is asked to make an over-all judgment. Or he may give different weights to the various specifics than another man does. Or the implicit process of weighting may be too complex for him to attempt, and he may, therefore, answer capriciously.[8]

To bring some evidence to bear on this question, we will examine data from studies of two organizations by the Survey Research Center. These studies used *both* global measures of influence within the organization and indexes based on influence in specific areas. The degree to which consensus is present for each of the two types of measures, both within and across hierarchical levels, and the relationships of each type of measure to other relevant variables will be examined.[9]

[5] P. F. Lazarsfeld and W. Thielens, Jr., *The Academic Mind* (Glencoe, Ill.: Free Press, 1958).

[6] "The Place and Function of Faculties in College and University Government," *A.A.U.P. Bulletin*, XLI (1955), 62–81.

[7] W. E. Chalmers, M. K. Chandler, L. I. McQuitty, R. Stagner, D. W. Wray, and M. Durber, *Labor-Management Relations in Illini City*, II: *Studies in Comparative Analysis* (Champaign, Ill.: Institute of Labor and Industrial Relations, University of Illinois, 1954).

[8] There is also the question of whether there is a single influence structure or several structures, one for each of several substantive areas. The literature indicates that the latter is the case for many community power structures (see, e.g., N. W. Polsby, "The Sociology of Community Power: A Reassessment," *Social Forces*, XXXVII [1959], 232–36). The theoretical and empirical work of March also suggests that influence needs to be tied to specific behaviors (see J. G. March, "An Introduction to the Theory and Measurement of Influence," *American Political Science Review*, XLIX [1955], 431–51).

[9] It would be desirable also to relate each type of questionnaire measure of influence to objective data about influence processes; such data were not available, however.

SITES

The sites at which the research was done are a manufacturing company and a set of automobile dealerships. In the manufacturing company, there are seven major production departments, each representing a different part of the production process. At the top of the formal authority structure in the production division are the manager in charge of manufacturing and the superintendents of the two separate physical plants. Below them are the heads of the seven production departments. Within each department, there are two or three foremen, and an equal number of group leaders, corresponding to the number of shifts of each department. Each foreman and his group leader, together with the group of employees they supervise, rotate shifts together as a unit. There were eighteen such production groups used in the present analysis.

The second set of data is for thirty-six auto dealerships located primarily in large metropolitan areas scattered throughout the United States. In all dealerships except two, the owner was an active executive. About half (nineteen) of the dealerships had general managers whose responsibility included the new-car as well as the used-car department, service, parts, the office, and sometimes other activities. Every dealership had a new-car sales manager and many also had assistant sales managers or "team captains," as they were sometimes called. At the bottom of the formal authority chain were, of course, the salesmen. The number of new-car salesmen in each dealership varied from four to twenty-six, with the median number being nine.

MEASURES

Manufacturing company.—In the manufacturing company, the major "global" measure of influence at different organizational levels (asked in August, 1958) was as follows:

In general, how much say or influence do you feel each of the following groups *has* on what goes on *in your department?*

a) Top manufacturing
b) Your foreman's supervisor
c) Your foreman
d) Your group leader
e) The employees in your department
f) Your union

[For each level judged, the following fixed alternative responses were provided: "Little or no influence," "Some influence," "Quite a bit of influence," "A great deal of influence," "A very great deal of influence."]

An additional global-type question concerning general influence (asked in December, 1958) was:

How much do you feel that you and the people you work with can influence company policy? *Check one:*

[Five alternatives from "We can influence company policy a great deal," to "We cannot influence company policy at all."]

The measurement of more specific aspects of influence in the manufacturing company is represented by the following questions (December, 1958):

1. When it comes to decisions about who should be selected to be transferred if many have applied for the same job opening, how much say or influence do you feel the persons listed below have on these decisions?
   a) The hourly paid employees
   b) The group leader
   c) The foreman
   d) Higher manufacturing managers
   e) The union executive committee
   f) People in staff departments, such as personnel, industrial engineering, scheduling, etc.

   [For each of the levels being judged, the following fixed alternative responses were provided: "Little or no influence," "Some influence," "Quite a bit of influence," "A great deal of influence," "A very great deal of influence."]

Similar questions were asked concerning the influence of persons at different levels concerning:

2. How pay raises should be granted;
3. How the employees are to be rotated on the different machines, and which employees are to work together on these machines;

4. Changes in the way the shifts are to be set up and rotated;
5. Changes in the methods of work;
6. How much an operator should produce per hour or how all such standards should be determined;
7. Who should be promoted to group leader.

An index score representing the influence of each organizational level, as reported by a respondent, was derived by assigning numerical scores to the degree of influence reported in each specific area and then averaging these scores. These index scores derived from the reports of individual respondents were, in turn, averaged for all persons under the same immediate supervisor.

Auto dealerships.—A global measure of influence distribution within dealerships was obtained from answers to the following question:

In general, how much say or influence do you think each of the following groups or persons *actually have* on what goes on in your department?

a) Owner
b) General manager
c) Sales manager
d) Assistant sales manager or team captains
e) The salesmen as a group.

[For each level being judged, the following fixed alternative responses were provided: "Little or no influence," "Some influence," "Quite a bit of influence," "Great influence," "Very great influence."]

Scores of 1 through 5 were assigned to each response alternative. For each organization level judged, a mean score of influence as perceived by the salesmen was computed.

An index measure of the influence of salesmen was obtained from answers to the following questions asked of salesmen:

In your own work, how much say or influence do you feel you usually have on each of the following types of decisions?

a) Whether to accept a questionable deal
b) The sales techniques you use
c) Whether a certain customer "belongs" to you or to another man
d) Whether a commission will be split between you and another
e) The amount to be offered on a trade-in
f) Your monthly sales quota
g) The bonus plans you work under
h) Your schedule for being on the floor.

[For each of these specific areas, the following fixed alternative responses were provided: "I have little or no influence," "I have some influence," "I have quite a bit of influence," "I have great influence," "I make final decision."]

Scores of 1 to 5 were assigned to each of the response alternatives. An index score for each individual was computed by simply averaging his scores on each of the specific items. An average index score for salesmen in each dealership was then computed. The global influence measure and the specific-item measure correlate +.33 for the 36 dealerships. (It will be noted that the global question refers to influence of the "salesmen as a group" while the specific-item question asks the salesman about "you" personally.)

RESULTS

Agreement within work groups.—If information obtained from organization members is to be most useful, there should be substantial agreement in the information reported. In each of the research settings, the amount of response variance within each work group was computed. The *F*-ratio of variance between groups as compared to variance within groups was also computed. In the manufacturing company these computations are based on eighteen groups of non-supervisory persons, each group reporting to a foreman.[10] The auto dealership data are based on thirty-six groups of salesmen, each group in a different dealership.

Table 1 shows the variance in answers to the global measures of influence as compared to variance in indexes based on specific influence questions. In the manufacturing company, direct comparisons can be made between the two types of influence

[10] The use of eighteen groups does not exactly parallel the global question which referred to influence within each of the (seven) departments; it was felt that there is sufficient variation in actual influence among groups within departments to merit treating them separately for this analysis.

questions as they apply to many organizational levels; in the dealerships, the only direct comparison possible is between alternative measures of non-supervisory (salesmen) influence.

The first column of Table 1 shows that there is, in general, less variance within units on the index measure of influence than on the global measure. This is true in both research settings. Thus in the manufacturing company, the average within-group variance on the influence *index* is 0.57 for non-supervisory employees and 0.74 for group leaders. The average variances on the *globa* questions measuring influence at thes levels are 1.17 and 1.15, respectively. Similar ly, the *index* of salesmen's influence in dea erships has an average variance of 0.43 whil the variance of the *global* question is 1.2! It should be noted, however, that, in th manufacturing company where data ar available concerning perceptions of highe levels of the organization, there is little di ference in the variance in responses to th influence index as compared to the globa question.

TABLE 1

VARIANCE IN RESPONSES OF NON-SUPERVISORY EMPLOYEES TO DIFFERENT TYPES OF INFLUENCE QUESTIONS

	Within-Unit[a] Variance for Responses of Non-supervisory Employees	F-Ratio[b] (Between-Unit Variance/ Within-Unit Variance)
	Manufacturing Company	
Global influence questions:		
Perception of influence exerted by:		
Top manufacturing	1.61	1.03 (N.S.)
Your foreman's supervisor	1.30	1.56 (N.S.)
Your foreman	1.22	1.68 (N.S.)
Your group leader	1.15	1.20 (N.S.)
The employees in your department	1.17	0.86 (N.S.)
Your union	1.31	1.59 (N.S.)
Perception of influence on "company policy by you and the people you work with"	1.16	1.16 (N.S.)
Index of influence in specific areas:		
Perception of influence exerted by:		
Higher manufacturing managers	1.54	3.30 (.001)
The foreman	1.10	2.10 (.05)
The group leader	0.74	2.32 (.01)
The hourly paid employees	0.57	2.33 (.01)
The union executive committee	1.30	1.81 (.05)
People in staff departments	1.62	2.83 (.005)
	Auto Dealerships	
Global influence questions:		
Perception of influence by:		
Owner	1.45	2.43 (.001)
General manager	1.10	1.68 (.05)
Sales manager	1.12	2.71 (.001)
Assistant sales manager or team captain	1.57	1.04 (N.S.)
The salesmen as a group	1.25	1.06 (N.S.)
Index of salesmen's influence in specific areas	0.43	1.31 (N.S.)

[a] *N* of groups in manufacturing company = 18; *N* of auto dealerships = 36.

[b] Numbers in parentheses indicate the significance levels of the *F*-ratios.

Differences among work groups.—Another way of judging the relative usefulness of the two types of influence measures is to see which one *differentiates among work groups* best—that is, which measure shows the highest ratio of between group-to-within group variance (*F*-ratio).

The second column of Table 1 shows that there are, in the manufacturing company, significant differences *among* work groups (*F*-ratios) in how much influence they perceive at each level; but this is so *only* when the index measures of influence are used. When the global influence measures are used, no significant differences among work groups are found. For judgments about influence at the non-supervisory level ("employees in your department") there is, in fact, more variance in judgment within groups than there is variance among groups.[11]

In the auto dealerships, neither the global measure of the influence of "salesmen as a group" nor the index measure of salesmen's influence shows significant differences among dealerships (*F*-ratios of 1.06 and 1.31, respectively). There are, however, significant differences among dealerships on the global measures of influence of the owner, the general manager, and the sales manager. The *F*-ratios for these global measures cannot be compared to parallel index measures,

[11] The fact that all work groups are under a single over-all management and some are in the same departments undoubtedly makes for small actual differences among the influence structures of groups and thus makes it harder for any measure to show differences. Much of the previous work using the "control graph" has been done in organizations where units were geographically separated and fairly autonomous and thus had fairly large actual differences in influence structure.

TABLE 2

VARIANCE IN RESPONSES OF NON-SUPERVISORY PEOPLE TO SPECIFIC INFLUENCE QUESTIONS WITHIN WORK GROUPS OF MANUFACTURING COMPANY[a]

	VARIANCE WITHIN GROUPS FOR JUDGMENTS CONCERNING						MEAN OF VARIANCES
	Higher Manufacturing Managers	Foreman	Group Leader	Hourly Employees	Union Executive Committee	Staff Departs	
Questions concerning influence on:							
Transfers	1.83	1.34	.96	1.08	1.78	1.92	1.49
Raises	1.59	1.56	.87	1.07	1.68	1.90	1.44
Rotation	1.83	1.11	1.09	.79	1.15	1.38	1.23
Shifts	1.97	1.35	.92	.90	1.57	2.42	1.52
Change in work methods	1.26	.94	.80	.72	1.51	2.22	1.24
Production standards	1.21	1.00	.86	.77	1.46	2.16	1.24
Promotion to group leader	1.40	1.13	.86	.80	1.80	2.19	1.36
Mean of variances	1.58	1.20	.91	.88	1.56	2.03	
No. of variances in column smaller than variance of global influence question	4	4	7	7	....	....	
No. of variances in column smaller than variance of influence index	3	2	0	0	1	1	

[a] Analysis is based on eighteen work groups.

since no index measures of influence at these higher levels are available.

Agreement on single specific items.—Thus far the amount of agreement within groups on the *index* of specific influence items as it compares to agreement on global influence measures has been discussed. We look next at the amount of variance on questions concerning *single* specific areas, that is, at the questions that comprise the indexes examined above. Table 2 shows these data for the manufacturing company and Table 3 for the auto dealerships. The following major findings should be noted:

1. The amount of variance in responses to single specific influence items is smaller than the variance in response to global influence questions. This is true for both research sites. In the manufacturing company, it is most strongly true for judgments of influence at one's own (non-supervisory) level and at the next highest level.

2. The amount of variance for single specific influence items is generally larger than for the index based on many of these specific questions. Again, this is true both for the manufacturing company and for the auto dealerships. It indicates that the sheer number of items of the index helps to reduce the variance (though this is not the whole story).

3. There are considerable differences in the amount of response variance depending on which specific area of influence is being judged. These differences appear to be somewhat more marked in the auto dealerships, although the same effect can be clearly seen in the manufacturing company.

4. In the manufacturing company (see Table 2), where more complete data are available for many hierarchical levels, the amount of agreement on specific influence questions seems to be affected more by the differences in the hierarchical level being judged than by the specific influence area being judged.

5. In the manufacturing company the judgments of employees agree most when they are reporting the influence of people on their own level and on the level closest to them (the group leader). As the level being judged becomes organizationally more distant for the respondents, the variance in judgments increases. This is true both when global influence questions are considered (see Table 1) and when specific influence questions are considered (see Table 2). In the auto dealerships, however, this phenomenon of increasing variance of judgment as the "organizational distance" increases, is *not* found.

Agreement between supervisors and subordinates.—We have examined the amount of

TABLE 3

VARIANCE IN RESPONSES OF SALESMEN IN AUTO DEALERSHIPS[a] TO SPECIFIC INFLUENCE QUESTIONS

Questions concerning Influence of Salesmen on	Within Unit Variance	F-Ratio[b]
Accepting questionable deals	0.70	1.61 (.05)
Sales techniques	0.87	1.50 (N.S.)
To whom customer "belongs"	1.22	1.82 (.01)
Commission-splitting	1.44	1.15 (N.S.)
Amount of trade-in	0.71	2.02 (.01)
Sales quota	1.70	1.78 (N.S.)
Bonus plans	0.55	1.10 (N.S.)
"Floor schedules"	0.89	1.34 (N.S.)
No. of variances in column smaller than variance of global influence question	6 of 8	....
No. of variances in column smaller than variance of influence index	0 of 8	....

[a] N of dealerships = 36.

[b] Numbers in parentheses indicate significance level of F-ratio.

agreement in the reports of non-supervisory employees. It is interesting also to examine the amount of agreement between the average perception of each group of employees and the perceptions of its immediate supervisor (Table 4).

The data show, for both research sites, very poor agreement between the perceptions of influence by persons at the two organizational levels. In the manufacturing company, most of the correlation coefficients for judgments at the two levels are actually negative. This is true both when global influence questions are used and when the index measure is used. Only for judgments of

union influence are there positive correlations, one of which (on the global measure) is .65 and statistically significant.

In the auto dealerships, agreement between perceptions of supervisors and subordinates is not much better. In only one instance—responses to a global question about the influence of the assistant sales manager—is there a statistically significant correlation (+.51) between judgments at the two levels.

TABLE 4

CORRELATIONS BETWEEN PERCEPTIONS OF INFLUENCE BY SUPERVISORS AND BY NON-SUPERVISORY PERSONS

	Correlations between Levels	N of Work Units[a]
	Manufacturing Company	
Global measure concerning influence of:		
Top manufacturing	−.28	13
The foreman's supervisor	−.24	13
The foreman	−.31	13
The group leader	−.14	13
The employees in your department	−.11	13
The union[b]	+.65*	13
Index measure concerning influence of:		
Higher manufacturing managers	−.46*	18
The foreman	−.08	18
The group leader	−.04	18
The hourly paid employees	−.03	18
The union executive committee	+.29	18
People in staff departments	−.14	18
	Auto Dealerships	
Global measure concerning influence of:		
Owner	+.18	34
General manager	+.16	23
Sales manager	+.14	26
Assistant sales manager	+.51*	12
Salesmen as a group	−.14	35
Specific items concerning salesmen's influence on:		
Sales quotas	−.06	32
Work schedules	+.08	32
Accepting questionable deals	−.31	32
Bonus plans	+.24	32

* Indicates correlation significant at .05 level (1-tailed test for positive r).

[a] Data on global influence measures for five foremen in manufacturing company were not available. N of dealerships varies mainly because some dealerships do not have one or more of the possible hierarchical positions; also, in several cases the best informed sales supervisor did not answer all questions.

[b] The variation in means of non-supervisory judgments for this measure is extremely small (range = 0.6 on a five-point scale).

Relation of influence measures to morale.—Another test of the usefulness of alternative measures of influence is the extent to which these measures are correlated with other relevant variables. Indeed, as Ebel points out: "Unless a measure is related to other measures, it is scientifically and operationally sterile."[12] From previous research we would expect to find high influence by a group of employees associated with high satisfaction and high productivity within that group.[13]

Several measures of employee satisfaction are available in the manufacturing company. First, there are attendance data, broken down by the number of instances of absence, of lateness, of leaving early, and by total days absent. Second, there are the following questions concerning employees' attitudes toward their foreman and toward the company:

1. All in all, how satisfied are you with your foreman?
   [Five alternatives from "Very satisfied" to "Very dissatisfied"]
2. If you had a chance to get a moderately better paying job working for another company in this area, how would you feel about changing?

[12] R. L. Ebel, "Must All Tests Be Valid?" *American Psychologist*, XVI (October 1961), 645 (see also L. J. Cronbach and P. E. Meehl, "Construct Validity in Psychological Tests," *Psychological Bulletin*, LII [1955], 281–302).

[13] See N. Morse and E. Reimer, "The Experimental Change of a Major Organizational Variable," *Journal of Abnormal and Social Psychology*, LII, No. 1 (1956), 120–29; and V. H. Vroom, *Some Personality Determinants of the Effects of Participation* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1960); Tannenbaum, "Control and Effectiveness in a Voluntary Organization," *op. cit.*; R. Likert, *New Patterns of Management* (New York: McGraw-Hill Book Co., 1961).

[Five alternatives from, "I would strongly prefer to stay at (name of company)" to "I would strongly prefer to change to the other company."]

Since the two preceding questions were asked in both the August and the December questionnaires, they can be related to influence measures obtained at both dates.

Additional index measures of attitude toward the foreman and attitude toward the company were obtained in December and can thus be correlated only with the measures of influence also obtained at that time. The index measure of attitude toward foreman includes items concerning (*a*) whether the foreman is "the kind of man you really feel like working hard for"; (*b*) confidence and trust in the foreman; (*c*) general satisfaction with the foreman; (*d*) desire to have a different foreman. The index measure of attitude toward the company includes items concerning (*a*) over-all attitude toward the company "as a place to work"; (*b*) pride and loyalty toward the company; (*c*) willingness to leave the company for more money; (*d*) whether the employee is working as hard for this company as he would for another.

In the auto dealerships, the following morale measures were used:

1. Turnover of salesmen during the year of the study (1960); a ratio of number of salesmen who left the dealership to number of salesmen at the time of study.
2. An average score in each dealership for salesmen's answers to the questions:
   a) Supposing that you were to stay in the auto sales business, how would you feel about the possibility of moving to another dealership if you got the chance?
   [Five alternatives from "I would definitely prefer moving to another dealership" to "I would definitely prefer staying at this dealership."]
   b) All in all, how satisfied are you with the amount of say or influence which you personally have in the dealership?
   [Four alternatives from "Very satisfied" to "Very dissatisfied: would like more say."]

Table 5 shows the correlation between the two types of influence measures and the various indicators of satisfaction and productivity. In the manufacturing company, neither the global influence measures nor the index measure shows a clear superiority as a predictor of employee morale. Neither type of measure has much relation to attendance. Both global influence measure B and the influence index show fairly strong associations with expressed satisfaction with the foreman and company. The associations with satisfaction with the company are somewhat stronger when global measure B is used than when the influence index is used. However, more limited data using global measure A show little association with expressed satisfaction with foreman or company.

In the auto dealerships, the global influence measure shows a slight over-all superiority as a predictor of morale. While the global measure has a slightly smaller association with turnover than does the index influence measure, it has a markedly stronger negative association with expressed willingness to change dealerships and also a stronger negative association with expressed dissatisfaction with one's own influence.

It might be suggested that the global measure of influence, being more ambiguous, is contaminated more by the degree of satisfaction with influence than is the specific-item measure. To provide data relevant to this question, partial correlations of each of the influence measures with turnover and with willingness to change dealerships were obtained.

Holding satisfaction with influence constant does *not* appreciably change the relation between either influence measure and turnover. Holding satisfaction with influence constant does reduce the correlation between the global influence measure and expressed willingness to change dealerships (from −.39 to −.16, no longer statistically significant). However, this partial association of the global measure with willingness to leave is still more strongly negative than the partial correlation of the index measure with willingness to leave.

Neither type of influence measure has a sizable association with sales performance in

the dealerships. There is, likewise, no appreciable correlation between sales performance of dealerships and either (*a*) a measure of relative influence at various hierarchical levels (slope of influence curve); or (*b*) a measure of the total level of influence in dealerships, obtained by summing the amount of influence reported at each level. These latter two influence measures are based on the global influence question.

SUMMARY AND CONCLUSIONS

Data from both sites show that there is more agreement among persons of the same organizational level when influence is measured by an *index* of questions concerning specific areas of influence than when it is measured by a *global* question. In other words, the index measure is more reliable than the global measure in this respect. However, good agreement is not obtained on

TABLE 5

CORRELATION OF DIFFERENT MEASURES OF NON-SUPERVISORY EMPLOYEES' INFLUENCE WITH MORALE AND PERFORMANCE

A. MANUFACTURING COMPANY (N = 18 WORK GROUPS)

MORALE MEASURES	MEASURES OF NON-SUPERVISORY WORKERS' INFLUENCE		
	Global Measure A "On What Goes on in Your Dept."	Global Measure B "On Company Policy"	Index Based on Specific Influence Areas
Absences[a]—over-all instances	.06	.24	.03
Absences of ½ day or more—instances	.12	.18	.02
Lateness—instances	.16	.35	.20
Left early—instances	−.22	.06	−.22
Absence—total number of days	.05	.07	−.10
Satisfaction with foreman (Measure A)	.17	.66**	.65**
Satisfaction with company (Measure A)	.08	.54*	.42*
Satisfaction with foreman (Measure B)	....	.62**	.65**
Satisfaction with company (Measure B)	....	.71**	.50*

B. AUTO DEALERSHIPS (N = 36 DEALERSHIPS)

	MEASURE OF SALESMEN'S INFLUENCE	
	Global Measure	Index Based on Specific Items
Morale measures:		
Turnover	−.22	−.28*
Willingness to change dealerships	−.39**	−.04
Dissatisfaction with own influence	−.44**	−.29*
Performance measures:		
Sales volume (ratio of actual sales to assigned quota)	+.07	−.04
Change in sales volume (from 1957 to 1960)	+.03	−.05

* Indicates correlation significant at the .05 level (1-tailed test).

** Indicates correlation significant at the .01 level or beyond (1-tailed test).

[a] All absence data are for the second half of 1958.

answers to all specific items. It may be that judgment about some matters is obscured by lack of consistency in the influence relations (e.g., practices about commission-splitting in auto dealerships may vary from one time to another). Judgment in other areas may be difficult for some people because the decision-making process involved may be rather unclear to them (e.g., auto salesmen may not be so sure about how dealership sales quotas are set as they are about how their own "floor schedules" are arranged). The investigator should be careful to ask questions within the clear knowledge of his respondents.

At both sites, agreement among non-supervisory people is better for the index of specific items, as a whole, than for any single specific item. This result suggests that it is the multiplicity of items that gives the index an advantage over the global question, apart from the specificity of the questions. However, the greater number of items of the indexes does not account fully for the greater consensus. At both sites, the mean variance for single specific items is less than for global items when we examine judgments of non-supervisory people concerning influence at their own level (and concerning group leaders at the manufacturing plant).

The data for the manufacturing company suggest that it is easier for people to judge influence at their own level and at levels with which they have frequent contact (group leader and foreman) than to judge levels farther removed (e.g., higher management and staff departments). These results are not duplicated, however, in more limited data for auto dealerships. It may be that the differences in size between the fairly large manufacturing company and each fairly small auto dealership accounts for this difference. In an auto dealership salesmen usually have considerable contact with the general manager and even the owner. Their perceptions of the influence of these top people would be, therefore, based on more information than the perceptions of the factory workers about the influence of, say, top plant managers.

While the data show the index measure of influence to have better reliability at the non-supervisory level than does the global measure, both influence measures show very poor reliability *between* organizational levels. In both sites, there is a striking lack of association between the perceptions of influence by non-supervisory groups and by their supervisors. This result may be due in part to the fact that there is only one judgment for a supervisor in each work group—making these judgments somewhat less reliable than if the responses of a number of supervisors could be averaged. However, it is extremely doubtful if the use of single supervisors adequately explains the total lack of association in the judgments at the two levels. It seems likely that people at different levels have somewhat different information and, moreover, interpret identical information in different ways.

In a previous study of local chapters of the League of Women Voters, Tannenbaum found significant positive correlations between the perceptions of board members and rank-and-file members (+.50, +.18, and +.25 for the global judgments of influence at three organizational levels).[14] He notes that these correlations are relative small and that "the measures are thus subject to some unreliability."[15] The present data indicate that this unreliability may, in some situations, become too great to be tolerated if we wish to have good measures of the objective distribution of influence. It seems likely that the excellent work done on control struc-

[14] Tannenbaum, "Control and Effectiveness in a Voluntary Organization," *op. cit.*

[15] Unpublished data by A. Tannenbaum and C. Smith indicate that member activity and expressed loyalty within Leagues of Women Voters are more highly related to average group perceptions of influence than to individual perceptions of influence (using a global question). One interpretation of these data is that the association between the global influence questions and morale is due to differences in the actual influence structure, even as imperfectly measured.

tures with questionnaire data can be extended more rapidly if other types of data—such as direct observation or analysis of documents—are used to supplement the perceptual data obtained from questionnaires.

That the perceptual data are useful in themselves, regardless of the extent to which they actually reflect influence structure is indicated in our data by the association of both the index and global measures with employee morale.[16] Of the two, the global-type measure is, in general, as useful as the index measure as a predictor of morale among non-supervisory employees. This occurs despite the greater reliability of the index measure at the non-supervisory level. Such a finding may be explained by the freedom that a global question gives the respondent to react to those aspects of the cognitive situation that are most salient to himself; the specific influence questions do not allow respondents this leeway. In other words, the global and index measures may be getting at perceptions of somewhat different aspects of the influence situation.

In addition to this possible cognitive difference between the two types of influence questions, there is also the possibility that the broader, more ambiguous global questions permit responses to be more colored by emotional feeling about the influence structure. The data indicate that the differences between influence measures cannot be attributed to one of the measures being more highly related to satisfaction with influence. However, it is still quite possible that responses to the global question are more colored by the individual's general feeling of status and importance in the organization than are responses to the specific questions of the influence index.

Whatever the explanation, these limited data indicate that a measure of influence based on questions about specific influence areas is a more reliable measure of the influence structure than is a global-type question (at least from the perspective of persons at the same organizational level), while the global-type question is equally good as a predictor of employee morale. Thus where the researcher is attempting to assess actual influence patterns and has to rely on questionnaire data for a rough measure of such patterns, the specific-item measure appears clearly more useful. Where the researcher wishes to get perceptions of the actual influence pattern, then the simpler global measure may be equally useful.

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[16] Previous studies have shown global influence measures to be moderately associated with organizational productivity (see Tannenbaum, "Control and Effectiveness in a Voluntary Organization," *op. cit.*, and Likert, *op. cit.*). For these studies, no index measures of influence are available. In the present data concerning auto dealerships, the lack of relation between either influence measure and sales performance is probably due more to special features of auto dealerships than to poor measurement of influence. These special features include a compensation system based on individual effort, considerable amount of autonomy for the jobs of the lowest levels, and a great variation in technical competence among salesmen. These factors may tend to lessen the impact which salesmen's perceived influence has on their sales performance.

MAKING CAUSAL INFERENCES FOR UNMEASURED VARIABLES FROM CORRELATIONS AMONG INDICATORS[1]

H. M. BLALOCK, JR.

ABSTRACT

A technique is suggested for making causal inferences from correlations among indicators of variables that themselves have not been measured. The procedure involves developing causal models using both the indicators and the underlying variables, but then ending up with predictions that use only the measured variables. By selecting several indicators of each underlying variable, some indicators being causes and others effects of the unmeasured variable, we can infer a spurious relationship between two unmeasured variables even if the variable producing the spurious relationship remains unidentified. It is argued that we should develop causal models even where not all variables are measurable.

It is generally accepted that one cannot make causal inferences directly from empirical data.[2] Nevertheless, it seems to be extremely difficult, if not impossible, to develop purely theoretical arguments in any other terms. True, one can avoid the concepts "cause" and "effect," but he usually then finds himself substituting similar notions, such as "conditions," "consequences," or "functions."[3] The present writer has taken the position that, whatever terminology one wishes to use, it would be advisable to make a clear and explicit distinction between the theoretical language, in which we think, and the operational language, in which actual research hypotheses are tested.[4] Causal arguments belong on the theoretical level, whereas testable hypotheses can only involve covariations.

Empirically, we find relationships among variables, and we control for other variables in order to make theoretical sense out of these relationships. A major purpose of controlling is to make causal inferences of one sort or another.[5] H. A. Simon has introduced a method for making causal inferences from correlational data, provided we are willing to make certain assumptions about outside variables that might possibly operate to disturb the pattern of intercorrelations.[6] The method also gives us a rationale for deciding the conditions under which it may or may not make sense to control for a given variable.

It is always desirable to state one's causal assumptions prior to his interpretation of the results of the application of controls. Ideally, these should include assumptions about the operation of outside variables with possible disturbing influences. Several major objections might be raised concerning the practicality or feasibility of this suggestion. First, one may question whether it is

[1] The writer wishes to thank Richard F. Curtis and Elton F. Jackson for their helpful suggestions.

[2] For an excellent treatment of this general problem see M. R. Cohen and E. Nagel, *An Introduction to Logic and Scientific Method* (New York: Harcourt, Brace & Co., 1934), chap. xiii.

[3] For a witty discussion of the various ways of attempting to dodge the causality issue see R. G. Francis, *The Rhetoric of Science* (Minneapolis: University of Minnesota, 1961), chap. iii.

[4] H. M. Blalock, *Social Statistics* (New York: McGraw-Hill Book Co., 1960), pp. 9–11.

[5] Some other reasons for controlling are to investigate interaction and to specify more exactly the form of a relationship (e.g., the slope).

[6] See his "Spurious Correlation: A Causal Interpretation," *Journal of the American Statistical Association*, XLIX (September, 1954), 467–79. In the application of this method we usually assume linearity and additivity, although theoretically the procedure can be generalized. Simon's method is based on techniques that have been used especially effectively by econometricians (see W. C. Hood and T. C. Koopmans [eds.], *Studies in Econometric Method* [New York: John Wiley & Sons, 1953]). For an excellent discussion of the problem on a more general level see H. Wold, "Causal Inference from Observational Data," *Journal of the Royal Statistical Society*, CXIX (1956), Ser. A, 28–60.

realistic to assume that variables left out of the picture do, in fact, operate as assumed. Such an objection can *always* be raised about any interpretations, however, regardless of whether or not we make our assumptions explicit. We shall not attempt to deal with considerations of this nature in the present paper.

Instead, our central concern will be with a second kind of objection to the use of explicit causal models, namely, that the variables actually measured may not be the ones in which we are primarily interested. For example, the variables being correlated may be demographic factors such as race, sex, income, marital status, chronological age, or, on the group level, murder rates, size of city, geographical region, or the percentage of women in the labor force. Causal thinking, however, may center around such variables as exposure to discrimination, social status, psychological maturity, or anomie. The measured variables may be considered theoretically unimportant in their own right. They are merely taken as indicators of other underlying variables. How, then, can we make causal inferences about the underlying variables on the basis of correlations among these indicators?

OPERATIONAL DEFINITIONS VERSUS INDICATORS

Before attempting to answer the above question, we shall first distinguish between operational definitions and indicators.[7] Having carefully defined what we mean theoretically by a given concept, we may then find it possible to settle on one or more procedures that we believe will enable us to "get at" the variable in question more or less directly. Presumably, we decide this on the basis of what some authors have termed "face validity" or by what Northrop refers to as "epistemic correlation."[8] For example, having defined sex, race, or income in whatever way we pleased, we could undoubtedly find operational procedures for distinguishing between males and females, Negroes and whites, or low- and high-income persons that would be relatively satisfactory to most social scientists. We would also probably be willing to agree that these procedures yielded operational definitions of, say, a female or a Negro, rather than being mere indicators of the underlying concepts. In a crude sense we might say that the variables concerned are "directly measurable."[9]

Suppose, however, that we are dealing with a much more abstract and vaguely defined theoretical concept such as "anomie" or "moral integration." To find an operational procedure with a high degree of face validity would now be much more difficult. One might wish to select certain measured variables, such as suicide or murder rates, as indicators of these more abstract concepts. But he might not wish to claim that anomie can be *defined* operationally in terms of, say, suicide rates.

We are obviously dealing with a question that is in part semantic and that, most certainly, should be treated as a matter of degree. Where the linkage between theoretical concept and operational procedure is in some psychological sense close, we may wish to conceive of the procedure as actually meas-

[7] The writer is distinguishing between a single indicator and a composite index made up of several indicators. It should be pointed out that some writers have objected to the term "operational definition" essentially on the grounds that they would prefer to conceive of "definitions" in a narrower sense of the word. These authors would substitute another concept such as "operational specification" (see Francis, *op. cit.*, p. 10, and H. Orbach, "Operational Definitions and the Natural Science Trend," *Midwest Sociologist*, XIX [May, 1957], 101–3). We must sidestep this particular dispute, which appears to the writer to be largely semantic in nature.

[8] F. S. C. Northrop, *The Logic of the Sciences and the Humanities* (New York: Macmillan Co., 1947), chap. vii. For a discussion of "face validity" see C. Selltiz *et al.*, *Research Methods in Social Relations* (rev. ed.; New York: Henry Holt & Co., 1959), pp. 164–66.

[9] As Northrop points out, strictly speaking we should not use operational definitions and theoretical definitions in the same realm of discourse. Instead, we conceive of some theoretically defined concepts as being linked with operations, whereas others are not. In this sense, *no* theoretically defined concepts are "directly measurable" (Northrop, *op. cit.*, pp. 128–29).

uring the theoretical variable in question. Where the link is much more tenuous, the operational procedure may be thought of as giving us an indicator, but it may also yield a variable (e.g., suicide or murder rates) which may be taken as completely distinct from the underlying variable.

Our reason for differentiating in this way between operational definitions and indicators is that it may help us make the very real intellectual leap from correlations to causal interpretations. On the theoretical level on which we wish to make causal inferences, we shall consider two types of variables: those with which we have associated procedures that we choose to call operational definitions, and those that can be measured only indirectly through indicators. But we shall treat the indicators themselves as distinct variables that must be brought into the causal picture.

To simplify matters, we shall suppose that each indicator variable can be taken as either a direct cause or a direct effect of one and only one of the underlying variables.[10] For example, our causal system might involve the two variables "anomie" and "suicide rates," with the former not being associated with an operational definition, and with the assumption being made that anomie causes suicides, rather than vice versa. It is hoped that our rationale for treating the variables in this rather unusual manner will become apparent as we proceed with a concrete illustration.

We will now have a causal model involving a relatively large number of variables, only some of which will be measured empirically. Can we apply a method such as that suggested by Simon in order to make causal inferences concerning this entire set of variables on the basis of the intercorrelations among only some of these variables? The answer is "yes," provided we have the right combination of causal arrows and provided we restrict ourselves to models involving one-way causation.

Since we will ordinarily be concerned with models consisting of more than four variables, it will not be possible to discuss systematically all of the various combinations. Instead, we shall merely illustrate the kind of procedure that can be used in tackling problems of this nature.

THE GENERAL PROCEDURE

The basic method we shall use is quite simple. It involves postulating a causal model that includes all the variables to be considered, those which have been operationally defined, those which are taken as unmeasured, and those which are to be used as indicators. We then make use of Simon's method to give us a set of predictions, one for each pair of variables that have not been linked directly with a causal arrow. These predictions will involve both measured and unmeasured variables. But under some conditions it may be possible to eliminate algebraically all those variables that have not been measured, leaving us with a set of predictions for only the measured variables. In the present paper it will be impossible to go into the question of the exact conditions under which we can actually eliminate all of these unmeasured variables, since the general case becomes quite complex. Instead, we shall merely illustrate a procedure which can then be applied more generally.

To simplify matters, let us suppose that we have no variables with which operational definitions have been linked. We shall deal with a model in which there are three unmeasured variables and three measured variables, each of which is taken as an indicator of one of the underlying variables. One of the indicators is assumed to cause the unmeasured variable that it represents, whereas the remaining indicators are taken as effects of their respective underlying variables.

[10] When we refer to "direct" causes we mean direct relative only to those variables which have been included in the causal system. We are ruling out the possibility of two-way causation between the indicator and underlying variable; e.g., suicide is both a cause and an effect of anomie. Also, if one indicator happens to be linked directly to more than one underlying variable we will be in difficulty. For example, if suicide is an indicator of both anomie and a high degree of conformity to norms, it will not be easy to disentangle the two underlying variables.

Suppose that we have measures on race (Negro versus white), voting behavior (Democrat verus Republican), and "alienation" (Srole's anomie scale[11]). We may have found, for example, a tendency for Negroes to vote Democratic and to score high on alienation. But we may have a theory involving certain other variables, namely, exposure to discrimination, discontent with the world in its present state, and political liberalism. To oversimplify somewhat, the theoretical argument may be that exposure to discrimination causes a general feeling of discontent, which in turn leads to political liberalism. We realize that other variables may operate to modify this simple causal picture. Ideally, we should bring as many disturbing influences as possible into the causal model as additional variables. But at some point we must stop and assume that outside variables have random effects.[12]

We assume that race causes exposure to discrimination, recognizing, however, that other factors may operate to create variation in the degree to which Negroes face discrimination, meaning that some whites will actually experience more discrimination than some Negroes. Likewise, we assume that a general dissatisfaction with the world (i.e., an attitude, a postulated psychological state) will cause the individual to respond in a certain way to the paper-and-pencil alienation questions.[13] Finally, voting behavior is taken as an indicator of political liberalism, and it is assumed that liberalism is a cause of one's choice of political party rather than vice versa. The six variables can be diagrammed as in Figure 1, where we have placed all of the unmeasured variables on a single line and have boxed in those variables that have actually been measured.

Notice that under the presumed causal model, the biological variable "race" becomes an indirect cause of voting behavior, but the causal connection is very indirect. And the variables which intervene are of much greater interest to sociologists.

Since there are six variables, there will be fifteen possible pairs of variables, each of which may or may not be linked with a direct arrow. There will therefore be a large number of predicted relationships among the various correlation coefficients. But, in fact, most of these predictions will be of no help empirically since X_2, X_3, and X_5 cannot be measured. Actually, there are only three measured variables, and it turns out that Simon's method will give us only one prediction applying to the intercorrelations among these three variables. Let us now see what this will be.

The method involves writing out a set of simultaneous equations, taking each variable in the system as a possible dependent variable. In the most general case involving linear models, in which we allow for the possibility of reciprocal causation, we shall have equations of the form

$$X_i = b_{i1}X_1 + b_{i2}X_2 + \ldots + b_{ik}X_k + e_i ,$$

where e_i represents the effects on X_i of all variables that have not been explicitly included.

In any particular case, however, certain of the b's can be taken as zero, meaning that there is no *direct* effect of some of the variables on X_i. For example, if $b_{i1} = 0$, this means that X_1 is not a direct cause of X_i. In instances involving one-way causation, such as in Figure 1, we may use ordinary least squares procedures to give us unbiased

[11] Leo Srole, "Social Integration and Certain Corollaries: An Exploratory Study," *American Sociological Review*, XXI (December, 1956), 709–16.

[12] See Simon, *op. cit.*, for a more exact statement of what we mean by random effects. The method involves setting up linear simultaneous equations, each with an error term. We then assume these errors to be uncorrelated. As Wold (*op. cit.*) points out, such an assumption is much less plausible when we are dealing with observational data than when randomization has been possible.

[13] This is an example of the general problem in which we assume some postulated internal psychological states that cause certain responses that can be measured. One might choose to call the response an operational definition of the underlying attitude, but we prefer not to do this. If one were willing to refer to alienation responses as an operational definition of discontent, then he could do away with one of the six variables in the causal system.

estimates of regression coefficients.[14] In Figure 1 all but five of the possible arrows have been erased, leaving us with some very simple results. With more complex models the work required to obtain a set of predictions would be much more cumbersome, though not nearly so tedious as the algebraic method discussed in Simon's original paper.[15]

As pointed out in a previous paper, for one-way causative models the method yields a prediction for each pair of variables which have not been connected by a causal arrow.[16] Each of these predictions involves the disappearance of some partial correlation coefficient. For example, since X_1 and X_4 have not been connected by an arrow, the partial $r_{14.23}$ can be expected to vanish. Furthermore, it can be shown that in the case of the simple causal chain $X_1 \rightarrow X_2 \rightarrow X_3 \rightarrow X_4$, we need control for only one of the two intervening variables in order to have the relationship between X_1 and X_4 disappear.[17] In particular, we should expect that $r_{14.3} = 0$, or equivalently that $r_{14} = r_{13}r_{34}$. Similarly, it can be shown that the model also predicts that $r_{46.3}$ and $r_{16.3}$ should both be approximately zero, except for sampling errors.
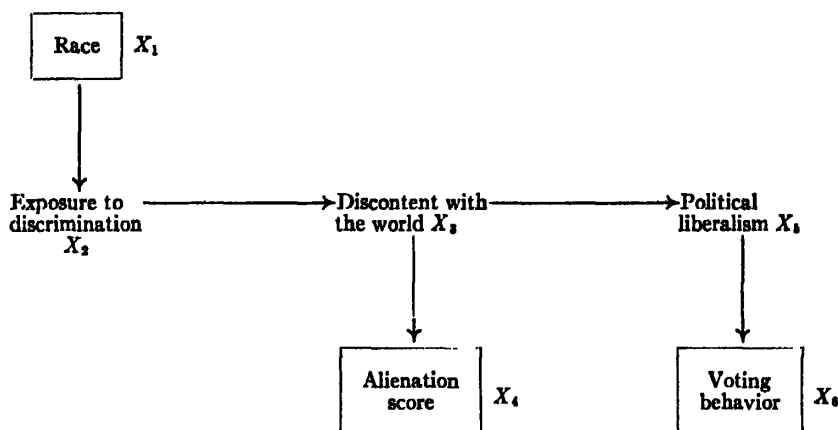
Fig. 1.—Causal model involving three unmeasured variables and three indicators

This gives us the three predictions

$$r_{14} = r_{13}r_{34} \quad (1)$$

$$r_{46} = r_{34}r_{36} \quad (2)$$

$$r_{16} = r_{13}r_{36} \quad (3)$$

We must now eliminate the unmeasured variable X_3. Multiplying equations (1) and (2) we get

$$r_{14}r_{46} = r_{34}^2 r_{13}r_{36} .$$

[14] See H. Wold and L. Jureen, *Demand Analysis* (New York: John Wiley & Sons, 1953), chap. ii.

[15] For those readers who wish to derive the predictions given below in eq. (1)-(3) using the algebraic techniques suggested by Simon, we note that there are six original equations with a potential of thirty off-diagonal coefficients. When all possible pairs of equations are multiplied and expected values taken to eliminate the error terms, we are left with thirty equations in the coefficients. In the model of Fig. 1 there are only five non-vanishing coefficients, and therefore only five of these equations will be necessary to fix their values. The remaining equations can then be combined into simple equations for testing the non-existence of the other twenty-five coefficients. It should be noted, however, that models involving one-way causation, as well as certain instances of reciprocal causation, can be set up in terms of what Wold refers to as "recursive" systems in which the matrix of regression coefficients is triangular. These recursive systems can be handled much more simply by means of ordinary least squares procedures (see *ibid.*).

[16] H. M. Blalock, "Four-Variable Causal Models and Partial Correlations," *American Journal of Sociology*, LXVIII (September, 1962), 182–94.

[17] *Ibid.*

Taking absolute values and noting that $r_{34}^2 \leq 1$, we see that

$$|r_{14}r_{46}| \leq |r_{13}r_{36}|$$

or

$$|r_{14}r_{46}| \leq |r_{16}|$$

Thus the magnitude of the correlation between race and voting behavior should be greater than the magnitude of the product of the two correlations involving alienation, unless alienation is perfectly correlated with discontent. Furthermore, the weaker the correlation between discontent and the alienation score (i.e., the poorer the indicator of the underlying attitude), the greater the discrepancy between r_{16} and the product $r_{14}r_{46}$.

This particular prediction can be compared with those for other causal models. For example, it can easily be shown that, if the causal arrow between X_3 and X_5 were reversed, neither r_{14} nor r_{46} should ordinarily vanish. But the expected value of r_{16} would now be zero. Similarly if the indicator X_6 were taken as a cause rather than an effect of X_5, then both r_{16} and r_{46} should be zero if X_3 causes X_5, but if X_5 causes X_3, then r_{16} should be zero but r_{46} should not vanish. Needless to say, we are presuming always that the effects of outside variables do not disturb these patterns and that there is little sampling error.

It should be emphasized that there are various alternative causal models that would give the same prediction as the model in Figure 1. In particular, if political liberalism, X_5, caused discontent, X_3, which caused exposure to discrimination, X_2, in turn causing race, X_1, we would also get the prediction that $|r_{14}r_{46}| \leq |r_{16}|$. Such a model would undoubtedly be ruled out on theoretical grounds, however. But more complex models might not be so easily rejected theoretically. As we argued above, for example, a simple reversal of the arrow between X_3 and X_5 would lead us to expect that r_{16} should vanish. But if we were to add a variable Z, which caused both X_1 and X_5, this would no longer be the case. Depending on the signs and magnitudes of the various coefficients, we might very well get the same prediction, under this more complex model, as implied for the model of Figure 1. We must therefore be cautious; whereas a given model can be rejected if it leads to incorrect predictions, it does not follow that correct predictions enable one actually to confirm a theory.

As we have just noted, it may make a considerable difference whether a given indicator is taken as a cause or an effect of the underlying variable. This fact should be kept clearly in mind. But what if we cannot decide whether the indicator should be assumed to be a cause or effect? Suppose, for example, we are making use of a variable, such as geographical region, which obviously stands in a very complex relationship to any particular underlying variable, say "local subculture." While we cannot give a definitive answer to this type of question, we would suggest that, whenever we wish to use a complex variable such as region as either an indicator or a control variable, we are almost certain to run into difficulties interpreting our results theoretically. In other words, it might be wise to confine our indicators to relatively simple ones that can readily be linked theoretically to the underlying concept. Likewise, it would be well to avoid the use of those underlying variables that are conceptually so complex as to make it difficult to decide their causal relationships to whatever variables are to be used as indicators.

Ideally, it would be preferable to have not one but several different predictions in order to evaluate the adequacy of a given causal model. Yet, with only three measured variables we cannot expect much more. A suggestion proposed by Curtis and Jackson would seem to be highly relevant.[18] One might make use of multiple indicators, hopefully finding several indicators for each underlying variable, some of which are causes of the unmeasured variable and some effects. For example, alternative indicators of discontent or political liberalism might be used

[18] R. F. Curtis and E. F. Jackson, "Multiple Indicators in Survey Research," *American Journal of Sociology*, LXVIII (September, 1962), 195–204.

as X_4 and X_6. In each instance, if the presumed causal model of Figure 1 is correct we should get

$$|r_{14}r_{46}| \leq |r_{16}| .$$

As Curtis and Jackson point out, the use of multiple indicators ordinarily provides some insurance against the possibility that outside variables are disturbing the pattern of intercorrelations, since it is relatively unlikely that a single disturbing influence will affect more than several of the indicators. To quote these authors:

In research using only one indicator of each conceptual variable, any uncontrolled factor related to both indicators could produce a spurious association without the knowledge of the researcher. However, the addition of even one indicator may improve the situation. Let us suppose that both I_{i1} (the first indicator of the independent variable, V_i) and I_{d1} (the first indicator of the dependent variable, V_d) are related to a third factor, Z_1, in such a way that the... relationship (between indicators) may be spurious. Z, however, cannot be controlled because no data are available. An extra independent indicator, I_{i2}, is then introduced. As noted above, I_{i2} will differ from I_{i1} in some ways, even though they are both representatives of the conceptual independent variable. Indeed, for our present purposes, the more difference between I_{i1} and I_{i2}, the better, for this makes it more likely that I_{i2} is *not* related to Z_1, even though I_{i1} is. If this is the case, and if *both* I_i's are associated with I_d, then *the results cannot be interpreted as due to the spurious influence of* Z *alone*. This is true whether the researcher knows of the existence of Z or not.[19]

INDICATORS AS CAUSES AND EFFECTS

We have just seen illustratively that it makes a difference whether an indicator is taken as a cause or an effect of the underlying variable. In practice it may be difficult to obtain more than one indicator or to find two indicators, one of which is a cause and the other an effect. Nevertheless, it will be instructive to discuss somewhat more systematically the kinds of inferences that can be made whenever one has available both types of indicators. In particular, we shall see that the indicators can then be used to make inferences as to the direction of causality among the underlying variables.

FIG. 2.—Causal model for determining direction of causality between two unmeasured variables, X and Y

Let us modify our illustrative example somewhat, as indicated in Figure 2. We shall again take exposure to discrimination (now X) and discontent with the world (now Y) as two unmeasured variables. Suppose the direction of causality between these two variables is in doubt, as indicated by the dashed line. We shall use four measured variables, two of which are new (income, and discrepancy between stated aspirations and actual achievement). Race (now a) and discrepancy (b) are taken as causes of the two unmeasured variables, but they are assumed unrelated causally to each other. Income level (c) is assumed to be affected by ex-

[19] *Ibid.* Italics in original.

posure to discrimination but not directly by any of the remaining variables. The alienation score (now d) is again taken as an effect of discontent.

Consider the three alternative causal models represented in Figure 3. The various correlations of the indicators with X and Y will, of course, not be obtainable, but we can compute all of the intercorrelations among indicators. By an examination of the patterns of relationships among the four measured variables, it will be possible to choose among these models, enabling us to infer direction of causality and even to infer whether or not the relationship between the

FIG. 3.—Three alternative causal models involving unmeasured variables, X and Y

two unmeasured variables is spurious and due to some other unmeasured variable Z.

It can be shown from Simon's method that for Model I, and for Models II and III as well, we should have $r_{ab} = 0$, whereas the values of r_{ac}, r_{bd}, and r_{cd} should *not* ordinarily be zero, subject of course to sampling errors. In other words, the predictions involving these particular four correlations do not enable us to distinguish among the three causal models. But Model I also predicts that r_{bc} should be zero, whereas r_{ad} should not vanish. The reason that r_{bc} should disappear is that b is operating on Y completely independently of X and its indicators. On the other hand, there is a causal chain between a and d by way of X and Y.

If we now look at Model II we see that we should have the reverse set of predi tions for these two coefficients; r_{ad} shoul vanish, whereas r_{bc} should not. Finally, i Model III in which X and Y are spuriousl related through Z, *both* r_{ad} and r_{bc} should b zero. We thus have an empirical method fo choosing among the three causal models.

It should be noted that we cannot di tinguish Model I from the situation in whic $X \rightarrow Z \rightarrow Y$, nor can we distinguish Mod II from a model in which $Y \rightarrow Z \rightarrow X$. T do so, we would need indicators for Z a well as X and Y.[20] Obviously, then, or would have to decide what Z is. In both these instances, however, Z is operating a an intervening variable between X and Y While it might be helpful to locate a numbe of such Z's in order to interpret the relatior ship between X and Y, our failure to fin such a Z might not create too many seriou theoretical problems.

The possibility that the relationship be tween X and Y is spurious, as in the case c Model III, poses a much more serious the oretical problem unless we can be assured c finding the proper Z. But notice that unde Model III the values of r_{bc} and r_{ad} should b zero for *any Z* operating in this particula causal manner. *We thus do not even have t know what Z is or to try out a specific set c*

[20] Note that if we had two indicators of Z, or a cause and one an effect, we could proceed in th same manner as in Models I and II.

possible Z's in order to make the inference that the relationship between X and Y is spurious. This is, to the present writer at least, a surprising result.

Several words of caution need to be introduced, however, in order to avoid jumping to conclusions. First, the mere fact that r_{bc} and r_{ad} are both zero should be interpreted in the light of the possibility that there may be sampling errors and also that several of the indicators might be only weakly related to the underlying variables. Another reason for a low correlation is that there may be very little variation in one or more of the indicators. If we recall, however, that Model III (as well as Models I and II) also predicts that r_{ac}, r_{bd}, and r_{ad} should *not* be zero, we have an additional check on the adequacy of the inference that the relationship between X and Y is spurious. If all of these three correlations are high and if both r_{bc} and r_{ad} are near zero, then we have reasonably good evidence of a spurious relationship even when Z is unknown or unmeasurable.

Second, we must again caution the reader that any inferences we make depend upon the adequacy of the causal model in other respects. For example, we are assuming no relationship between Z and either a or b, and this may be unrealistic. Likewise, there should be no direct causal links among the indicator variables. We are supposing outside variables to have no distorting effects. As we indicated earlier, the suggestion made by Curtis and Jackson regarding the use of multiple indicators is quite relevant. The more indicators we have in the positions of variables a, b, c, and d, the more plausible become our assumptions about variables having possible distorting effects.

We have already noted that Simon's method requires the assumption that all relationships are linear and hence additive in nature. In view of the fact that we are dealing with variables that are taken as unmeasured, how can such an assumption be justified? Had we been in a position to measure each variable, linearity could be investigated empirically and, if necessary, certain mathematical transformations (e.g., logarithmic) could be made in order to assure approximate linearity.[21] If all relationships among measured variables turn out to be linear, and if these variables are linked together only indirectly through unmeasured variables, then it might be plausible to argue that relationships with the underlying variables would also be linear if these latter variables could in fact be measured. But this is obviously a mere simplifying assumption that cannot be tested.

It appears as though we must pay a certain price for the use of unmeasured variables, and assumptions about the forms of relationships are among these. We should point out, however, that methods such as factor analysis and latent structure analysis, which also enable one to make inferences about unmeasured variables, likewise require the use of restrictive assumptions. In particular, factor analysis ordinarily makes use of linear models and can actually be considered a special case of the type of analysis under consideration in the present paper.[22]

In general, the more complex our causal models, the simpler our assumptions have to be about how these variables are interrelated. When we introduce further complications in the form of unmeasured variables, we must be even more cautious in making our interpretations. Although it is perhaps not obvious from the rather simple illustrations used above, if there are a relatively

[21] An alternative is to restrict the range of variation so that a linear approximation is reasonably valid. This will usually be possible in the case of the measured variables but leaves unanswered the question of how to conceive of restricted variation in variables which cannot be measured.

[22] In factor analysis we make use of causal models in which there are no direct causal links among any of the measured variables. Intercorrelations among these variables are supposedly due to the unmeasured variables or factors which, when controlled, would leave us with vanishing partials among the measured variables. Thus factor analysis deals with a very specialized type of causal model involving unmeasured variables. If used in situations in which some of the measured variables are directly linked to each other, rather than only indirectly through the unmeasured factors, factor analysis can produce results that may be highly misleading. See, e.g., studies involving factor analyses on census data.

large number of unmeasured variables as compared with those that have been measured, there will either be too many unknowns for a definite solution, or the computations will become quite tedious. We would therefore recommend that the relative number of such unmeasured variables be kept to a minimum. Unless a theory involving a high proportion of unmeasured variables is either logically rigorous or capable of yielding precise predictions, it will ordinarily prove difficult to subject it to crucial tests. There will be few, if any, predictions that could not also be made by alternative theories, which may possess the added virtue of greater simplicity.

CONCLUDING REMARKS

We have suggested a technique for making causal inferences from correlations among indicators of variables which themselves have not been measured. Basically, the procedure involves developing causal models using both the indicators and the underlying variables, but then ending up with predictions that use only the measured variables.

There can be little disagreement that in sociological theory we have made use of numerous concepts—such as anomie, functional integration, institutionalization, cohesiveness, secularization, and disorganization—that have either been only vaguely defined or that do not seem to lie close to the operational level. These are the "big words" sociologists are often accused of using. Sometimes they are thrown into theoretical discussions with almost reckless abandon. In other instances they may be rather vaguely linked with measured indicators that are referred to as "correlates," "manifestations," or "symptoms" of the underlying variables. Sometimes the "big words" are utilized to provide explanations for empirical relationships, but upon detailed logical investigation we find that these theoretical explanations make much less sense than we had supposed. At least if this is an unfair characterization of the state of contemporary sociological theory, it is often made by the layman.

One possibility is to do away with the big words completely and to make sure that every concept we use theoretically is defined with sufficient precision that we can readily associate specific operations with it. Another is to retain whatever vaguely defined concepts we may think will ultimately prove useful, while at the same time attempting to spell out exactly how we might link these theoretical concepts with specific measured variables. In particular, we might attempt to develop causal models that relate both types of variables. In so doing, many of these vaguely defined concepts should become clarified. For example, it may be decided to take what appeared to be a single variable (e.g., social class) and to distinguish among several different dimensions of the variable, associating each dimension with a particular indicator.

To the present writer, it seems noteworthy that, in spite of the numerous discussions of theory-building found in the sociological literature, we have had few if any serious attempts to develop causal models involving more than three or four variables.[23] It may, of course, be argued that sociological theory is not yet at a stage where this is possible. But by the same token, it has often been claimed by the layman that sociology has not yet gone very far beyond "common-sense" explanations.

We would suggest that if a larger number of explicit attempts were made to use multivariate causal models, even where not all the variables are as yet measurable, we might be in a position to take advantage of those mathematical and logical techniques that are being used so effectively in a field such as economics. In so doing, we might be more able to advance beyond common sense, while at the same time clarifying our basic concepts and developing a body of specific propositions relating these variables in the complex manner in which they appear to be related in real-life situations.

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[23] For a significant exception see S. H. Udy, Jr., "Technical and Institutional Factors in Production Organization: A Preliminary Model," *American Journal of Sociology*, LXVII (November, 1961), 247–54.

MEASURING A NATION'S PRESTIGE

MICHIYA SHIMBORI, HIDEO IKEDA, TSUYOSHI ISHIDA, AND MOTÔ KONDÔ

ABSTRACT

In order to ascertain the structure of the international prestige system and the relative importance attributed to each of eight determinants of a nation's prestige, questionnaires and a card-selecting system were used with about nine hundred senior high school and college students in Japan. Nations are, in the consciousness of all the subjects, ranked in terms of their status in the international prestige system, typically in three to five classes. The relative importance of eight determinants for ranking a nation's prestige was computed by means of two methods, a ranking method and the method of paired comparison. Since the correlation between the results obtained by the two methods is high and no marked difference between various subgroups was found, the scores were weighted by the results obtained from the method of paired comparison. Thus a tentative nation's prestige index is composed as follows: economic 7, cultural 5, political 5, international 5, physical 4, national character 4, military 4, attitude 2.

INTRODUCTION

One of the main sources of conflict and tension in the contemporary world is nationalism of a certain kind. Any treat to sovereignty, any defeat in international competition, is a blow to a nation's self-respect. This is especially true for the leading nations; but even the developing nations are sensitive about their position relative to a rival or model country. In these instances "natiocentrism," instead of ethnocentrism, is operative. It is ironic that nationalism has grown while physical distances have become shorter.[1]

As one approach to a sociology of nationalism, we took the status of nations in the international prestige hierarchy as a focus of investigation. Our previous research revealed that, although prestige is attributed to a nation when it is evaluated as a whole, it has eight determinants: physical (geographical and demographic); economic; internal political; military; artistic and scientific; international relational aspects; national character; and attitude (i.e., rater's like-dislike) toward the nation.[2] Our aims in this paper are to ascertain the structure of the international prestige system and to estimate the relative weight of the eight factors in determining a nation's prestige. As subjects we used some nine hundred senior high school and college students in a Japanese community. Two main techniques of research were used, questionnaire and card selection.

THE INTERNATIONAL PRESTIGE SYSTEM

The prestige of one nation in the eyes of people in another nation is subjective and difficult to quantify. We have tried to deal only with the more tangible aspects of national prestige. Our terminology of "first-class," "second-class" and so on as indicators of the nation's prestige is interchangeable with the terms upper-upper, lower-upper, middle, upper-lower, and lower-lower class nations, used to rate social classes.[3] Our terms are in general use in Japan. We can ask two questions. In how many classes are all nations ranked in terms of prestige? And how many nations are covered in each class?

[1] H. C. Duijker and N. H. Frijda, *National Character and National Stereotypes*, Vol. I: *Confluence* (Amsterdam: North-Holland Publishing Co., 1960).

[2] The findings of our research into nations' prestige for about a thousand pupils in primary and lower secondary schools are reported in our "How the U.S.A. and the U.S.S.R. Look to the Children in a Japanese Community," *Elementary School Journal*, LXII (January, 1962), 181–88; see also "An Attempt To Construct a National Prestige Index," *Indian Journal of Social Research*, III (1961), No. 1, 25–36, in which our theoretical frame of reference is presented.

[3] Cf. William Lloyd Warner *et al.*, *Social Class in America* (Gloucester, Mass.: Peter Smith, Publisher, 1957).

Insofar as a nation's prestige is originally a sociopsychological phenomenon that reflects an evaluation of the relationships of one nation to one or more others, its analysis requires an international perspective. One nation's prestige should be treated in the framework of the international prestige system, which is a structural sum of the prestige given to all the nations in the world by each other.

TABLE 1

PERCENTAGE OF RESPONDENTS DESIGNATING VARYING NUMBERS OF NATIONAL PRESTIGE CATEGORIES

No. of Categories	Per Cent
1	0
2	6
3	30
4	18
5	29
6	2
7–10	2
11 or more	1
Don't know	12
Total	100

INTERNATIONAL STRATIFICATION

For the first question, using questionnaires and interviews, the subjects were asked to respond to the following statements: "We usually say, as you know, 'some nation is a first-class or a second-class or a third-class nation,' from the viewpoint of ranking the nation as a whole. Then into how many classes or groups do you think you can generally classify all the nations in the world?" No one rejected such classification of nations, and no one said all nations are in the same class (Table 1). Nearly all of the subjects said that nations might be classified into three, four, or five classes. More important than the variation in the criteria for classification is the fact that there are prestige classes among nations in the minds of our subjects. Moreover, it would seem that the more the subjects know about the nations, the more distinctively they could classify them. Of course, the distinction made will vary with person classifying and the factors that he relies upon. In the following discussion the nations will be divided into five classes.

Let us now consider the second question: How many nations are in each class? Here some subjects, through the use of check lists, were required to specify the number of the nations in each class when all the nations are supposed to be divided into five strata. Another thirty subjects were asked to classify 119 cards with the proper name of a nation into five groups, identifying each name with one of the five classes according to the same principle of classification as the check list. Only the findings by the card-selecting method are shown (Table 2).

The distribution was distinctly pyramidal; first-class nations are the smallest in number and fifth-class ones most numerous. On the other hand, while the questionnaire replies also indicated that first-class nations are the fewest in number, third-class nations were most numerous; the order of frequency being third, fourth, second, fifth, and first. The difference between the two sets of ratings may reflect a difference between the structure of the international prestige hierarchy in our consciousness and in our concrete identification. Generally, the subjects tend to rank the well-known nations as higher and define them as a clear-cut class, while they are inclined to rank less well-known nations lower and put them together into larger categories.

TABLE 2

NUMBER AND PERCENTAGE OF NATIONS ASSIGNED TO EACH OF FIVE PRESTIGE CLASSES

Prestige Class	No.	Per Cent
1	6	5
2	12	10
3	27	23
4	32	27
5	42	35
Total	119	100

NATIONAL STATUS

Since all nations can be placed somewhere in the structure of international prestige, we are led to ask another question: What is the status of each of the main countries in the world from the point of view of national prestige?

In our questionnaires the subjects were asked to answer the question, "What kinds of nations do you think would be classified into the first-class, the second-class, and so on, when the nations are divided into five groups? Name the nations that you can identify with these criteria." In the card-selecting method, subjects were required to classify the proper names of nations into five classes in the international prestige hierarchy. The two methods produced similar results.

First-class nations: U.S.A., U.S.S.R., U.K., France
Second-class nations: Japan, West Germany, Red China, Italy, Canada, Switzerland, Sweden, Norway, Australia, East Germany
Third-class nations: India, Philippines, Burma, Indonesia, Iran, China, Yugoslavia, Portugal, Poland, Belgium, Finland, Hungary, Denmark, Czechoslovakia, Spain, Greece, Netherlands, Austria, New Zealand, Brazil, Chile, Argentina, Mexico, Egypt

Other nations were classified as fourth- or fifth-class nations. European countries are generally placed higher, although some non-European countries are also thus ranked. It may be interesting to add that the United States and the U.S.S.R. monopolize the nomination for the highest-prestige countries: 99 and 85 per cent, respectively.

In this context we must mention some peculiar cases. We find that some subjects judge a nation's prestige only in terms of the camp to which it belongs or its socio-political regime; they tend to accentuate and exaggerate certain factors in ranking a nation, using some simple and subjectively important aspect rather than thinking in terms of the nation as a whole. For instance, one subject nominated as first-class nations those of communism, as second-class nations those of neutralism, as third-class nations those of less neutralism than the above, and as fourth and fifth those of capitalism. Such responses, although uncommon, merit special attention.

Whether a people rank a nation as higher or lower may depend partly upon the relationship of the people to the nation. This is connected directly with the status that they give to their own nation in the international prestige system. Therefore, the status of the subject's nation must be considered when evaluating his ranking of other nations.

TABLE 3

PERCENTAGE DISTRIBUTION OF PRESTIGE ASSIGNMENTS FOR JAPAN

Prestige Class	Per Cent
1	5
2	47
3	36
4	4
5	2
Don't know	6
Total	100

How does Japan look to the Japanese, our subjects?[4] They were asked, "In what class do you think Japan will be ranked when the nations are divided into five groups from first-class through fifth-class nations?" Eighty-three per cent of the 878 respondents ranked Japan as a second- or a third-class nation, and she may therefore be placed between these (Table 3). (By the card method the nominations of Japan as a second-class nation totaled 51 per cent.)

Further consideration of national prestige in the international prestige system must deal with its determinants.

A NATION'S PRESTIGE INDEX

WEIGHTING THE DETERMINANTS OF A NATION'S PRESTIGE

As explained in the introductory remarks, we concluded from our preceding survey

[4] Cf. Jean Stoetzel, *Without the Chrysanthemum and the Sword: A Study of the Attitudes of Youth in Post-war Japan* (London: William Heinemann Ltd., 1955), chap. v.

that eight factors "determine" a nation's rank in the international prestige hierarchy. Our present purpose is to discover the differential quantitative weights of these factors and to formulate a nation's prestige index. To do this, two techniques were used: a ranking method and a paired comparison. The former technique requires that the subjects arrange the determinants in order of importance for their own ranking of the nations; the latter asked subjects to check which factor whey thought more important for twenty-eight paired combinations of the eight determinants.

TABLE 4

RANK-ORDER CORRELATIONS BETWEEN SUBCLASSES OF THE SUBJECTS

Subclasses	Correlation
Senior high boys v. girls	.64
College and university boys v. girls	.79
N-Senior high v. P-senior high	.64
Senior high v. college and university	.86
Junior college v. 4-year university	.96
Arts v. science course	.86

In ranking the importance of the factors, about eighty-five subjects failed to assign a rank to each factor. The "military" and "attitude" factors (as listed in the introduction above) were the only ones put last by any appreciable number of raters, and each was put last by about a fourth of all subjects. On the other hand, nearly half the subjects assigned the economic factor to first rank, while only an eighth or less put any other factor in first place. Only one person thought his subjective liking for a nation was a factor in its prestige.

Summing the weighted ranks (the highest being weighted 8) and dividing by number of respondents give the mean rank of each factor. These were as follows: economic 7.0, internal-political 5.4, international relations 5.2, cultural 4.4, physical 4.2, national character 3.9, military 3.6, and attitude 2.4.

RANK-ORDER CORRELATIONS BETWEEN DIFFERENT SUBCLASSES OF THE SUBJECTS

Before determining a nation's prestige index, it is necessary to test possible differences of mean scores for the eight determinants among different subclasses of students in order to determine whether we must find different indexes for subpopulations or whether one index can be used for all subjects.

We classified our subjects by sex, school, and course of study (for college students). It is generally believed that the national senior high school in this survey attracts boys and girls of much higher intelligence and social class than the private girls' senior high school, and that the four-year university selects cleverer students than the girls' junior colleges. The students in the arts courses are presumed to be more internationally minded than those in science and technology.

Thus we obtained six pairs (or twelve subclasses). The rank-order correlation coefficients were computed by Kendall's *t* for the six pairs, the results of which are presented in Table 4.[5] These correlation coefficients range from .64 to .96, so that it can be concluded that the assessment of the factors is similar in most of the subgroups.

A NATION'S PRESTIGE INDEX

We then applied the second method, paired comparison, which is generally recognized as a more accurate tool for scaling various factors.[6] Since there are eight determinants for a nation's prestige, we can make twenty-eight pairs. A random sampling method was used to pick the two determinants for pairing and to arrange the twenty-eight pairs in our questionnaire.

In Table 5, for example, the number in the first column of the second row (467) shows the total number of respondents who judged the economic factor to be more im-

[5] M. G. Kendall, *Rank Correlation Methods* (London: Charles Griffin & Co., 1948), p. 83. For similar research see, e.g., Edgar H. Schein and J. Steven Ott, "The Legitimacy of Organizationl Influence," *American Journal of Sociology*, LXVII (May, 1962), 682–89.

[6] Gardner Lindzey (ed.), *Handbook of Social Psychology* (Cambridge, Mass.: Addison-Wesley Publishing Co., 1954), pp. 345–47.

portant than the political factor. After completing this table in this way, all the numbers in each cell were divided by the total response, 595. The numbers in each cell were then transformed by Z-transformation in accordance with the hypothesis of normal distribution. Then these numbers were totaled in each column. When the total numbers are divided by the number of filled

TABLE 5

NUMBER OF RESPONSES BASED ON THE METHOD OF PAIRED COMPARISONS*

	Economic	Political	International	Cultural	Physical	National Character	Military	Attitude
Economic	....	128	135	112	97	86	75	67
Political	467	....	266	235	210	173	175	85
International	460	329	....	258	223	168	196	86
Cultural	483	360	337	....	255	197	256	134
Physical	498	385	372	340	....	276	273	157
National character	509	422	427	398	319	....	310	109
Military	520	420	399	330	322	285	....	214
Attitude	528	510	509	461	438	486	381	....
Total	3465	2554	2445	2134	1864	1671	1666	852

* The respondents who do not check all the items perfectly are omitted from this analysis, so that the total number is considerably smaller than in the other method.

cells (7) for each column in the table, we arrive at a scale value for each determinant (Table 6).

The scale values obtained by Z-transformation can be transformed by linear transformation. In order to distribute the numbers in Table 6 within the range of 6.96 and 2.41 (the highest and the lowest scores obtained by the ranking method), we used the linear equation $Y = AX + B$. (X represents the result of the paired comparisons and A and B are definite coefficients.) To get coefficients A and B, numerical resolution was tried between each scale value (X) and each mean score obtained by the ranking method (Y) in one-to-one correspondence. Then the mean for A is 3.02 and for B is 4.53. Using these coefficients, linear transformation is possible and thus new scale values are gained. The results presented in Table 6 may be termed the weighted values of national prestige determinants.

TABLE 6

SCALE VALUE AND WEIGHTING OF FACTORS IN NATIONAL PRESTIGE INDEX

Prestige Determinants	Scale Value	Weight
Economic	0.85	7.10
Political	0.26	5.32
International	0.21	5.16
Cultural	0.03	4.62
Physical	−0.13	4.14
National character	−0.23	3.84
Military	−0.25	3.77
Attitude	−0.74	2.30

When these values are rounded, we get numerical values for each determinant that are very similar to those obtained by the ranking method: namely, economic 7, political 5, international 5, artistic and scientific 4, physical 4, national character 4, military 4, and attitude 2. The correlation between the results from the ranking method and those from the paired comparisons is .996. Therefore, we propose tentatively to represent the figures just mentioned as the components of the national prestige index.

CONCLUSION

With the purpose of exploring one aspect of the sociology of nationalism, we have tried to analyze how a nation's prestige is

determined in an international prestige system. Nations are evaluated on eight factors and ranked in terms of their composite score. The degree of importance attributed to each factor varies. By finding statistically their relative importance, we have constructed what we call a national prestige index. Needless to say, the prestige of nations is always in flux, dependent upon time, place, and person, so that the findings of our previous and present surveys, which were conducted within a year's time and in the same community, differ from each other, although the subjects were of different ages. Therefore the present index is tentative, to be elaborated and verified. Nevertheless, our belief is that a nation's prestige or rank can be studied objectively, and that further studies, especially for comparable subjects in different countries, will throw some light on socio-psychological aspects of contemporary global tensions through cumulative and comparative data of the same kind.

HIROSHIMA UNIVERSITY

LETTERS TO THE EDITOR

DAVIS, "STRUCTURAL BALANCE, MECHANICAL SOLIDARITY, AND INTERPERSONAL RELATIONS"

March 14, 1963

To the Editor:

James A. Davis (*American Journal of Sociology*, LXVIII [January, 1963], pp. 444–62) has performed a useful service to sociologists by acquainting many of them with Heider's theory of balance and its relevance to sociological problems. However, there are two major points at which the Davis article is likely to create some confusion with regard to the applicability and generality of the *P-O-X* model.

First of all, Davis does not deal with the problem of "common relevance" (see Theodore M. Newcomb, *The Acquaintance Process* [New York: Holt, Rinehart, & Winston, 1961]). To apply the *P-O-X* model to any empirical situation, it is necessary that the object (*X*) be of perceived common relevance to the actor(s). Davis' example of Nixon and Kennedy vying for the presidency is an excellent example of the confusion that may result from lack of consideration of this point. He states that the two candidates both "liked the presidency," but that this did not promote "liking" between them as balance theory would predict. Now while I am certain that Nixon and Kennedy agreed on liking the position of President, this is not the point—either for balance-theory predictions or for interaction between Nixon and Kennedy. The (cognitive) objects of *common relevance* are "Nixon-as-President" and "Kennedy-as-President." With regard to these objects, the attitudes of the two candidates were quite disparate.

Second (and related to the issue of common relevance), Davis, like many others who have worked with the balance-theory model, gives little explicit consideration to the influence of *norms* and to the possible utility of balance theory in *accounting for the development and necessity of norms* in situations of potential imbalance. I propose that "norms of imbalance"—norms specifying that an object involved in interpersonal interaction is *not* to be regarded as of common relevance—arise in situations of potential imbalance to insure continuation of the particular social relationship without disruptive conflict, when such continuation is necessary for the "survival" of the social system. Let us consider several examples of norms of imbalance:

1. Radcliffe-Brown's classic analysis of avoidance taboos demonstrates that a norm of imbalance arises in a situation in which disruptive conflict might exist between a *P* (husband) and an *O* (mother-in-law) if an *X* (wife) were regarded as of common relevance (subject to the authority of both simultaneously). Since potential relationships among the three would be so diffuse, the norm (in the extreme case) prohibits all interaction between *P* and *O*, that is, it specifies that *no* objects are to be regarded as of *common relevance.*

2. In the exchange relationship the disparity in *P*'s and *O*'s attitudes toward their respective profits or losses is recognized by both actors; but, even if one of them is a heavy loser, a norm of imbalance proscribes hostility.

3. In sports and other zero-sum games, one finds a norm of "good sportsmanship" that explicitly prescribes friendly feelings on the part of the loser.

4. Agreements to disagree without hostility are quite common in many interaction situations.

In short, there is nothing in balance theory to justify the conclusion that the ex-

change relationship must be one in which there is "some gain in exchange and high reward from competition to offset the imbalances that occur" (Davis, p. 461). Sanctions associated with socially accepted norms may provide the rewards or costs. Davis' conclusion, with its implicit assumption that mutual gratification must result from the interaction sequence, is not only unnecessary but misleading. Conclusions such as this provide ammunition for those who charge the "consensual" models and theorists ignore conflict and coercion. However, far from ignoring such situations, balance theory calls attention to the (potential) common relevance of certain objects of conflicting orientations and the consequent necessity for *emergent norms of imbalance* to prevent disruptive conflict.

C. NORMAN ALEXANDER, JR.
University of North Carolina

REJOINDER

April 3, 1963

To the Editor:

Dr. Alexander aims to reduce the "confusion" in my article by means of the concepts of "common relevance" and "norms of imbalance." Since he nowhere defines common relevance and then proceeds to define norms of imbalance in terms of common relevance, I find it hard to agree that the confusion has been appreciably reduced. As for the question of whether it is conformity to social norms or personal "profit" that motivates people to continue in imbalanced situations, I would leave this to Professor Homans and Dr. Alexander to settle, although my personal impression is that these are two ways of looking at the same thing. All I suggested was that since people do continue in some imbalanced situations, there must be *something* other than balance that motivates them. While I mentioned profit from exchange and reward from competition as alternative sources of motivation, a reasonably careful reading of the article would show that I did not rule out all others.

JAMES A. DAVIS
University of Chicago

DOCTOR'S DEGREES IN SOCIOLOGY, 1962

According to reports received by the *Journal* from forty-eight departments of sociology in the United States and Canada offering graduate instruction, 185 Doctor's degrees were conferred in the calendar year 1962.

BOSTON UNIVERSITY

Grainger Browning, A.B. Shaw, 1939; M.A. Boston, 1946. "Social Prestige in a Low-Income Housing Community."

Elaine Hagopian, A.B., M.A. Boston, 1954, 1956. "Morocco: A Case Study in the Structural Basis of Social Integration."

Jaroslav G. Moravec, M.A.T., Junior College, Chotebor (Czechoslovakia), 1929; JUD. (Dr. of Law) Charles University (Prague), 1934. "Social Order through Law."

BROWN UNIVERSITY

Harry T. Groat, A.B., M.A. Bowling Green State, 1954, 1958. "College Student Migration: 1887–1958."

UNIVERSITY OF CALIFORNIA (BERKELEY)

Robert Ross Alford, A.B., M.A. California (Berkeley), 1950, 1951. "Class Voting in Four Anglo-American Countries."

Toyo-Masa Fusé, B.A. Missouri Valley College, 1954; M.A. California (Berkeley), 1956. "A Sociological Analysis of Neo-Orthodoxy in American Protestantism: A Study in the Sociology of Religion."

Fred Harry Goldner, M.A. Chicago, 1950. "Industrial Relations and the Organization of Management."

Ritchie Peter Lowry, A.B., M.A. California (Berkeley), 1952, 1954. "'Who Runs This Town?' A Study of the Quality of Public Life in a Changing Small Community."

Stanford Morris Lyman, A.B., M.A. California (Berkeley), 1955, 1957. "The Structure of Chinese Society in Nineteenth-Century America."

UNIVERSITY OF CALIFORNIA (LOS ANGELES)

Gerard A. Brandmeyer, A.B. Fordham, 1954; M.A. Illinois, 1956. "Status Crystallization and Political Behavior: A Replication and Extension Research."

Fred Thalheimer, A.B., M.A. California (Los Angeles), 1956, 1959. "Religious Beliefs and Practices of Academicians."

CATHOLIC UNIVERSITY OF AMERICA

Mary Grace Connolly, B.S.N.E. Catholic, 1949; M.S. Yale, 1951. "Mental Illness and Use of Community Resources."

UNIVERSITY OF CHICAGO

Lathrop Vickery Beale, A.B. Randolph-Macon Woman's College, 1943; M.A. Chicago, 1948. "Religiousness and Integration into the Local Community."

Anita Beltran, A.B., A.M. Philippines, 1954, 1960 "Social Origins and Career Preparation among Filipino Students in American Universities."

Aparecida Gouveia. "Student Teachers in Brazil."

Andrew M. Greeley, St. Mary of the Lake Seminary, 1954; A.M. Chicago, 1961. "Religion and the June, 1961, College Graduate."

Nathaniel Hare, A.B. Langston, 1954; A.M. Chicago, 1957. "Changing Occupational Status of the Negro in the United States."

Sultan Hashmi, A.M. Chicago, 1960. "Trends and Factors in Urban Fertility Differences in the United States."

Mahmud S. Jillani, A.B., A.M. Government College (Lahore), 1953, 1955. "Resettlement Patterns of Displaced Persons in Pakistan."

Philip M. Marcus, A.B. Brooklyn College, 1954; A.M. Chicago, 1959. "Trade-Union Structure: A Study of Formal Organization."

Judah Matras, S.B., A.M. Chicago, 1956, 1957. "Israel: Absorption of Immigrants, Social Mobility, and Social Change."

Maurice Richter, A.B. Bard College, 1953; A.M. Chicago, 1954. "A Study of Cognitive Inconsistency."

Walt P. Risler, A.B. Upsala College, 1948; A.M. Chicago, 1949. "Personal Documentation and Projective Analysis in Family Research: A Study of Parent-linked Delinquency."

Richard Seaton, A.B. Columbia, 1947. "Hunger in Groups."

Alma Ficks Taeuber, A.B. Washington State College, 1954; A.M. Chicago, 1960. "A Comparative Urban Analysis of Negro Residential Succession."

UNIVERSITY OF COLORADO

Charles Howard Gray, A.B. Denver, 1953. "Coalition, Consensus, and Conflict in the United States Senate, 1957–60."

Edith Mary Sherman, B.M.E. Colorado, 1939; M.A. Denver, 1955. "An Inquiry into the Nature of the Gerontological Movement in Colorado, 1935–62."

Jules J. Wanderer, B.A. Colorado, 1957. "Conditions and Consequences of Anomie."

COLUMBIA UNIVERSITY

Seymour S. Bellin, A.B. Brooklyn College, 1943; Certificate, Paris, 1946. "Extended Family Relations in Later Years of Life."

Frances G. Cheek, A.B. Toronto, 1946. "Family Interaction with Schizophrenics."

Philip H. Ennis, A.B. Johns Hopkins, 1948; M.A. Harvard, 1951. "The Contextual Dimension in Voting."

Frank A. Fasick, A.B. Pennsylvania State, 1951. "The First-Year Medical Student: Selected Factors Affecting His Expectations and Experiences."

Nathan Glazer, B.S.S. City College of New York, 1944; M.A. Pennsylvania, 1945. "The Social Basis of American Communism."

Leonard H. Goodman, A.B. Connecticut, 1949; M.A. Columbia, 1952. "A Sociological Case Study of a Teachers College, with Special Reference to Academic Freedom."

Alan S. Meyer, B.S.S. City College of New York, 1947; M.A. Columbia, 1951. "The Not-So-Solid South—A Study of Variability in Southern Sentiment on School Desegregation."

Robert E. Mitchell, A.B. Michigan, 1952; M.A. Harvard, 1955. "Minister-Parishioner Relations."

Milton Moss, B.S.S. City College of New York; M.A. Columbia, 1937. "Short-Run Changes in Consumer Demands: A Study in Methods of Observations and Special Reference to Automobile Demand."

Anthony R. Oberschall, A.B. Harvard, 1958. "Empirical Social Research in Germany, 1848–1914."

James L. Price, B.S. Ohio State, 1950; M.A. Illinois, 1954. "State Bureaucracy: A Comparative Study into the Conditions of Organized Stability."

Melvin Pringe, A.B., M.A. Brooklyn College, 1953, 1957. "The March toward Doctorship: A Study of the Transition from Medical Student to Intern."

Romolo Toigo, Ph.B. Chicago, 1948. "Parental Social Status as an Ecological and Individual Determinant of Aggressive Behavior Displayed by Third-Grade Children in the Classroom."

David Wallace, B.S. Westminster College, 1930. "Stability and Change in Our Town's Voting."

John Western, A.B., M.A. Melbourne, 1956, 1958. "Some Aspects of the Socialization Process in Graduate School."

CORNELL UNIVERSITY

John Joseph Carroll, S.J., A.B. Sacred Heart College, 1948; M.A. Fordham, 1959. "The Filipino Manufacturing Entrepreneurs: A Study of the Origins of Business Leadership in a Developing Economy."

Robert McNamara, S.J., A.B., M.A. St. Louis, 1949, 1953. "The Interplay of Intellectual and Religious Values."

Joseph Bernard Tamney, B.S., M.A. Fordham, 1954, 1957. "An Exploratory Study of Religious Conversion."

EMORY UNIVERSITY

James Carroll Simms, A.B., M.A. Maryland, 1956, 1957. "Values and Status Variables as Determinants of Academic Achievement."

UNIVERSITY OF FLORIDA

Benjamin E. Haddox, B.A. Stetson, 1945; M.A. Florida, 1960. "A Sociological Study of the Institution of Religion in Colombia."

FLORIDA STATE UNIVERSITY

Charles H. Newton, A.B., M.A. Nebraska, 1956, 1958. "Patterns of Career Decisions within the Professions."

HARVARD UNIVERSITY

Bernard Joseph Bergen, A.B. City College of New York, 1956. "Social Class, Symptoms, and Mental Illness: Implications for Preventive Psychiatry."

Miles Richard Cramer, A.B. Michigan, 1957; M.A. Harvard, 1960. "Community Leadership and the Desegregation Process."

Peter Carter Dodd, A.B. Princeton, 1950. "Role Conflicts in the School Principalship."

Robert Dreeben, A.B. Oberlin College, 1952; A.M. Columbia, 1954. "Organization and Environment: The Relationship between Mental Hospital and District Court."

Herman Israel, A.B. City College of New York, 1956; M.A. Harvard, 1958. "Labor's Sense of Distributive Justice in Industry."

Richard Jung, Charles University (Prague). "Analysis of Psychosocial Development: An Empirical Study of Adult, Educated Women.

Bernard Partis, Jr., A.B., M.A. Harvard, 1956, 1960. "Education as a Factor in Ethnic Attitudes."

Roger Boyd Walker, A.B. Magdalene College (Cambridge), 1955; M.A. Cambridge, 1959. "Communication in a Manufacturing Organization."

Irving Kenneth Zola, A.B. Harvard, 1956. "Sociocultural Factors in the Seeking of Medical Aid."

UNIVERSITY OF ILLINOIS

Samuel Walter Byuarm, B.A. Langston, 1949; M.A. State University of Iowa, 1950. "Community Action: A Case Study in Racial Cleavage."

Harry Cohen, B.B.A., M.A. City College of New York, 1956, 1959. "The Demonics of Bureaucracy: A Study of a Government Employment Agency."

Kenneth James Downey, B.S. State University of New York, College for Teachers at Buffalo, 1954; A.M. Illinois, 1959, "Parental Interest in the Institutionalized, Mentally Retarded Child."

Robert Lee Herrick, B.A. York College, 1953; M.A. Chicago, 1958. "Organized Churches and the Social Integration of a Community."

Doyle Kent Rice, B.A. Arkansas Polytechnic College, 1952; M.A. Arkansas, 1955. "Functional Reciprocity and Operant Value: An Experimental Test of the Devaluation Hypothesis in Reciprocal Relations."

Michael Schwartz, A.B., A.M. Illinois, 1958, 1959. "Style of Rule Enforcement and Group Effectiveness."

Julie Ellen White, B.A., M.A. Texas State College for Women, 1945, 1950. "Dimensions of Conformity and Evasion in Residence Halls for University Women: A Sociological Analysis of Normative Behavior in a Large-Scale Social Organization."

INDIANA UNIVERSITY

William J. Chambliss, B.A. California (Los Angeles), 1957; M.A. Indiana, 1960. "The Selection of Friends."

Roland Chilton, B.A. Monmouth College, 1951; M.S. Wisconsin, 1958. "Social Factors and the Residential Distribution of Official Delinquents, Indianapolis, Indiana, 1948–50 and 1958–60: A Statistical Study."

Raymond Forston, M.A. Columbia, 1946; Ph.D. Indiana, 1952. "The Threat of Elizabethan Puritanism to the Existing Social Order."

Arthur Kline, M.A. Indiana, 1948. "Prediction of Marriage with Reference to Women Graduates of Indiana University."

IOWA STATE UNIVERSITY

Albert F. Anderson, B.S., M.S. Iowa State, 1956, 1960. "Theoretical Considerations in the Analysis of Migration."

Charles L. Mulford, B.S., M.S. Iowa State, 1958, 1959. "Some Relationships between Formal Organizations, Community Problems, and Leadership."

C. Harding Veigel, B.A. North Central College, 1952; M.A. Bradley, 1956. "An Experimental Study in Differential Perception of Status Criteria."

STATE UNIVERSITY OF IOWA

Kent P. Schwirian, B.S. Illinois State Normal, 1959; M.A. Iowa, 1960. "Residence and Social Participation."

Merlin A. Taber, B.A. Penn College, 1948; M.A. Iowa, 1953. "The Professionalism of Community Services as Related to the Size and Location of Cities."

LOUISIANA STATE UNIVERSITY

Elinor Roth Nugent, B.S., M.A. Missouri, 1938, 1943. "The Relationship of Fashion in Women's Dress to Selected Aspects of Social Change in the United States from 1850 to 1950."

UNIVERSITY OF MICHIGAN

Bernard Byron Berk, A.B. California (Los Angeles), 1956; A.M. Michigan, 1957. "Informal Social Organization and Leadership among Inmates in Treatment and Custodial Prisons: A Comparative Study."

Henry Elsner, Jr., A.B., A.M. Michigan, 1952, 1953. "Messianic Scientism: Technocracy, 1919–60."

Allan Gunnar Feldt, B.S., M.A. Michigan, 1954, 1958. "The Local Ecological Community: An Investigation of Relative Independence in an Urban Society."

Ted Teruo Jitodai, A.B. Washington, 1952; A.M. State College of Washington, 1954. "Migration, Mobility, and Social Participation."

Jack Ladinsky, B.S.Ed., M.A. Missouri, 1954, 1957. "Career Development among Lawyers: A Study of Social Factors in the Allocation of Professional Labor."

John Carl Leggett, A.B., A.M. in Pol. Sci., A.M. in Soc. Michigan, 1954, 1956, 1958. "Working-Class Consciousness in an Industrial Community."

Melvin Litwack Reichler, A.B. Queens College, 1955. "Community Power Structure in Action."

David Paul Street, B.S. Northern Illinois, 1957; A.M. Michigan, 1958. "Inmate Social Organization: A Comparative Study of Juvenile Correctional Institutions."

Yuzuru Takeshita, A.B. Park College, 1951; A.M. Michigan, 1952. "Socioeconomic Correlates of Urban Fertility in Japan."

Aida Kaiser Tomeh, B.A. American (Beirut), 1954; A.M. Michigan, 1957. "Informal Group Participation and Settlement Patterns in Metropolitan Detroit."

Raymond Harry Wheeler, B.S.Ed. Miami (Ohio), 1951; A.M. Cincinnati, 1954. "The Relationship between Negro Invasion and Property Prices in Grand Rapids, Michigan."

MICHIGAN STATE UNIVERSITY

Robert Bealer, B.S., M.S. Pennsylvania State, 1953, 1955. "The Differential Distribution of Market News Functions: An Interpretive Study of Selected Structural Dimensions."

Delwyn A. Dyer, B.S., M.S. Michigan State, 1953, 1959. "Self Attitude, Role Saliency, and Role Internalization and Their Effect on 4-H Leader Tenure and Effectiveness."

Eugene C. Erickson, B.S., M.S. North Dakota State, 1953, 1955. "The Reputational Technique in a Cross-Community Perspective: Selected Problems of Theory Measurement."

James Geschwender, B.S. State University of New York, College for Teachers at Buffalo, 1955; M.A. Michigan State, 1959. "Status Consistency, Cognitive Dissonance, and Social Change."

Robert Holloway, B.S., M.S. Oregon, 1954, 1958. "Systemic Linkage, Influence, and Control in a

Hospital Decision-making Structure: A Cross-validation Study."

Clinton J. Jesser, B.A. Sioux Falls College, 1956; M.S. South Dakota State College, 1958. "An Exploration into Factors Affecting Social Participation of Professionals in Rural Areas."

Angelo Lacognata, B.A. Buffalo, 1957; M.A. Rochester, 1959. "Role Expectations of University Faculty and Students: A Social-Psychological Analysis."

Kim Rodner, B.A., M.A. Michigan State, 1956, 1959. "Social Change and the Unified Science Perspective: The Resource-Control Movement in Industrial Society."

Henry J. Watts, B.A., M.A. Buffalo, 1957, 1960. "Methodological Problems in the Measurement of Values."

David L. Westby, B.S., M.A. Wisconsin, 1951, 1957. "A Study of Status Arrangements in Three Michigan Communities."

Norbert Wiley, B.S. Loyola, 1955; M.A. Notre Dame, 1956. "Class and Local Politics in Three Michigan Communities."

George Won, B.A., M.A. Hawaii, 1955, 1957. "Democratic Sentiments in Unionism: A Case Study of the U.A.W. Convention."

UNIVERSITY OF MINNESOTA

Mary Adams, B.S. State University of South Dakota, 1946; B.S. Johns Hopkins, 1949; M.A. Columbia, 1952. "Functionalism versus Social Behaviorism in the Current Sociology of Illness: A Test of the Empirical Adequacy of Theory."

Francis George Caro, B.S. Marquette, 1958. "A Social-Class Comparison of Attitudes of Male High-School Students toward College and Dominant Occupational Goals."

Donald A. Hansen, B.A., M.A. Minnesota, 1955, 1958. "The Impact of Middle-Class Delinquency on the Family: An Exploratory Study."

Roy Herman Rodgers, B.A. Wheaton College, 1951; M.A. North Carolina, 1957. "Improvements in the Construction and Analysis of Family Life-Cycle Categories."

Edward H. Rybnicek, B.A. Denison, 1949; M.A. Minnesota, 1957. "The Changing Church in the Changing City."

UNIVERSITY OF MISSOURI

Vernon H. Edmond, B.A. Oklahoma Agricultural and Mechanical College, 1954; M.S. Purdue, 1955. "Logical Error as a Function of Group Consensus: An Experimental Study of the Effect of Erroneous Group Consensus upon Logical Judgments of Graduate Students."

James L. Lowe, B.S., A.M. Missouri, 1939, 1949. "Educational and Occupational Aspirations of High-School Seniors."

UNIVERSITY OF NEBRASKA

Laurence L. Falk, A.B. Indiana Central College, 1955; M.A. Nebraska, 1960. "The Minister's Responses to His Perceptions of Conflict between Self-expectations and Parishioners' Expectations of His Role."

John M. Hunnicutt, A.B. Hastings College, 1951; M.A. Nebraska, 1954. "An Investigation of the Relation between the Elements of the Social System and Withdrawals of Student Nurses."

Barry A. Kinsey, A.B. Oklahoma State, 1953; M.A. Nebraska, 1957. "Alcohol and Women: A Sociocultural Study of Forty-six Female Inebriates at the Willmar State Hospital, Willmar, Minnesota."

GRADUATE FACULTY, NEW SCHOOL FOR SOCIAL RESEARCH

Benjamin Benari, B.A., M.Sc.E. City College of New York, 1931, 1932. "A Sociological Study of Jewish Reconstructionism."

Abraham S. Kampf, B.S. New York, 1951; M.A. New School for Social Research, 1953. "A Study of Contemporary Synagogue Art."

NEW YORK UNIVERSITY

Irving Gellman, A.B., M.A. New York, 1951, 1956. "Alcoholics Anonymous: An Organization of Social Deviants."

Gerald Gordon, A.B., M.A. New York, 1954, 1955. "A Delineation of the Sick Role."

Jacob Sodden, A.B. Brooklyn College, 1941; M.A. New York, 1943. "The Impact of Suburbanization on the Synagogue."

STATE UNIVERSITY OF NEW YORK AT BUFFALO (FORMERLY UNIVERSITY OF BUFFALO)

Eugene B. Piedmont, B.S. State University of New York, College of Education, 1956; M.A. Rochester, 1959. "An Investigation of the Influence of Ethnic Grouping Differences in the Development of Schizophrenia."

UNIVERSITY OF NORTH CAROLINA

David Almon Gover, A.B., M.A. Michigan, 1947, 1951. "Employment as a Factor in Marital Adjustment of Middle- and Working-Class Wives."

Berton Harris Kaplan, B.S. Virginia Polytechnic Institute, 1951; M.S. North Carolina, 1952. "Social Change in a Mountain Community."

Mark Cushman Thelin, A.B., M.A. Oberlin College, 1955, 1958. "Executive Characteristics and Community Involvement: A Sociological Analysis of Top Business Executives in Four North Carolina Cities."

NORTHWESTERN UNIVERSITY

Troy Smith Duster, B.S. Northwestern, 1957; M.A. California (Los Angeles), 1959. "The Social Response to Abnormality."

Peter S. McHugh, B.A., M.A. California (Los Angeles), 1957, 1959. "Order and Disorder in Social Time and Space."

Morris H. Sunshine, A.B., M.A. Missouri, 1949, 1954. "Anomie, Alienation, and Norm Compliance."

UNIVERSITY OF NOTRE DAME

Thomas Monterville Coffee, A.B. St. Benedict's College, 1952; M.A. Emory, 1955. "An Empirical Study of Residential Propinquity and Marital Selection: South Bend, Indiana, 1954–58 Inclusive."

Sister M. Brigid Fitzpatrick, A.B. Mount St. Mary's College, 1947; M.S.W. Catholic, 1949. "The Sister Social Worker: An Integration of Two Professional Roles."

Sister Mary Christopher O'Rourke, B.A. Brown, 1940; M.A. Notre Dame, 1955. "The Impact of the Educational Environment upon the Development of a Professional Self-image."

Sylvester P. Theisen, B.A. St. John's, 1947; M.S.E. Notre Dame, 1951. "A Social Survey of Aged Catholics in the Deanery of Fort Wayne, Indiana."

OHIO STATE UNIVERSITY

C. Leroy Anderson, B.S., M.S. Brigham Young, 1957, 1959. "Assessment of Orientations toward Dependence upon Public Assistance."

Alice Yun Chai, B.A. Ohio Wesleyan, 1955; M.A. Ohio State, 1957. "Kinship and Mate Selection in Korea."

Giles Edward Gobetz, M.A. Washington, 1955. "Adjustment and Assimilation of Slovenian Refugees."

Arthur Eugene Havens, B.S. Iowa State College, 1959; M.S. Ohio State, 1960. "Social-Psychological Factors Associated with Differential Adoption of New Technologies by Milk Producers."

Charles A. Hildebrandt, B.S., M.A. Kent State, 1955, 1959. "Relationship of Some Personal and Social Variables of School Children to Preferences for Mixed Schools."

Judson R. Landis, A.B. California (Berkeley), 1957; M.A. Ohio State, 1959. "Social-Class Differentials in Self, Value, and Opportunity Orientation as Related to Delinquency Potential."

Anastassios D. Mylonas, M. Crim. California (Berkeley), 1955. "Prisoners' Attitudes toward Law and Legal Institutions."

Frank R. Scarpitti, B.A. Fenn College, 1958; M.A. Ohio State, 1959. "Differential Socialization: The Delinquent versus the Non-Delinquent."

UNIVERSITY OF OREGON

James M. Rollins, B.A., M.A. Los Angeles State College, 1952, 1955. "Two Empirical Tests of a Parsonian Theory of Family Authority Patterns."

Sherrad Lee Spray, B.S. Oregon, 1958. "Behavioral Mix in Role Performances: A Study of Executive Behavior."

UNIVERSITY OF PENNSYLVANIA

Lawrence Joseph Cross, S.J., A.B., M.A. Loyola, 1943, 1951. "The Catholics in Norristown, Pennsylvania."

Thomas E. Dow, B.A. Hunter College, 1958; M.A. Pennsylvania, 1960. "A Sociological Analysis of Family Reaction to Disability and Institutionalization."

Ann Ratner Miller, A.B. Bryn Mawr College, 1943. "State Labor-Force Trends and Differentials in the United States from 1870 to 1950."

Earl Y. Reeves, B.A. Eastern Baptist College, 1949; M.A. Pennsylvania, 1954. "A Comparative Study of the Success or Failure of Negro and White Offenders on Probation."

Richard Stephen Sterne, B.A. Swarthmore College, 1942; M.A. Duke, 1947. "Broken Homes and the Violations of Delinquency Norms in First Offenses among Juveniles."

K. C. Zachariah, B.S. Union Christian College (India), 1946; M.S. Central Research Institute (India), 1948. "Historical Study of Internal Migration in the Indian Subcontinent, 1901–31."

PENNSYLVANIA STATE UNIVERSITY

Joseph E. Faulkner, A.B., B.D., M.A. Emory, 1949, 1953, 1956. "Attitudes Associated with Differential Participation in the Church on the Part of Labor Union Members."

UNIVERSITY OF PITTSBURGH

Martin D. Adler, A.B., M.S.W. Pittsburgh, 1952, 1956. "Hemiplegia and the Social Structure."

PRINCETON UNIVERSITY

Thomas Kirby Burch, A.B. Loyola College, 1956; M.A. Princeton, 1959. "Internal Migration in Venezuela: A Methodological Study."

PURDUE UNIVERSITY

Whitney Hyde Gordon, B.A. Pomona College, 1954; M.S. Purdue, 1956. "Stress and the Jewish Community of Middletown."

Robert Perrucci, B.S. State University of New York, 1958; M.S. Purdue, 1959. "Social Status, Goals, and Rewards: A Study in the Social Organizations of a Psychiatric Ward."

UNIVERSITY OF SOUTHERN CALIFORNIA

Eiji Cannon Amemiya, A.B. George Pepperdine College; A.M. Southern California. "Economic Differentiation and Social Organization of Standard Metropolitan Areas in the United States: 1950."

Bernard Barber, A.B. Los Angeles State College, 1954; Fil.Kand. Stockholm, 1955. "A Study of the Attitudes of Mothers of Mentally Retarded Children as Influenced by Socioeconomic Status."

Stuart Alan Brody, A.B., California (Los Angeles), 1955; A.M. Southern California, 1958. "Husband-Wife Communication Patterns Related to Marital Adjustment."

Robert Lane Brown, A.B. Whittier College, 1949; M.S. in Ed. Southern California, 1952. "Attitudes of Ministers and Lay Leaders of the American Baptist Convention of the State of Washington on Selected Social Issues."

Charles Clifford Crider, A.B. Washington Missionary College, 1942; A.M. Seventh-day Adventist Theological Seminary, 1951. "Regional Variations in the Estimation of Social Class from Auditory Stimuli."

Isis Istiphan, A.B. American (Cairo), 1947; M.S. Columbia, 1953. "Role Expectations of American Undergraduate College Women in a Western Coeducational Institution."

Jane Ross Mercer, M.A. Chicago, 1948. "An Analysis of Factors in the Family's Withdrawal of a Patient from a Hospital for the Mentally Retarded."

Joseph Edward Ribal, B.S. Northern Illinois, 1952; A.M. Chicago, 1953. "The Selfish Self: A Social-Psychological Study of Social Character."

Robert Burtch Rogers, A.B. Louisiana State, 1951; A.M. Southern California, 1955. "Perception of the Power Structure by Social Class in a California Community."

James Rex Smith, A.B. Phillips, 1943; B.D. Yale, 1945; M.A. Southern California, 1960. "Personality and Interpersonal Factors Associated with the Duration of Marriage Counseling."

Abdolhamid Zahedi, B. of Economics, B. of Law, Tehran (Iran), 1948, 1949; M.S. Southern California, 1957. "An Analytical Study of Attorneys' Occupational Values and Satisfactions."

UNIVERSITY OF TEXAS

Donald Floyd Allen, B.A. North Texas State College, 1951; M.A. Texas, 1956. "Changes in the Role of the American University Professor."

Norval Dwight Glenn, B.A. New Mexico Agricultural and Mechanical College, 1954; M.A. Texas Technological College, 1957. "The Negro Population in the American System of Social Stratification: An Analysis of Recent Trends."

Hillquit Lynch, B.S., M.S. Houston, 1946, 1947. "The Romantic Complex and the Adolescent: An Analysis of Basic Assumptions in Family Sociology."

TULANE UNIVERSITY

David Hale Malone, B.A., M.A. Tulane, 1952, 1959. "Individual Scope in the Occupational Images of Trainees."

UNIVERSITY OF UTAH

James Marion Baker, B.S. Wayne, 1953; M.S. Brigham Young, 1957. "The Relationship between Social Stress and Medical Health."

Henry van der Velden, A.B. Postel (Belgium), 1932; M.S. Utah, 1959. "Parent-Child Relationships and Adolescents' Desire To Leave Home."

VANDERBILT UNIVERSITY

Verne C. Bechill, B.A. Ohio Wesleyan, 1954; M.A. Emory, 1956. "A Comparison of Role Incongruity in Married and Divorced Couples."

Paul Deutschberger, B.S. City College of New York, 1940; M.A. Columbia 1941. "Role and Feedback in the Management of the Social Encounter."

Paul Geisel, B.A. Willamette, 1956; M.A. Vanderbilt, 1958. "I.Q. Performance, Educational and Occupational Aspirations of Youth in a Southern City—a Racial Comparison."

WASHINGTON UNIVERSITY (ST. LOUIS)

Rodney Michael Coe, B.S. Iowa State, 1955; M.A. Southern Illinois, 1959. "Institutionalization and Self-conception."

UNIVERSITY OF WASHINGTON

Wallace A. Dynes, B.A., M.A. Florida State, 1952, 1954. "An Explication of the Traditional Usage of 'Norm.'"

Asghar Fathi, B.A. American (Beirut), 1954; M.A. Washington (Seattle), 1959. "The Effects of Latent Positions on Interaction."

Ørjar Øyen, M.A. Washington (Seattle), 1953. "Ecological Context and Residential Differentiation: Neighborhood Attachment in Four Areas of Oslo."

Ernest George Palola, B.S., M.S. Washington (Seattle), 1956, 1958. "Organization Types and Role Strains: A Laboratory Study of a Complex Organization."

Norma Armstrong Verwey, B.A., M.A. Acadia, 1957, 1958. "Relationship of Adaptability to Interactional Contingency and Interpersonal Prediction."

WASHINGTON STATE UNIVERSITY

James E. Conyers, B.A. Morehouse College, 1954; M.A. Atlanta, 1956. "Attitudes of Negroes and Whites toward the Phenomenon of 'Passing.'"

WAYNE STATE UNIVERSITY

Robert B. Smock, B.A. Adrian College, 1946; M.A. Wayne, 1953. "Social Change and Personal Debt—a Historical Introduction to the Sociology of Consumer Credit."

WESTERN RESERVE UNIVERSITY

Eleanor K. Caplan. B.S., M.A. Western Reserve, 1955, 1958. "Attitudes and Behavior in a Middle-

Class Biracial Neighborhood: A Situational Approach to Relationship and Prediction."

UNIVERSITY OF WISCONSIN

Leon Francis Fannin, B.A., M.A. Wisconsin, 1953, 1957. "A Study of the Social-Class Affiliation and Societal Reaction to Convicted Sex and Non-Sex Offenders."

Kenneth Gordon Lutterman, B.S., M.S. Wisconsin, 1951, 1954. "Giving to Churches: A Sociological Study of the Contributions to Eight Catholic and Lutheran Churches."

Leonard Paul Metzger, B.A. Swarthmore College, 1954; M.S. Oregon, 1958. "A Factorial Study of Attitudes toward Jews."

Earl Richard Quinney, B.S. Carroll College, 1956; M.A. Northwestern, 1957. "Retail Pharmacy as a Marginal Occupation: A Study of Prescription Violation."

Lalit Kumar Sen, B.A., M.A. Calcutta, 1947, 1950. "Social Dimensions of Modernization in Four Indian Villages."

Austin Theodore Turk, B.A. Georgia, 1956; M.A. Kentucky, 1959. "Adolescence and Delinquency in Urban Society: A Study in Criminological Theory."

YALE UNIVERSITY

David Alexander Goslin, B.A. Swarthmore College, 1958; M.A. Yale, 1959. "Accuracy of Self-Perception and Adolescent Adjustment."

Charles Jones, B.A. Princeton, 1938; M.S. Yale, 1953. "Social and Cultural Change in Three Minnesota Chippewa Indian Communities."

Derek L. Phillips, B.A. Rutgers, 1959; M.A. Arizona, 1960. "Help Sources and Rejection of the Mentally Ill: An Experiment in Influencing Responses to Mental Disorder."

Margaret Plymire, B.S. Illinois, 1954; M.A. Yale, 1961. "Adaptation to Illness: A Study of Patterns of Search."

DOCTORAL DISSERTATIONS NEWLY STARTED IN 1962

The following list of 178 doctoral dissertations *newly started* in 1962 in universities and colleges in the United States and Canada is compiled from returns sent by forty-six departments of sociology. This list includes dissertations in social work, divinity, and other related fields.

BOSTON UNIVERSITY

Shirley Kolack, B.S., M.A. Boston, 1955, 1957. "Status Inconsistency among Social Work Professionals."

Robert W. Morgan, Jr., B.A. Harvard, 1948; M.A. Boston, 1960. "Socialization of Medical Students in Nigeria."

Thomas Mott Osborne II, A.B. Williams College, 1947; M.A. Boston, 1958. "Religion, Values, and Social Participation."

Mary Ann Quarles, A.B. Mount Holyoke College, 1947; M.A. Kentucky, 1952. "A Comparative Study of the Relation of the Treatment Program at the New Jersey Reformatory for Women for the Fifty-Year Period, 1910–60, to Concepts of Penology."

Robert T. Smart, A.B., M.A. Boston, 1948, 1949. "A Study of Interaction between Surgeons and Anesthetists in a Large Teaching Hospital."

BROWN UNIVERSITY

Robert Bruce Jessen, B.A. Union College, 1958. "Migration of the Older Population of the United States, 1950–60."

John Joseph Macisco, B.A., M.A. Fordham, 1958, 1959. "Internal Migration in Puerto Rico: A Comparative Analysis, 1940 and 1960."

Alvan O'Neil Zarate, A.B., M.A. Connecticut, 1958, 1960. "Differential Rural-Urban Fertility in Mexico."

UNIVERSITY OF CALIFORNIA, LOS ANGELES

Roland Chester Bower, A.B. San Diego State College, 1957; M.A. California (Los Angeles), 1961. "Hospital Organization, Social Control, and Performance."

Shelley Chandler, A.B., M.A. California (Los Angeles), 1961, 1962. "Work Involvement and Its Consequences for Some Aspects of Non-occupational Life."

James T. Duke, A.B., M.A. Utah, 1957, 1958. "Equalitarianism and Recruitment of Elites in a New Nation."

Chad Gordon, A.B., M.A. California (Los Angeles), 1957, 1962. "Self-conception and Social Achievement Behavior."

Maurice Jackson, A.B., M.A. California (Los Angeles), 1957, 1958. "Conformity and Reference-Group Behavior in Metropolitan Los Angeles."

George Kagiwada, A.B., M.A. California (Los Angeles), 1958, 1962. "Class and Ethnic Identification: A Study of Japanese-Americans in Los Angeles."

Witold Krassowski, A.B., M.A. Purdue, 1952, 1954. "Naturalization and 'Assimilation Proneness' of California Immigrant Population."

Ronald M. Pavalko, A.B., M.A. California (Los Angeles), 1956, 1961. "Occupational Choice and Professional Recruitment: A Study of Pre-dental Students."

Eleanora Petersen, B.A. Ohio State, 1940; M.A. California (Los Angeles), 1953. "Some Role Dimensions of Loyalty."

Gerald M. Platt, B.A., M.A. Brooklyn College, 1955, 1957, "A Study of the Problems of Cognition and Reference."

Herman Schwendinger, B.A. College of the City of New York, 1948; M.A. Columbia, 1956. "Instrumental Theory of Delinquency."

Harvey Van Der Merwe, B.A., M.A. Stellenbosch (South Africa), 1956, 1957. "Industrializing Elites in a Saskatchewan Community."

CATHOLIC UNIVERSITY OF AMERICA

Joan E. Backschieder, B.S.N. College of Mount St. Joseph-on-the-Ohio, 1956; M.A. Catholic, 1959. "Role of Socioenvironmental Factors in Mental Illness in a Tri-racial Isolate Population."

Sister Mary Esther Heffernan, B.Ph., M.A. Chicago, 1948, 1951. "The Patterns of Conformity and Deviance in the Social Structure of a Total Institution."

Sister Mary George O'Toole, B.A. St. Joseph's (Windham), 1951; M.A. Catholic, 1961. "A Religious Community as a Social System—Using Constitutions of the Congregation of Religious Sisters of Mercy."

Sister Mary Regis Ramold, A.B. Loras College, 1957; M.A. Catholic, 1958. "A Religious Community as a Social System—Use of the Constitutions of the Ursuline Nuns of Congregation of Paris."

UNIVERSITY OF CHICAGO

Alan F. Blum, A.B. Roosevelt, 1957; A.M. Chicago, 1962. "Primary Groups and the Intergenerational Transmission of Sociopolitical Information."

J David Colfax, B.S., Pennsylvania State, 1955; A.M. Pittsburgh, 1960. "The Big-City Voter: A Study of Political Participation in Chicago."

Robert L. Crain, B.A. Louisville, 1956. "Intercity Influence in the Diffusion of Fluoridation."

Kai T. Erickson, A.B. Reed College, 1953; M.A. Chicago, 1955. "Wayward Puritans: A Study in the Sociology of Deviance."

Wolf V. Heydebrand, Grad. Status Frankfurt (Germany), 1954; M.A. Chicago, 1961. "Structural Relationships in Formal Organizations: A Comparative Study."

Nathan Kantrowitz, B.S. City College of New York, 1950; M.A. Chicago, 1955. "Pre-Civil War Political Realignment."

R. S. Krupp, B.S. S.D. College (Alleppey, India), 1948; M.S. Kerala, 1950. "Recent Trends in World Mortality and Their Implications for a Revised System of Model Life Tables."

Ismail Maung, A.B. Rangoon, 1955; M.A. Chicago, 1961. "Urban Structure in New Zealand, 1956."

Margaret Peil, B.S. Downer College, 1951; A.M. Fordham, 1961. "The Use of Child-rearing Literature by Low-Income Families."

Joan Stelling, A.B. Michigan, 1955. "Religion and Mobility in Urban Society."

UNIVERSITY OF COLORADO

Harold Charles Meier, B.A. Colorado, 1951. "The Oral Communication of Health-Disease Beliefs in a Serial Reproduction Experiment."

COLUMBIA UNIVERSITY

Diana Crane, A.B. Radcliffe College, 1953. "Selection of Research Topics by University Professors."

Celia Heller, A.B. Brooklyn College, 1944. "Ambition and Offense among Mexican-American Youth—a Comparative Study."

Florence McClure, A.B. Cornell, 1928. "Participation in Community Organizations by Parents of Children with Physical Disabilities."

Josephine Schoenborn, B.S. Columbia, 1954; M.A. New York, 1956. "After Columbia: Recipients of Columbia Graduate Degrees in Sociology and Career Patterns."

Mildred Anne Schwartz, A.B., M.A. Toronto, 1954, 1956. "Attitudes toward Political Issues and Party Preference in Canada."

Thomas P. Wilson, A.B. Reed College, 1955. "Inmate-Staff Relationships and the Inmate System in a Correctional Institution."

CORNELL UNIVERSITY

Nona Yetta Glazer, B.A., M.A. Oregon, 1955, 1957. "Socioeconomic Opportunity and Varieties of Civil-Military Relationships."

John MacGregor, B.A. Maine, 1961. "Theory and Associated Research on the Bases of Interpersonal Attraction."

DUKE UNIVERSITY

Engin Inel, B.A. American College for Girls (Turkey), 1951; M.A. Duke, 1962. "Information or Noise? A Cross-cultural Experiment on the Meaning of Response Style in Social Research."

EMORY UNIVERSITY

Faith Lynn Magdovitz, A.B. Chatham College, 1960; M.A. Emory, 1961. "The Effects of Role on Social Perception."

Ted Dwayne Westermann, B.A. Concordia Seminary, 1950; M.A. Emory, 1962. "Goal Consensus and Conflict between Subunits of a Complex Organization."

UNIVERSITY OF FLORIDA

Jose Fabio Barbosa da Silva, Certificate in Sociology, São Paulo, 1960; Postgraduate Course, Escola de Sociologia de São Paulo, 1960. "A Sociological Analysis of Migration in Brazil."

Joseph Sardo, B.A., M.A. Florida, 1950, 1953. "A Comparative Study of Rural Social Organization in Colombia and Sicily."

W. Kennedy Upham, B.A. Maryville College, 1952; M.A. Florida, 1962. "A Sociological Study of the Relationships of Man to the Land in Central America."

FLORIDA STATE UNIVERSITY

Jeanine Gavin, B.A., M.A. Bradley, 1954, 1956. "Verbal Reinforcement in the Small Group."

Karl B. King, B.S., M.S. Southern Methodist, 1950, 1960. "Marital Role Expectations of Negro Adolescents."

George W. Wallis, A.B. Georgia, 1953; M.A. George Peabody College for Teachers, 1954. "Some Social Dimensions of Civil Defense."

FORDHAM UNIVERSITY

Ruth Narita Doyle, A.B. Manhattanville College, 1951; M.A. Fordham, 1955. "Parent-Child Relations in the Social Mobility Aspirations of Some Puerto Rican High-School Students."

HARVARD UNIVERSITY

Gordon Allan Fellman, A.B. Antioch College, 1957; M.A. Harvard, 1962. "The Failure of Radical Politics in India: Case Study of a Party and a Leader."

Zelda F. Gameson, A.B., M.A. Michigan, 1958, 1959. "Organizational Responses to Members' Characteristics."

Joseph Zvi Namenwirth, Doctoral Candidate, Amsterdam, 1954, 1957. "Supra-national Integration in Western Europe."

Harry Alan Scarr, A.B. Michigan, 1956. "Value-Orientation Assessment: Developing a Methodology."

Robert Everett Stanfield, A.B. City College of New

York, 1957; M.A. Harvard, 1961. "A Study in Delinquency and Its Control: The Family and the Peer Group."

Rebecca S. Vreeland, A.B., M.A. Radcliffe College, 1958, 1961. "The Effect of Organizational Context upon College Students' Occupational Choices and Attitudes."

UNIVERSITY OF ILLINOIS

Bruce K. Eckland, A.B., M.A. Illinois, 1956, 1960. "The Social Consequences of College Dropouts."

Thomas Duggan, A.B., M.A. St. Louis, 1957, 1959. "Desegregation of the New Orleans Catholic Schools."

George A. Pownall, A.B., M.A. Illinois Normal, 1952, 1957. "The Role of the Parole Supervising Officer."

Gerald D. Suttles, A.B. Reed College, 1959; M.A. Illinois, 1962. "Multigroup Membership and the Social Control of Delinquent Behavior."

Eugene P. Wenninger, B.S. Ohio State, 1953; M.A. Kent State, 1960. "Status Aspirations, Interest Groups, and Bureaucratic Stability."

INDIANA UNIVERSITY

Joseph W. Scott, B.S. Central Michigan, 1957; M.A. Indiana, 1959. "Sources of Changes in Community and Family Life."

Edmund W. Vaz, B.A., M.A. McGill, 1951, 1955. "Middle-Class Delinquency."

IOWA STATE UNIVERSITY

Gerald E. Klonglan, B.S., M.S. Iowa State, 1958, 1962. "Role of a Free Sample Offer in the Adoption of a Technological Innovation."

Richard D. Warren, B.S., M.S. Iowa State, 1952, 1960. "Influence of a Dealer Training Program."

STATE UNIVERSITY OF IOWA

William R. Hazard, B.S. Wisconsin, 1958; M.S. Iowa, 1960. "Self-complementarity and Similarity: An Empirical Study of Mate Attraction."

Richard Ingersoll, A.B., B.S. Central Michigan, 1958; M.A. Iowa, 1960. "The Self and Situational Stress: An Empirical Study.

Donald McTavish, B.S., M.A. Iowa, 1956, 1960. "Some Effects of the Industrial Context of Occupations."

John W. Prehn, B.A. Macalester College, 1959; M.A. Iowa, 1960. "Migrant Status and Social Mobility among College Graduates."

JOHNS HOPKINS UNIVERSITY

Roland Hawkes, A.B. Tufts, 1958; M.A. Boston, 1960. "A Model of Urban Ecology."

Maurice Pinard, A.B., M.A. Montreal, 1951, 1955. "The Sudden Rise of the Social Credit Party in Quebec, 1962."

Donald Van Eschen, A.B. Beloit College, 1956; M.A. Chicago, 1960. "Rural Political Behavior."

UNIVERSITY OF KENTUCKY

Martin Jay Crowe, B.A., M.A. State College of Washington, 1956, 1959. "The Occupational Adjustment of Industrial Workers Recruited from a Rural, Low-Income Kentucky Mountain Neighborhood."

Gordon F. DeJong, B.A. Central College (Iowa), 1957; M.A. Kentucky, 1960. "Human Fertility in the Southern Appalachian Region."

Ali A. Paydarfar, B.A. Tehran (Iran), 1956; M.A. Kentucky, 1960. "Modernization and Demographic Characteristics of Iranian Provinces and Selected Nations."

LOUISIANA STATE UNIVERSITY

Arthur F. Clagett, B.A. Baylor, 1943; M.A. Arkansas, 1957. "The Louisiana Prison: A Sociological Analysis of Felons Committed to Prison in 1959."

Robert J. Dolan, B.S., M.S. Louisiana State, 1949, 1958. "Role Analysis: A Study of the Roles of the County Agent."

Barbara Goodnight, B.A. Texas Woman's, 1960; M.A. Louisiana State, 1962. "The Significance of the Adolescent Clique as an Agency of Socialization."

Arthur Jones, B.A. Baylor, 1959; M.A. Louisiana State, 1962. "Career Choice in Two Agribusiness Professions."

UNIVERSITY OF MICHIGAN

Michael T. Aiken, B.A. Mississippi, 1954; M.A. Michigan, 1955. "Kinship in an Urban Community."

T. R. Balakrishnan, B.A., M.A. Madras (India), 1952, 1954. "Internal Migration and Economic Opportunity—a Study of Net Migration in the Standard Metropolitan Areas of the United States, 1940–50."

James Ryland Hudson, B.A. Columbia College, 1958; M.A. Michigan, 1960. "Labor Unions in a Small Community: Power without Prestige."

Ronald L. Johnstone, B.A., B.D. Concordia Seminary, 1956, 1960; M.A. Michigan, 1960. "A Study of the Political Orientations, Activities, and Leadership of Negro Clergymen in Detroit, Contrasting Militant, Moderate, and Pacifistic Approaches to Local Decision-making Processes."

Eric Robert Krystall, B.A. Witwatersrand, 1956; B.S. London School of Economics, 1960; M.A. Michigan, 1962. "The Children of the Negro Mother: A Study of Negro Family Structure and Family Size in Detroit."

William F. Pratt, A.B. Wayne, 1951. "A Study of Premarital Pregnancies in a Major Metropolitan Area."

MICHIGAN STATE UNIVERSITY

Paul Harold Besanceney, B.Lit. Xavier, 1947; A.M. St. Louis and John Carroll, 1954; S.T.L. West Baden College and Michigan, 1957. "Factors As-

sociated with Protestant-Catholic Marriages in the Detroit Area: A Problem in Social Control."

MISSISSIPPI STATE UNIVERSITY

Joseph H. Bruening, B.A., M.A. Florida, 1954, 1956. "A Theory of the Community Based on the Concepts of the Overlapping Action Areas."

Edward C. Lehman, Jr., B.A., M.A. Mississippi State, 1958, 1963. "A Technique for Determining Non-response Bias on Mail Questionnaires."

UNIVERSITY OF MISSOURI

Ivan Chapman, B.A., M.A. San Francisco State College, 1954, 1955. "A Comparative Study of Delinquents and Non-Delinquents in Springfield, Missouri."

Buford B. Rhea, Jr., B.A., M.A. Tennessee, 1951, 1955. "Organizational Analysis and the Educational Experience: An Exercise in Sociological Theory."

Daniel M. Schores, Jr., A.B. Central Methodist College, 1950; B.D. Duke, 1953; M.S. Missouri, 1963. "Osage Beach: Social Stratification in a Resort Community."

Ansel P. Simpson, A.B. Virginia State College, 1949; M.A. Pennsylvania, 1950. "Attitudes, Status, and Minority-Group Membership: A Study in Experimental Sociology."

UNIVERSITY OF NEBRASKA

Walter Baeumler, B.A., M.A. Omaha, 1951, 1961. "Life-Cycle and Membership and Participation in Formal Voluntary Associations."

Bartolomeo J. Palisi, B.A., M.A. Brooklyn College, 1956, 1960. "Ethnicity, Family Structure, and Participation in Voluntary Associations."

NEW SCHOOL FOR SOCIAL RESEARCH

Emily Alman, B.A. Hunter College, 1945; M.A. New School for Social Research, 1959. "Apperception of Self and Reality."

Morris L. Fried, B.A. Buffalo, 1951; M.A. New School for Social Research, 1958. "The Sociology of an Occupation: The Shorthand Reporter."

Elmer Lear, B.A., M.S. City College of New York, 1938, 1941; M.A. Johns Hopkins, 1953; Ph.D. Columbia, 1951. "The Western Leyte Guerrilla Warfare Regime: A Sociopolitical Study in the Withholding of American Recognition."

Roslyn Lippman, B.A. Brooklyn College, 1952. "Present-Day Proletariat Sects."

Henry Seligson, B.S. City College of New York, 1943; M.A. Teachers College, Columbia, 1949. "Occupational Interests in School Settings."

NEW YORK UNIVERSITY

Hwa-Kuo Yeh, A.B. San Francisco State College, 1954; M.A. New York, 1960. "Mate Selection among the Chinese in Singapore: A Sociological Approach."

Richard Korn, B.S. City College of New York, 1948. "Delinquent Theft and the Orientation to the Other: A Study of Enlargement and Constriction in the Range of Interpersonal Reciprocity."

Gerald Marwell, B.S. Massachusetts Institute of Technology, 1957; M.A. New York, 1959. "Dimensions of Legislative Conflict."

Manfred Stanley, A.B. City College of New York, 1954; M.A. New York, 1958. "Heritage of Change: A Study of Potential East African Elite."

STATE UNIVERSITY OF NEW YORK AT BUFFALO (FORMERLY UNIVERSITY OF BUFFALO)

Ronald G. Jones, B.A., M.A. Virginia, 1950, 1955; Ed.D. Buffalo, 1959; "Toward a Philosophical Sociology of Education."

UNIVERSITY OF NORTH CAROLINA

Charles Michael Bonjean, A.B. Drake, 1957; M.A. North Carolina, 1959. "Community Leadership."

Barbara Robinson Bradshaw, A.B. Denver, 1952; M.A. Northwestern, 1956. "Cottage Parents: An Occupational Study."

John Rochester Earle, A.B. Wake Forest College, 1958; M.A. North Carolina, 1961. "Marital Relations and Family Integration."

Joy Rochell Gold, A.B., M.A. George Washington, 1958, 1960. "Effect of Administrative Atmosphere on the Role of the Teacher."

Dean DeWayne Knudsen, A.B. Sioux Falls College, 1954; M.A. Minnesota, 1961. "High-School Dropouts."

Richard Anthony Lamanna, A.B., M.A. Fordham, 1954, 1958. "Role of Negro Public School Teacher in Process of School Desegregation."

Hugh Max Miller, A.B., M.A. North Carolina State Teachers College, 1955, 1958. "Teacher Roles and Community Organization."

James Allen Williams, A.B. North Carolina, 1958; M.A. Cornell, 1961. "Interviewer Bias."

NORTHWESTERN UNIVERSITY

Herbert R. Barringer, A.B. San Diego State College, 1959; M.A. Northwestern, 1961. "Primary versus Secondary Social Orientations: Korea and the United States."

David B. Booth, B.S., Chicago, 1949; M.A. Illinois, 1961. "Status Consistency and Situs Mobility."

Rose M. Giallombardo, B.A. Connecticut, 1958; M.A. Northwestern, 1960. "Role Structure in a Women's Prison."

Gilbert M. James, A.B. Greenville College, 1955; M.A. Washington, 1957. "A Study of Anomia."

Peter A. Orleans, B.A. Antioch College, 1958; M.A. Wisconsin, 1961. "The Social Contexts of Political Participation in the Metropolis."

Sonya Orleans, B.A. Antioch College, 1958; M.A. Northwestern, 1962. "Normative and Coercive Power and Compliance to Tax Law."

Kenneth J. Reichstein, B.A. Syracuse, 1959; M.A.

Northwestern, 1961. "The Professional Ethics of Lawyers."

OHIO STATE UNIVERSITY

Joseph Albini, A.B. Pennsylvania State, 1954; M.A. Ohio State, 1956. "An Evaluation of Therapy with Disturbed and Defective Children."

Norman J. Johnson, B.A. Duquesne, 1957; M.A. Ohio State, 1960. "Toward a Taxonomy of Organizations."

Lawrence Moyer, B.A., M.A. Ohio State, 1959, 1960. "Occupational Variables in the Role of the Sick."

Constantina Safilios Rothschild, B.S. Athens (Greece), 1957; M.S. Ohio State, 1959. "The Effect of the Nature of the Self-image of Rehabilitants upon the Perception of Their Family Roles and Their Rehabilitation Success."

Hans Sebald, A.B. Manchester (Indiana), 1958; M.S. Ohio State, 1959. "Significant Others and Self-perceptions: A Study of Adolescent Boys."

Tom Webb, B.A. Baldwin-Wallace College, 1947. "Classification of Teachers by Bureaucratic and Professional Normative Orientations to Education Issues."

UNIVERSITY OF OREGON

Robert Blumstock, B.A., M.A. City College of New York, 1956, 1957. "The Relation between Values as Goals and Behavior in an Organization."

Roy T. Bowles, B.S. Brigham Young, 1959. "Occupational Behavior Requirements and Work Values."

James T. Borhek, B.A. Illinois, 1953; M.A. California (Berkeley), 1960. "An Incongruence Theory of Intolerance."

Donald J. Call, B.A., M.A. Oregon, 1957, 1959. "Juvenile Delinquency in a Small City-Rural Area."

David L. Dodge, B.A. San Diego State College, 1958. "Stress, Integration, and Chronic Disease Morbidity and Mortality."

Martin Meissner, B.Com. British Columbia, 1958. "Technology and Group Structure."

Caleb R. Paulus, B.A. Rangoon (Burma), 1934; M.A. Madras (India), 1940. "Impact of Urbanization on Fertility in India."

Curt Tausky, B.A. Portland State College, 1959. "Social Mobility Anchorage Points of Middle Managers."

PRINCETON UNIVERSITY

Robert Merten Cook, B.Mgt.E. Rensselaer Polytechnic Institute, 1956; M.A. Princeton, 1962. "Planned Social Change: Personality and Social Structure."

Mohammed Guessous, B.A., M.A. Laval, 1958, 1960. "Alternative Models for a Theory of Social Change."

Michael Lewis, A.B. Brooklyn College, 1959; M.A. Princeton, 1962. "Matrifocal Family in the Urban Setting."

Harry Webb, B.A. Michigan State, 1957; M.A. Princeton, 1963. "Political Left and Right: Line or Circle?"

Ian Isaac Weinberg, B.A. Exeter College, Oxford, 1961. "The English Public Schools."

John Skelton Williams, Jr., A.B. Bowdoin College, 1959; M.A. Princeton, 1962. "Demographic Aspects of the Never Married Woman."

UNIVERSITY OF SOUTHERN CALIFORNIA

Genevieve Delta Carr, B.A. Ohio Wesleyan, 1935; A.M. Southern California, 1952. "Psychosociological Factors in Fertile and Infertile Marriages."

Billyanna Niland, A.B., D.D.S., M.A. Southern California, 1940, 1944, 1948. "Social Factors Related to Dentistry as a Career."

Fernando Penalosa, A.B., M.A. Denver, 1949, 1950; Ph.D. Chicago, 1956; M.A. Southern California, 1959. "Class Consciousness and Social Mobility in a Mexican-American Community."

SOUTHERN ILLINOIS UNIVERSITY

Herschel Edward Aseltine, B.A., B.D. McMaster, 1951, 1952; M.A. Chicago, 1955. "The Dispersed City in Southern Illinois."

Clement Blakeslee, B.A. Kansas, 1956. "The Functions of the Bar and Social Disorganization."

Victor B. Streufert, B.A., Concordia Theological Seminary, 1951; M.A. (Soc.) Washington (St. Louis), 1957. "Sect-Type Activity among Southern Appalachian Migrants in the Uptown Community of Chicago." "Forms of Religious Behavior."

UNIVERSITY OF TEXAS

Walter Joseph Cartwright, B.A. Southern Methodist, 1943; M.A. Texas, 1960. "The Cedar Chopper: Social Study of a Differentiated Subgroup and Its Stabilization as a 'Dispersed Community.'"

Sanford Irwen Labovitz, B.A. California (Los Angeles), 1960; M.A. Texas, 1963. "Technology and Division of Labor."

UNIVERSITY OF TORONTO

Thomas R. Maxwell, B.A., M.A. Toronto, 1940, 1952; Th.M. Princeton, 1945. "The Protestant Ministry as a Career."

Samuel Sidlofsky, B.A., M.A. Toronto, 1960, 1962. "The Changing Urban Structure and Postwar Immigrants, with Special Reference to Toronto's Italian Population."

Donald B. Williams, B.A., M.A. Toronto, 1961, 1962. "A Comparative Study of Deviant Behavior among Ethnic Groups in Ontario."

UNIVERSITY OF UTAH

Frank Q. Sessions, B.S. Idaho State College, 1960; M.S. Idaho, 1962. "Factor Study of a Stable Rural Community."

VANDERBILT UNIVERSITY

Elmora M. Matthews, B.A. North Texas State College, 1943; M.A. George Peabody College for Teachers, 1959. "Structure and Stress in Two Rural Neighborhoods."

WASHINGTON UNIVERSITY (ST. LOUIS)

Neil Hamilton Cheek, Jr., B.S. Minnesota, 1959. "Social Change and Organizational Integration: The Evolution and Development of New Departments in Complex Organizations."

David Reese Schmitt, B.A. Miami (Ohio), 1960; M.A. Washington (St. Louis), 1962. "A Study of the Invocation of Moral Obligation."

Glaucio Ary Dillon Soares, B.L.L. Faculdade de Direito Candido Mendes, 1957; M.A. Tulane, 1959. "Economic Development and Political Radicalism."

UNIVERSITY OF WASHINGTON

Jeannette Folta, B.A. Boston, 1959. "Role Performance and Role Perception in a Situation Which Threatens a Social System."

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Joseph Jones, A.B., M.A. Washington (Seattle), 1960, 1962. "Structural Ambiguity: Its Occurrence and Effects in Hospital Settings."

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WASHINGTON STATE UNIVERSITY

Robert H. Coombs, B.S., M.S. Utah, 1958, 1959. "A Sociological Analysis of Washington State University Freshman Admission Procedures."

Gordon D. Morgan, A.B. Arkansas Agricultural and Mechanical College, 1953; M.A. Arkansas, 1956. "Assimilation of Nigerian Students in American Colleges."

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WESTERN RESERVE UNIVERSITY

Alan J. Crain, A.B. College of Wooster, 1955; M.A. Western Reserve, 1959. "Mother-Child Interaction and Social and Medical Performance of Diabetic and Non-diabetic Children."

UNIVERSITY OF WISCONSIN

John Michael Armer, B.A. Whittier College, 1959; M.S. Wisconsin, 1962. "Status Ascription and Educational Aspiration in a Metropolitan Community."

James Suhil Bang, B.S., M.S. Mississippi State, 1956, 1957. "Historical Trends of Urbanization and Their Impact upon Selected Aspects of Rural Life in Wisconsin, 1900–1960."

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Jerry Don Rose, B.S., M.Ed. Oklahoma, 1954, 1954. "Impressive Qualities and Social Reactions to Deviants."

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Elizabeth Casper, B.A. Bryn Mawr College, 1958; M.A. Chicago, 1960. "Parents, Spouses, and the Chronically Ill."

Lucy H. Conant, B.A. Radcliffe College, 1947; M.P.H. Harvard, 1957. "The Role of the Visiting Nurse: Interactive Processes with Patients."

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Joel Nelson, B.A. Columbia, 1959. "Primary Relations in the Nuclear Family."

BOOK REVIEWS

Quantification: A History of the Meaning of Measurement in the Natural and Social Sciences. Edited by HARRY WOOLF. Indianapolis: Bobbs-Merrill Co., 1961. Pp. 224.

Liberation from cant is Thomas Kuhn's precious gift to us in "The Function of Measurement in Modern Physical Science," the third of nine essays in this symposium reporting a Social Science Research Council conference of 1959. As if for the reader's convenience, in the first essay S. S. Wilks manages to condense into eight pages most of the standard cant on measurement in science. Then A. C. Crombie in the second essay appears to substantiate the cant by his account of medieval physics, but this account is so knowledgeable and incisive as actually to set up the pins for Kuhn's strike. The purely theoretical analysis, especially in mathematical model-building, which Wilks decries and which Crombie shows to be characteristic of medieval physics, and hence presumably retrograde, comes alive again in Kuhn's account.

Again and again Kuhn drives home his thesis:

> Because most scientific laws have so few quantitative points of contact with nature, because investigations of those contact points usually demand such laborious instrumentation and approximation, and because nature itself needs to be forced to yield the appropriate results, the route from theory or law to measurement can almost never be travelled backwards. Numbers gathered without some knowledge of the regularity to be expected almost never speak for themselves . . . [p. 44].
>
> When measurement is insecure, one of the tests for reliability of existing instruments and manipulative techniques must inevitably be their ability to give results that compare favorably with existing theory. In some parts of natural science, the adequacy of experimental technique can be judged only in this way. When that occurs, one may not even speak of "insecure" instrumentation or technique, implying that these could be improved without recourse to an external theoretical standard [p. 42].

Then what establishes the validity of a theory or law? Kuhn eschews ideology and looks at the social reality: at a given time a theory is true because the community of scientists says it is. The tame data in the textbook table "confirming" a law is a "good fit" only on the authority of the scientific community—there is no external standard of goodness of fit. Measurement indeed plays crucial roles: it permits the exploitation of new theoretical discoveries, and it provides accepted rules for the struggle between partisans of an old and a new theory. But exploitation was so efficient and struggle so orderly in physics only because theory became both intricate and unambiguous through extensive mathematization.

Like any good sociologist (we can claim him as kin in approach if not in training) Kuhn is doubtful (n. 10) about the wisdom of letting the subjects know how they actually function. But social scientists certainly need this insight into how physical science works. For the one common core in our "methodology" training is a course in statistics. The clear implication of Kuhn's account in my opinion is that statistics —the magic machine that works backward from numbers to substance—is bad training for science. Not that we can blame statisticians instead of ourselves. Wilks's essay is no doubt valuable to men concerned with practical problems of measurement in applied fields (all his examples are of this type)—measurement as the basis for decision. But let us dispense with this cant that general, arbitrary techniques of experimental design and data reduction are important to the development of a particular science. Such procedures as "identifying causes" through analysis of variance techniques have no role to play in the development of rich, unambiguous theory in sociology.

Lazarsfeld's essay brings alive in a masterful way the social and intellectual climates in which early systematic research in social science developed in Europe, as well as the politicking and machinations among the chief intellectuals and their sponsors. He poses with clarity fascinating questions in the sociology of knowledge, but for answers he substitutes simple juxtapositions buttressed with "of course." "The Englishman . . . looked for causal relations between quantitative variables. The German . . . tried to derive

systematically the best set of categories by which a state could be characterized" (p. 155). Why this difference? "The critical German problem of the time was civic reconstruction [after the Thirty Years' War]. Problems of law and of administration had high priority . . . competition between the principalities. . . . International law started a few miles from everyone's house or place of business. No wonder then that it was a spirit of systematically cataloguing what existed, rather than the making of new discoveries that made for academic prestige" (p. 157). This and other fragments of his answer seem to me to be non sequiturs.

Later, in his discussion of the Belgian Quetelet, Lazarsfeld seems to be in a bit beyond his depth or at least beyond the existing state of knowledge in his dicta on what "mathematical sociology" (his phrase) has proved and his hints as to what it should tackle. Occasionally Lazarsfeld descends to the use of stereotypes: "Quetelet assumes a *deterministic* relation between the hypothetical construct and its manifestations. This obviously derives from his training as a natural scientist. . . . But in the social sciences, indicators or symptoms have a *probabilistic* relation to the underlying propensity" (p. 176). Probability concepts often are used only temporarily to bridge gaps in understanding or else simply as a convenience to avoid unrewarding attention to dull detail; there well can be an underlying deterministic relation.

Lazarsfeld concludes with a highly stimulating section focused on LePlay.

HARRISON WHITE
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Types of Formalization in Small Group Research. By JOSEPH BERGER, BERNARD P. COHEN, J. LAURIE SNELL, and MORRIS ZELDITCH, JR., with Foreword by SANFORD M. DORNBUSCH. Boston: Houghton Mifflin Co., 1962. Pp. 159. $4.50.

This tiny book asks, "What can a mathematical model do?" The authors suggest three answers and present a detailed description of a model as an example in each case. The mathematical exposition has been kept to a minimum, and non-mathematicians should find the book quite readable.

Drastically oversimplified, the three answers are as follows: If there is very little theory available to explain some data, a simple mathematical model can be used to determine the most useful and economical set of variables to describe a process, and the most sensible ways to deduce the values of these variables from the data. The example given is from Cohen's forthcoming book, a formalization of the Asch conformity experiments. If the beginnings of a theory are available, a translation of the theory into statements in formal logic may help clarify the conceptualization, and this is exemplified by Harary and Cartwright's use of graph theory to describe Heider's balance theory. Finally, for that rare situation in which a respectable theory exists, a model can be written to test the theory. Here the example is the Estes-Burke application of a Markov chain to learning theory.

One can argue with this typology, but this is probably not important; the typology will be useful if it helps the rest of us to get straight our notions of what a model can do. A primary intention of the book is probably to sell model-building to those whom Dornbusch calls either "curious" about models or "frankly hostile" to them, and it should be rather successful in doing this. For those already interested in models, the well-done annotated bibliography of forty-one articles (restricted to social-psychological models) should be worth the price of the book alone.

The book should do a good bit of marriage counseling between those of us who love models and our colleagues who view them with a mixture of awe and hatred. The careful definition of what model-building is, and particularly the use of the word "formalization," should help us realize that the distinction between mathematical and non-mathematical work is not as great as it first appears to be. The authors demonstrate that models can be used in a variety of different ways to solve various types of problems and can be applied at different stages in the research process. The non-mathematical reader will be somewhat reassured to discover that the authors not only know, but pay attention to, the work of "non-deviant" social psychologists. One criticism is that the authors, in reporting on three rather good models, do not criticize the bad work that has been done in this field. Thus, the innocent reader might conclude that the models literature is generally good and useful, whereas the exact opposite is probably closer to the truth. Someone should shoot down some of the more

useless work, not for the purpose of entertainment, but in an effort to spell out the limitations of formalization. For example, Cohen had to simplify the already simple experimental techniques used by Asch, and the authors note that, "although a number of studies have made use of the Asch situation, it is not possible to construct a model for any of the existing versions of his experiment." This is a rather frightening remark, and should have been amplified.

ROBERT L. CRAIN

University of Chicago

Simulation in Social Science: Readings. Edited by H. GUETZKOW. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1962. Pp. xv+199. $5.25.

The problem of studying highly complex processes in a systematic fashion has brought a number of social scientists to the use of simulation. Complex processes involving individuals, groups, organizations, or nations have been represented by models in which three-man groups may act as nations, or computers enact a theory of thinking.

Like statistics, simulation has acquired different forms in different social sciences. Some simulations employ men, others computers, and some both. Man-computer simulation may be conducted in "real time," as in the instance of air defense, where human decisions and computer production of new decision problems are intermixed in a continuous sequence. Computers may, in Monte Carlo procedures, inject random error into a process, reproducing uncertainty.

The striking diversity of approach and field in this volume seems natural for an editor who is a professor of political science, psychology, and sociology, and has worked in all three areas, particularly their intersection. (Guetzkow's own extensive work, with his colleagues at Northwestern, on internation simulation has also just been published by Prentice-Hall.) In the present volume, following an introductory chapter in which Robert Dawson reviews definitions, uses, advantages, and problems of simulation, thirteen chapters exhibit applications in psychology, sociology, political science, economics and business, education, industrial engineering, and military operations. This considerable scope is best indicated by listing authors, titles, and original sources from which reprinted or adapted:

Chapter ii: Carl I. Hovland, "Computer Simulation of Thinking" (*American Psychologist*, 1960).

Chapter iii: Jack A. Adams, "Some Considerations in the Design and Use of Dynamic Flight Simulators" (*Air Force Personnel Research and Training Center Research Report*, 1957).

Chapter iv: Morris Zelditch, Jr., and William M. Evan, "Simulated Bureaucracies: A Methodological Analysis" (new).

Chapter v: James S. Coleman, "Analysis of Social Structures and Simulation of Social Processes with Electronic Computers" (*Educational and Psychological Measurement*, 1961).

Chapter vi: Ithiel de Sola Pool and Robert Abelson, "The Simulmatics Project" (*Public Opinion Quarterly*, 1961).

Chapter vii: Harold Guetzkow, "A Use of Simulation in the Study of Inter-Nation Relations" (*Behavioral Science*, 1959).

Chapter viii: Guy H. Orcutt, "Simulation of Economic Systems" (*American Economic Review*, 1960).

Chapter ix: K. J. Cohen *et al.*, "The Carnegie Tech Management Game" (*Journal of Business*, 1960).

Chapter x: Norman Frederiksen, "In-Basket Tests and Factors in Administrative Performance" (*Educational Testing Service Invitational Conference*, 1960).

Chapter xi: Donald G. Malcolm, "System Simulation—a Fundamental Tool for Industrial Engineering" (*Journal of Industrial Engineering*, 1958).

Chapter xii: Normal H. Jennings and Justin H. Dickins, "Computer Simulation of Peak Hour Operations in a Bus Terminal" (*Management Science*, 1958).

Chapter xiii: R. P. Rich, "Simulation as an Aid in Model Building" (*Operations Research*, 1955).

Chapter xiv: Robert L. Chapman *et al.*, "The Systems Research Laboratory's Air-Defense Experiments" (*Management Science*, 1959).

Because of the diversity of approaches and fields represented, this reader, in collecting some of the more outstanding simulations in the social sciences, serves not only the student but also the researcher seeking new ways of studying complex processes. Out of studies such as these may come leads for further exploiting the value of simulation for the social sciences.

JACK SAWYER

University of Chicago

Ouvriers d'origine agricole ("Workers of Agricultural Origin"). By ALAIN TOURAINE and ORIETTA RAGAZZI. Paris: Laboratoire de Sociologie Industrielle de l'École Practique des Hautes Études, 1961. Pp. 128.

This study compares the orientations of four groups of workers in two large automobile factories in Paris: (1) semiskilled young workers of rural origins ($N=45$), (2) semiskilled young workers of urban origins ($N=41$), (3) skilled workers ($N=38$), and (4) young apprentices to middle-class occupations ($N=34$). These groups are chosen to give a range of types of career experience and of promotion chances. Since careers are a central motivation of work discipline in bureaucratic enterprises, the study throws considerable light on the sociology of work.

The semiskilled groups who are objectively in low positions now, without much chance of success within the enterprise, but which differ in rural and urban origins, bear the brunt of the analysis. The first part of the monograph studies the career orientations of the respondents. All three groups of workers of urban origin are more likely to conceive of success as the result of a systematic life-plan that has to overcome serious obstacles in the educational system and in changing one's style of life to a middle-class style. The workers of rural origin interpret success more in terms of money, conceive their present unfortunate situation as due to bad luck, and underestimate both the obstacles to success and the style-of-life differences between the middle and working class. They are more oriented to entrepreneurial success than workers of urban origin.

This means that semiskilled workers of urban origin conceive of their life-chances as being bureaucratically organized, by education and training, and conceive of themselves as badly situated in the system that they conceive to exist. The workers of agricultural origins also think themselves to be badly situated, but not because the system has rejected them. Instead their bad luck prevented their making as much money as they might. Some sources of these attitudes in the family of origin and consequences for their own child-rearing are suggested.

Clearly not only American automobile workers dream. But we now know the French dream is concentrated among workers of rural origin.

In the rest of the monograph the orientations of these groups toward technical progress and toward the social class system are outlined, and many of the findings are quite intriguing (for instance, workers of agricultural origins are more likely to define the word "bourgeois" as those who do not work; p. 73).

These attitudes toward progress and class are interpreted by reference to the career orientations found earlier, that is, by explaining the proportion of a group giving a certain response on the impact of improved machinery by the proportions of the same group giving a certain response on a question of career orientations. The responses on the two questions for each individual, cross-classified, are never reported. Much less is there cross-tabulation under controls. The authors have, quite properly for an exploratory small-sample study, intuitively picked a level of significance for their analysis considerably lower than the conventional 0.05 level, somewhere near 0.15. Differences that do not meet the conventional criterion are used in the analysis. It is merely confusing that the occasional differences that meet the 0.05 criterion have chi-square tests reported, often based on improper collapsing. The authors' statistical intuition is better than their formal competence, and they should have trusted it all the way through.

A general question in the strategy of exploratory research with small samples is brought up by this study. If a sociologist knows he will be exploring the relations among several variables and can only afford a small sample, the value of increased precision of the measures of crucial variables is very great. If one has a cardinal measure of a confounding and a dependent variable, and uses the independent variable (whether measured by a dichotomy or not) to explain variation around the regression line, he only uses up two degrees of freedom in making the control. The same type of improvement comes with ordinal measures of crucial variables to a slightly smaller degree, involving more complicated statistics. A small investment in greater precision of measurement of crucial variables thus greatly increases the power of a small sample study for multivariate analysis. If one can afford a great many cases, the only disadvantage of less precise (e.g., dichotomous) measures is that one does not know whether the confounding variable is really controlled or not. At least there are enough cases in the various cells to see whether the confounding variable washes out or increases the correlation of the independent with the dependent variable. Psychologists, who must work mostly with small samples, have quite properly invested heavily in precision of measurement so as to avoid the running-out-of-cases problem.

It seems that a small investment in more

precise measures of two or three variables and more cross-tabulation of responses would have greatly increased the value of this already very informative study.

ARTHUR L. STINCHCOMBE

Johns Hopkins University

The Worker in an Affluent Society. By FERDYNAND ZWEIG. New York: Free Press of Glencoe, 1961. Pp. xvii+268. $4.50.

This study about the mutual impact of family and work life in industry stands in the long tradition of social surveys on the living conditions of the British working class. It is based on 672 interviews gathered between May, 1958, and July, 1959. The sample is taken from factory workers—unskilled to supervisors—in five large, well-conducted British firms from prosperous and expanding branches of industry. There is no claim that this sample is nationally representative.

Zweig's main interest is the married worker. Female workers were interviewed in only one of the firms. They are treated in a separate chapter, as are various categories of single men and men approaching retirement. The character of the study is descriptive. None of the few generalizations that the author attempts at the end refer specifically to the core topic—the mutual impact of family and work life. However, a host of relevant conclusions emerges from the presentation of the data. Typical examples are: the fact that family upsets affect work more strongly than vice versa; that a wife's willingness to adjust is of crucial importance for the worker's acceptance of shift work; that marriage motivates the worker to perform well and to look for stable and lucrative jobs, sometimes at the cost of improving his skills; that better housing conditions and TV have contributed to the worker's home-centeredness, etc.

In the course of his inquiry Zweig was impressed by what he felt to be an important social change in the life-style and attitudes of the worker under the impact of postwar development. Thus the study transcends its narrower original topic and attempts to outline the trend of this change. From Zweig's description emerges the rather familiar picture of a worker who enjoys a greatly improved standard of living, is satisfied with his work, is security-minded, and is strongly acquisitive. This worker is very much home-centered; he marries early, plans his family, is a happy husband and interested father with strong ambitions for his children. The pattern of family relations has changed from the authoritarian to the partnership model, a change reinforced where the wife goes out to work herself. Contact with neighbors, co-workers, and more distant relatives follows the maxim "friendly, but not too close." For all his conformity, this new type of worker is an individualist and hardly class conscious. The only negative feature that Zweig finds is the very limited cultural interest and participation of the worker.

For all the inherent interest of the material, the value of the study would have been greatly increased if the author had put his findings into the context of what is already known about the subject of the "new" working class. Nowhere does Zweig compare his results with those of other relevant studies, either to point out differences and agreements or to support those of his conclusions that, because they are marginal to his main topic, are based on somewhat scanty evidence. This last remark is particularly pertinent with respect to the subject of class consciousness.

A related shortcoming of the study lies in the fact that, in outlining the trend of social change, Zweig's base line for comparison remains rather unclear. In the beginning he refers to his earlier book *The British Worker* (Penguin, 1952), but he uses neither this nor any other study for a systematic comparison. Instead he contrasts his data with the familiar image of an oppressed and embittered working class, living in poverty, threatened by unemployment, burdened by too large families, families where the father bullies wife and children and is given to drink, making the pub his home. The highly general and negative image of the past occasionally seems to have seduced the author to paint the present in too glowing colors, even where the data would have lent themselves to a different interpretation. A case in point is his conclusion that except for "odd cases," shift work is no longer a problem (p. 65), while his data show that a substantial group of about 43 per cent are more or less upset and disturbed by it (p. 54).

The few generalizations that Zweig attempts at the end are primarily of a sociopsychological, if not psychological, nature, and do not strike the reader as being novel. Thus Zweig refers to the principle of ambivalence, that is, the fact that man's attitudes are often ambivalent; and

the principle of projective generalization, which means that respondents tend to impute their own attitudes to others or to "man" in general. More sociological in nature is the generalization about "hedgehog behaviour" that refers to the maxim of "close but not too close" and is attributed to the fact that the workers are comfortably off, do not need the solidarity of kin or class, but desire freedom to enjoy their life undisturbed, finding the needed warmth in their happy families.

The two other generalizations are closely connected. One, called the homeostatic principle by Zweig, refers to his observation that aspirations adjust to reality, making for a rather equal level of perceived satisfaction even under objectively quite different conditions. The second is the principle of "as it is": people can adapt themselves to practically anything and having done so prefer things as they are, the status quo, to a change. Zweig evaluates the noted adjustment mechanism very positively, calling a person with full power of adjustment healthy-minded (p. 191). He seems to forget that dissatisfaction and the will to change circumstances rather than adjust to them is the motor of social change without which the improved living conditions of the working class would not have been achieved. It would be a fact of far-reaching implications if the British working class—or the worker in the affluent society in general—were characterized by an adjustment tendency and resistance to change to the exclusion of contrary tendencies. Zweig does not maintain that this is so; he simply does not discuss these implications of his study. But one should probably not blame the author for not doing what he never intended to do. Zweig admits that his intentionally chosen descriptive approach has limitations. These limitations are quite obvious all through the book.

RENATE MAYNTZ

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The Emerging City: Myth and Reality. By SCOTT GREER. New York: Free Press of Glencoe, 1962. Pp. v+232. $5.75.

The Emerging City is a valuable book, less because of the jacket claim that it "reformulates our perspective" on the city and more for its fluently coherent yet concise arrangement of hypotheses about the social functions and ecological processes of contemporary American urban regions. The book is useful as a common reference for the specialties at work on urban problems, as a source of significant and testable ideas for social research, and as a supplementary text for courses in urban sociology, planning, state and local government, and location economics.

In seven terse chapters, Scott Greer has integrated several of his earlier papers with helpful additional materials. He begins by contrasting contending conceptions of the city; counters with his own essentially ecological construction; and relates his image to what we know about decision processes and power structures. His formulation underscores the political nexus of all urban social problems. Emphasis on the functions urban regions serve in the larger national system strengthens his concluding treatment of the demographic and economic trends that will soon transform most problems of urban regions into primarily federal issues.

The blend of demography and ecology with political behavior research is especially refreshing and gives *The Emerging City* the quality of contributing to a kind of policy science. This advantage has its costs. Political scientists will be dissatisfied with Greer's incomplete coverage of (and possibly insufficient discrimination among) studies of community power structure. Urban sociologists are apt to bemoan the absence of a comparative approach, as the hypotheses and their illustrations are fitted rather strictly to the American case. In addition, the book strains at points toward too much verve. Metaphors mix with special jargon. For example: "The pattern of neighborhoods in the city may be imagined as a galaxy, located in an attribute space. . . . The galaxy leans toward the familistic end of the continuum of life styles" (p. 82). Still, for this reviewer, this is one book about urbanism that glows, though it does not burn like a morning star.

ROBERT A. DENTLER

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The Government of Corporations. By RICHARD EELLS. New York: Free Press of Glencoe, 1962. Pp. xvi+338.

This book is an excellent treatment of the corporation as a private state with a still-hazy relation to public law. As the conception of a

kind of dollar democracy through stockholder control becomes increasingly fictitious, questions concerning the institutional position of the large business corporation in American society have multiplied faster than the answers. The author reviews the views of both analysts and publicists in dissecting the issues of corporate power and control.

Eells traces the corporation's relations with "external" interests—such as unions, suppliers, stockholders, and customers—and the way these interests and their partial controls are evolving in both law and practice. He has a somewhat less extensive discussion of the exercise of corporate power over employees. By concentrating his intramural attention on the "traditional" political concerns of constitutional powers and the rights of citizens, Eells understates the significance of organizational structure and the influence of the organization as such among the "interests" to which executives are attentive. The internal environment, in other words, is relatively neglected.

With respect to interests and accountabilities, Eells misses two points that would have helped in further clarification of the situation: (1) Most of the environing interests hold veto powers over managerial actions beyond some limits. Executives exercise nominal autonomy within the boundaries, but the boundaries are in fact neither stationary nor clearly demarcated. (2) The organizational structure reduces the hierarchical power of executives, for they are dependent on expert information about and liaison with constituencies. Constituencies are represented *within* the managerial group by staff functions. The legal unity of the corporation is substantially dissipated by an examination of its social organization.

This is not a book for scanning. Though the author is articulate, scholarly, and generally reasonable, the exposition seems to me unnecessarily prolix. Topic sentences commonly run to five or six lines, with a structure that is semantically accessible and grammatically sound, but rather loosely strung together with qualifiers—sentences, in other words, like this one. In two words, no bite. But nevertheless, in two more words, good book.

WILBERT E. MOORE

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Public Opinion and American Democracy. By V. O. KEY, JR. New York: Alfred A. Knopf, Inc., 1961. Pp. xiv+566.

This is a book well worth the consideration of every student of public opinion. For classroom purposes, it is the best text treatment of public opinion in print, and it has certain chapters that will be of value as collateral reading in connection with other subject matter.

One might best summarize the content by dividing it into the familiar, the new, and the redevelopment of some old notions that have been unwisely dropped from texts appearing in recent years. In the realm of the most familiar content, we find discussions of the standard correlates of public opinion response—distribution by region, by socioeconomic status, education; the influence of the family and the mass media; a discussion of various dimensions of public opinion, such as intensity and stability. The source materials for these discussions are taken from the data files of the Michigan Survey Research Center, and constitute the most up-to-date resources currently being utilized by a public opinion text.

The most important new development in the text is the discussion of the effect of public opinion on the decision-making process, and of the channels through which public opinion comes to the decision-makers. A major shortcoming of the study of public opinion in the past has been that it all too often treats opinion only as a *dependent* variable; that is, the discussion has revolved around the social and psychological processes by which opinion comes to be formed by individuals, to the exclusion of the political processes by which aggregates of opinions come to influence the actions of individuals at the command posts. As Herbert Blumer pointed out some years ago, public opinion polls can hardly be used as reliable predictors of collective action until detailed consideration is given to the process by which collective sentiments are translated into action.

Another new aspect of the book is the attempt by the author to create, out of indexes of political participation and involvement, a concept of "political stratification" that does not, in the opinion of this reviewer, come off very well. It would seem that nothing new or suggestive is added to the frame of reference by thinking of differential participation and psychological involvement as constituting a stratification of a public. Perhaps the reviewer's sociological bias is showing badly here, but he tends, following Weber, to think of political stratification in terms of individual or factional power or influence. There are numerous studies of community and national power structures

and of interpersonal influence structures that could be worked into the frame of reference for studying public opinion, under the rubric of political stratification. These studies receive only the most cursory attention. The presentation and analysis of materials from the Survey Research Center is a most admirable feature of this book, but the author might have made more use of studies, conducted under other auspices, about subjects relevant to the study of public opinion.

We have mentioned that the book contains certain revivals of past approaches to the study of public opinion. We have in mind specifically the penetrating discussion of the interpretation of the marginals of public opinion polls that is presented in the first few chapters. While no one wishes to disparage the valuable results that have issued from the practice of cross-tabulating poll responses against predictor variables, the fact remains that some interesting implications can be drawn from the sheer distribution of the response, relevant to the nature of the consensus that exists with respect to the issue at hand. In his discussion of the interpretation of these marginals, Key develops a very suggestive differentiation between "permissive consensus" and "directive consensus" that will be of great interest to students of public opinion.

So much for the book that Key wrote. What remains, then, is to develop a methodological criticism that may be applied to all books of this genus. This has to do with the basic conceptual image of public opinion that developed in the classic treatments by Bryce, Dicey, Lowell, and Lippmann. In the naïve and non-empirical early days, it was more or less taken for granted that people thought about and discussed public issues in the same way as those who analyzed public opinion. The polls brought disillusion in this respect, for they showed that substantial proportions of the general population were neither deeply opinionated nor informed about issues that seemed to be of greatest moment. To find this out, it was necessary for the pollster to prepare a list of items for commentary by the respondent. Somehow, however, the public opinion researchers never reacted properly to the knowledge that many of the items on their lists were not deemed of sufficient importance to occupy the attention of large proportions of the general population and, to this day, "issues" continue to be defined almost entirely in terms of the orientation of the analyst, rather than by the population under study. It would be of great value to know what kinds of news (for this is the basis of all general public discussion) people *do* react to and talk about and think about, and it would be of even greater value to know *why* they react to these events in addition to, or to the exclusion of, the kinds of events they "ought" to be discussing. Such a behavioral orientation could serve as a valuable supplement to the classical image of public opinion.

WILLIAM ERBE

State University of Iowa

The Man on Horseback. By S. E. FINER. New York: Frederick A. Praeger, Inc., 1962. Pp. 268. $6.50.

Starting with the fact that insubordinate armed forces represent one of the more common methods for transferring political power, Professor Finer has gone on to ask why and has written an informative and provocative book. In the process of doing this he has surveyed and catalogued the known instances of military intervention in the twentieth century. The subject is itself of major importance and has not hitherto been systematically explored. On this basis alone, the author has done well by his readers.

It is on the basis of its arguments, however, that this book is meant and deserves to be considered, and they, in simple fashion, can be summed up in the assertion that every country gets the degree of military intervention it deserves. The military in every modern nation is by virtue of its monopoly of force and its superior degree of organization and discipline always in a position to take over the mechanism of government (pp. 6–14). That it does not always do so is attributable to the fact that force itself cannot administer anything but the most primitive of communities, and the belief, increasing in intensity as the society grows more complex, that a clear moral title is indispensable to the ruler. "Where public attachment to civilian institutions is strong, military intervention in politics will be weak" (p. 21). If the military does not intervene it is because it knows that it needs civilian assistance to do so successfully and has reason to believe that such assistance will not be forthcoming.

This being the case, civil-military relations in various societies are classifiable in terms of the general degree of civic commitment. The higher the level of "political culture," that is, adherence to fixed procedures for the acquisition

and retention of public office, belief in the legitimacy of the existing office-holders, and the widespread existence among the general public of a cohesive network of well-organized voluntary associations, the less active and open the intervention of the armed forces into public life. Finer devotes the bulk of his book to the examination of specific military revolts in the light of a classification of this kind: Countries of Developed Political Culture, for example, France, Germany, Japan; Countries of Low Political Culture, for example, Argentina, Turkey, Egypt; Countries of Minimal Political Culture, for example, Haiti, Iraq, Sudan. As "culture" and legitimacy decline, military influence changes into blackmail, then into the displacement of civilian cabinets, and finally into an outright military regime.

In sum, Finer argues that insofar as the modern state is concerned civilians owe whatever liberties they possess to their own efforts. This thesis runs counter to that expounded by Professor Huntington. The latter is indeed attacked, rather effectively, it seems to me (pp. 24–25). This may be the beginning of a useful controversy.

M. D. Feld

Center for International Affairs
Harvard University

They Shall Take Up Serpents: Psychology of the Southern Snake-handling Cult. By Weston La Barre. Minneapolis: University of Minnesota Press, 1962. Pp. 208. $3.75.

This book by Mr. La Barre, a professor of anthropology at Duke University, ranges much wider than its title would suggest. The author has done field work and he describes the Southern snake-handlers in reportorial detail: there are even reports on what particular cultists wore and how tall they were. Excellent case histories of cult leaders are included, along with a perceptive analysis of their followers. But most of all the book reveals a wide knowledge of anthropological and psychiatric research. It is an unusual combination of factual reporting and learned scholarship and is very well written. Both the lay reader and the professional anthropologist will find it interesting and profitable.

It is difficult to describe the book. It is not a monograph on psychiatry, though it applies acute psychiatric insights to a new area of phenomena. It is not really a study in the sociology of religion; except for an excellent but undocumented description of Southern mill workers, it employs almost no sociological methods. Perhaps the most accurate categorization would be that the volume is a pioneering and definitive study of herpetological religion.

A comprehensive report is given on the Southern snake-handling cult, especially in Tennessee, Kentucky, North Carolina, and Florida. But there is also much material on the practices and symbolism of similar cults since the Stone Age, with special attention to ancient Israel, Greece, Rome, Egypt, Africa, and more recent ethnographical data. The author argues the question of diffusion versus independent parallelism; he does not resolve it, but leans toward the theory of diffusion. It is quite clear that the Southern cult is of Semitic origin, drawn immediately from the Bible, though influences from Mexico may also have been of some significance.

In psychiatric terms La Barre interprets the snake-handlers as being, consciously or not, a phallic cult. He points out: "Man has more myths by far about snakes than about any other single animal. Small wonder, when the snake is man's own sexuality." At times his analogies between serpents and sex are debatable, but on the whole he presents a convincing analysis.

Liston Pope

Yale University

The Sociology of Science. Edited by Bernard Barber and Walter Hirsch. New York: Free Press of Glencoe, 1962. Pp. viii+662. $8.50.

This reader reprints thirty-eight articles and selections from books on social aspects of science. The selections originally appeared since 1945, except for an essay by Max Weber and three papers by Robert Merton. The title of the volume belies the fact that this is an interdisciplinary collection, with contributions by natural scientists, historians, and philosophers, as well as social scientists. The editors give proper recognition to the pre-eminence of Merton's work in the field by devoting over one hundred pages to selections from his writings. The range of topics is indicated by the headings of the six parts of the reader: "The Social Nature of Science and the Scientific Role"; "Science and

Society: Reciprocal Relations"; "The Social Image of the Scientist and His Self-Conceptions"; "The Organization of Scientific Work and Communication among Scientists"; "The Social Process of Scientific Discovery"; and "The Social Responsibilities of Science." The editors provide short introductions to each part. A selected bibliography covering the period from 1952 through 1961 is included.

Almost all of the selections are concerned with the physical and biological sciences. Social science by and large is ignored. In this respect, the sociology of science is following the philosophy and history of science. Perhaps in the future it may lead the philosophy and history of science by conducting research on social science itself.

The best selections are those that analyze social aspects of science historically; notably Merton on science in seventeenth-century England, Charles Gillispie on science in the French Revolution, Thomas Kuhn on simultaneous discovery in the nineteenth century, and Joseph Ben-David on nineteenth-century medicine. These papers excel in linking important scientific developments to various aspects of the societies within which they occurred. Perhaps their greatest strength lies in the effort to treat scientific *ideas* seriously, and to relate rather than reduce them to cultural and sociohistorical contexts.

An issue that recurs through many of the selections is the impact of pressures for the solution of practical problems on the development of science. A closely related issue concerns the independence and control of scientific activity. The relation between the nature of the demand for scientific results and the modes of control over scientific activity is more complex than generally assumed. Three papers on Soviet science illustrate this point.

All in all, a very stimulating volume on a most difficult and important subject.

WILLIAM KORNHAUSER

University of California
Berkeley

Research in Family Planning. Edited by CLYDE V. KISER. Princeton, N.J.: Princeton University Press, 1962. Pp. xv+662. $12.50.

In October, 1960, a conference on Research in Family Planning was held in New York under the joint sponsorship of the Milbank Memorial Fund and the Population Council, Inc. Persons involved in the relevant field studies were included among the participants, as were many working in action programs. Thus there was a widely dispersed representation with reference to residence, area of work, and field of specialization. The proceedings were edited by Clyde V. Kiser, whose skilled, affable, and unstinting effort had contributed so much to the organization and functioning of the conference.

The first five of the ten sections are summaries of research studies and statements of outlook by responsible officials of governments or family-planning movements; they concern India, other Asian countries, the Middle East, the United States and Latin America, and Europe. The final five sections concern research in methods of fertility control, the acceptability of methods, problems of measurement, problems of research design, and problems of motivation and communication. There is then a summary of the conference.

A résumé is not possible in brief scope, and selective mention would be invidious. The conference and the collected papers are a significant point in the emergence of population growth as a problem of governments and in the use of research and experimentation as guides to the formulation of policy and action programs. The fact that the results of field research were presented from Asia and Latin America as well as the countries of advanced modernization is new; so also is the theoretical, speculative, and emerging empirical study of communication and motivation as bases for effective action programs.

These are the positive assessments. There are negative evaluations that must be made, however. Field studies are limited in number and in scope, closely patterned in approach, and, if experimental, remarkably alike in the limited impact on the fertility of the women in the experimental areas. The statements on communication and motivation are more properly sociological in theoretical approach, but field experimentation alone can demonstrate the effectiveness of activities derived from their assumptions in reducing fertility. The report on research in method indicates advances, but again the development is slow, and, given the path of advance from basic science to widely used technology, necessarily so.

Research in Family Planning is an invaluable addition to teaching materials in demographic

and related courses and seminars. Field studies in India, Ceylon, and Latin America previously unavailable except in fugitive monograph form, if at all, are here summarized. The major recent studies in Europe and the United States are also noted. Problems of method, measurement, and research design are given brief and nontechnical treatment. And the problems of motivation and communication in today's underdeveloped areas are brought into research and experimental focus as contrasted with traditional planned-parenthood approaches.

IRENE B. TAEUBER

Office of Population Research
Princeton University

Handbook of Small Group Research. By A. PAUL HARE. New York: Free Press of Glencoe, 1962. Pp. xiv+512. $10.00.

There are times in the history of every scientific discipline when people interested in the particular area feel a need for stock-taking. Confronted with an ever increasing number of journal articles and research reports, they ask: What do we now know? What facts have we accumulated? Then, taking up the challenge, some scholars set out to perform the encyclopedic task. They write (or, more frequently, edit) a handbook.

Such endeavors surely are necessary, and their importance is widely recognized. Witness the popularity of—to cite just two of many possible examples—the *Psychological Bulletin* and the *Annual Review of Psychology*. Most investigators do not have either the time or inclination to make a comprehensive search of the literature in their particular area. If they are to obtain maximum benefit from the research efforts of others, they must be provided with occasional surveys of their field. A. Paul Hare has attempted to satisfy just such a need in his *Handbook of Small Group Research*. Here, say his publishers, is a review "of all of the relevant research in the [small group] field from 1900 to 1959." The bibliographic effort is indeed prodigious; 1,385 titles are cited, covering such diverse topics as the study of first impressions (this material is now out of date), communication networks, and street-corner gangs.

A compendium of facts is not very useful, obviously, unless it is organized in some satisfactory fashion. Categories must be constructed within which data can be grouped together, and there must be some integration within and between categories. Hare employs twelve relatively broad classes organized within three somewhat larger supercategories. Thus, he covers: (1) "Group Process and Structure," which includes such topics as "Norms and Social Control," "Roles," and "Interpersonal Choice"; (2) a very broad class of "Six Variables Affecting the Interaction Process," such as "Personality," "Group Size," "Communication Network," and "Leadership"; and (3) "Performance Characteristics." Anyone interested in these problem areas would do well to consult the Hare *Handbook*. A good deal of his bibliographic research has been done for him. He will know specifically what articles to read—for a start.

If he is wise, however, the reader will not take Hare's capsule statements at face value. While most of the summaries seem to be correct, as far as the reviewer can tell, there are occasional errors and misleading oversimplifications. For example, in discussing public compliance to some opinion without private acceptance of this belief, the author makes the point that "where pressure is exerted on a person to express an opinion different from the one he privately affirms, there is a tendency for him to change his overt opinion," and he cites two studies presumably containing supporting evidence: Janis and King (1953) and Kelman (1950). The first of these experiments deals with role-playing, and Hare apparently is thinking of role-playing as a situation in which the role-player is more or less coerced into expressing views contrary to his initial beliefs. However, the major implication of the role-playing research is that this "overt compliance" under certain conditions produces a change in beliefs consistent with the expressed statements. Generally, and this may be what the author means but does not say clearly, the greater the felt freedom to state the role-played views, the greater the subsequent attitude change. In addition, the Kelman study mentioned here has nothing to do with this topic; Hare probably has in mind an experiment by Kelman (not cited in the book) published in 1953. Further, although the author confines his proposition (taken from Festinger) to situations of threat or restraint, he might have done well to bring up the relevant experiment by Thibaut and Strickland. This study suggests that in otherwise free situations increased pressure for compliance on people who are committed to their initial beliefs

and who are task-oriented rather than being oriented socially often results in greater, not less, overt adherence to the initial beliefs. (To set the record straight, Hare's statement regarding the reviewer's doctoral dissertation is incorrect.)

But it is primarily at the theoretical level that the present volume suffers. A scientist should do more than just collect facts. He should also be concerned with formulating abstract propositions that integrate the available facts. Facts are meaningful to the extent that they can be imbedded in a larger context. Evidently not giving sufficient attention to the problem of such abstract generalizations, Hare sometimes seems to have assembled his references mainly with scissors and paste. To illustrate, in the section dealing with leadership traits the author abruptly goes from a discussion of the authoritarian behavior of emergent leaders to three experiments by Raven and French, demonstrating that elected leaders are more successful in influencing their groups than are leaders who "take over." This latter research, of course, should have been included in the sections on norms and roles. Raven and French sought to show that election to the leadership position "legitimatizes" influence attempts—such behavior is appropriate to the formal leadership role—so that these attempts are more successful than when they are carried out by "ordinary" group members.

All in all, the present *Handbook* can be very helpful to the researcher and the serious student. But the book would have been better if the author had devoted less time to collecting facts and more time to tying them together.

LEONARD BERKOWITZ
University of Wisconsin

Uncertainty and Structure as Psychological Concepts. By WENDELL R. GARNER. New York: John Wiley & Sons, 1962. Pp. vii+369. $8.95.

Despite expressions of disenchantment on the part of many who were initially enthusiastic about the potential of information measurement in psychology, Professor Garner here affirms its power as a method of problem analysis and concept development. The essentials of his position may be summarized as follows: The measure of information (or uncertainty) is statistical in character. Unlike related statistical measures, however, it assumes nothing about the metric properties of variables; it may be applied wherever categories can be distinguished and probability values attached to them. This same quality gives it great mathematical generality; it lends itself to development as a particular case of the multivariate analysis. Finally, a special form of this analysis permits determination of the amount of interrelatedness in a system of variables, providing a means of handling such psychological concepts as structure and meaning in a quantitative manner.

The book is organized in a way that first presents relevant mathematical terms, and then uses them to examine the experimental applications of information measurement in a number of psychological problem areas. It begins with an account of the basic information function and its applications in experiments involving only stimulus or response uncertainty, and proceeds through progressively complex forms of the multivariate analysis. As the specification of uncertainty components for the experimental variables increases, the results improve in their capacity to describe the nature of the behavior under investigation. For example, analysis of experiments employing multidimensional perceptual stimuli (in comparison with those employing only unidimensional stimuli) leads to a statement of general principle concerning perceptual discrimination processes, and also suggests that stimulus dimensionality is an overriding consideration in the design of research in this area. Such outcomes represent, in part, achievement of one major objective of the work, that is, the use of information mathematics as a tool for analyzing not simply data, but problems.

The second objective entails the manipulation of multivariate information analysis terms to formulate three new concepts identifiable as total, internal, and external constraint. These mathematical concepts are set forth as the counterparts of different forms of structure, in the psychological sense, and meaning, insofar as meaning refers to perceived structure rather than signification. The use of these constraint, or structure, concepts in re-evaluating the experimental literature on pattern perception, language redundancy, sequential behavior, and concept formation constitutes one of the most important methodological—and perhaps theoretical—contributions to have appeared in recent years.

MURRAY ABORN
National Institutes of Health

Personality and Decision Processes. By ORVILLE G. BRIM, JR., DAVID C. GLASS, DAVID E. LAVIN, and NORMAN GOODMAN. Stanford, Calif.: Stanford University Press, 1962. Pp. ix+336. $8.00.

The authors set out to investigate the influences of personality, social structure, and the situation, individually and in interaction, on decision processes. After outlining a descriptive model of phases of decision processes, they creatively incorporate the evaluation and strategy-selection phases in a set of questions they call the decision-process test, which they then use to present four child-rearing decision problems to their respondents. Sex and social class breakdowns represent their indexes of social structure; and to measure personality the authors "throw in" a variety of tests designed to measure ability, affect level, beliefs, and a variety of background characteristics.

With approximately two hundred respondents—mostly Jewish—and fifty variables, the "computerizing" begins. Parallel factor analyses are performed for six groups differentiated by sex and social class for the purpose of exploring the stability of factor structures across groups. Personality and decision-situation effects are also viewed, taking into account interaction results among the basic variables. The monograph also deals with comparisons of individual and group decisions, and the fit of the research results with current utility ideas.

It should be obvious that the researchers have ambitiously taken on a complicated task which they describe as exploratory. A number of explanatory concepts are used to organize the various sets of variables, but there is no theoretical framework or hypotheses in the usual sense. The theoretical ideas presented are a combination of insights and the usual platitudes. But on the whole the study is nicely executed and is one of the better examples of this type of investigation.

The presentation does not fare so well. The material is poorly organized, and the writing is ponderous, repetitive, and tedious. This is due, in part, to the difficulty of presenting such complicated materials and relationships, and it is not surprising to find that the authors conclude that "the results are too complex to permit a separate summary." However, more could have been done to make this a more readable work. Among other things, the title is misleading, and putting all the tables at the end of the book does not facilitate the reading process.

Ultimately, and most importantly, the question remains as to what results from the thousands of correlations and the many factor analyses. The purpose of an exploratory study is to raise new ideas and open new areas for research. The authors feel they accomplished this goal. Their suggestions for further research—of which the volume does not abound—leave this reviewer weary. Concluding in the usual fashion they say: "A number of experiments could be undertaken now that would give a more sharply defined picture of some of the relationships in our work. For example, would the same social class differences have occurred if non-Jewish subjects were used?" It probably would have been wiser to sample more carefully in the first place.

DAVID MECHANIC

University of Wisconsin

Contemporary Research in Personality. Edited by I. G. SARASON. Princeton, N.J.: D. Van Nostrand Co., 1962. Pp. xii+411. $7.00.

In constructing this book on personality, editor Irwin G. Sarason has arranged forty-four recent research papers into nine sections. The assessment of personality in standardized testing situations is the topic of the initial sections. The first of these concerns paper-and-pencil, or direct self-report, techniques; the second is about more indirect procedures that involve the person's fantasy; and the third provides general discussion of relevant methodological and conceptual problems. The fourth and fifth sections consider the developing individual and the sociocultural influences on his functioning. The behavioral, especially perceptual, influences of the self and other aspects of personality are the subjects of the next section. The seventh section considers the learning process and the effects upon performance of frustration and stress. Deviant behavior, or psychopathology, is covered in the final two sections. The first of these includes analyses of abnormal behavior and considerations relevant to treatment effectiveness, while the second presents a number of case studies of severely disturbed individuals.

Saranson has selected this interesting group of papers from the psychological journals. This means that the book bears the unmistakable stamp of commitment to systematic research, and the less pervasive, though clear, concern with understanding the normal person. But it

also means that the book is characterized by diversity. The nine major topics do not bear any readily identifiable systematic relationship to one another. Within each topic, the papers differ markedly in method, purpose, theoretical orientation, and conclusions. Included are experiments, multivariate studies, single case descriptions, critical reviews, and conceptualizations, any one of which may have been written from one of a number of behavioristic, dynamic, psychometric, social, or eclectic points of view. There is little agreement as to data units and measurement procedures. Moreover, the effect of Sarason's attempt to sample widely in a cross-sectional manner has been to augment the heterogeneity that indeed characterizes the field by underplaying the integrating influence of continuing issues and methodological traditions.

Such a diverse collection of papers cries out for an interpretive framework to add clarity and perspective. Sarason's short introductory remarks do not go very far toward filling this need. The closest he comes is to suggest that the field of personality is "primarily concerned with the isolation of variables relevant to the understanding, prediction and manipulation of individual differences and human variability." He may have been overly inhibited by the understandable wish to avoid the kind of loose, global theorizing that bears a tenuous and equivocal relationship to specific situations and methods.

This book will achieve its greatest usefulness as an example of the kinds of research being done on personality. As such, it will provide little advantage over reading the psychological journals for the person already familiar with the field. It is more suited to the neophyte. And yet, it may well be somewhat unenlightening for him, by virtue of its uninterpreted diversity and lack of depth, unless he receives ancillary tutoring in the rudiments of a complex area.

It is to Sarason's credit that there are several exciting pockets of wisdom in the collection. Particularly noteworthy are Paul E. Meehl on the possibility that so-called structured tests permit subjective interpretation, Raymond B. Cattell on the uses and importance of multivariate procedures, Julian B. Rotter on the advantages for predicting motivated behavior of considering the situational context, E. Lowell Kelly on the consistency of personality, Theodore M. Newcomb on factors in interpersonal attraction, and Jerome D. Frank on the complexity of assessing the effectiveness of psychotherapy. It is papers such as these that influence the form of a field. Appropriately enough, one-half of the authors mentioned are past presidents of the American Psychological Association.

SALVATORE R. MADDI

University of Chicago

Society and Self: A Reader in Social Psychology. Edited by BARTLETT H. STOODLEY. New York: Free Press of Glencoe, 1962. Pp. 713. $7.50.

Since social psychology is taught in departments of both psychology and sociology, and since a book of readings (edited by Newcomb, Hartley, *et al.*) has long been available to the psychological teachers, it was likely that a comparable book would be prepared for the teachers in sociology. This book compares well with Newcomb and Hartley in any edition. Most of the fifty selections are reports of empirical researches by sociologists, adding up to an impressive display of empirical knowledge.

Following a pattern used by many sociologist-teachers of social psychology, the editor starts with some selections to illustrate the concept of social structure. The first chapter, by Levine and Sussmann on "Social Class and Sociability in Fraternity Pledging," fails to achieve the difficult task set for it, and hence starts the book off poorly. However, although of minor significance, it is an intrinsically adequate piece of research; this is true of many of the subsequent chapters. Thus the book proceeds to present, one after another, examples of excellent empirical research, but without illustrating or clarifying the general concepts or principles they are supposed to illustrate or clarify for the student.

These concepts and principles, offered by the editor as titles for the parts and sections, include, after the concept of social structure: aspects of psychosocial process (role image, self-image, and the "interchange" approach); the uses of social structure (six of them); problems of social non-definition or anomie (confusion, uniformity, violence); self-responses to social structure ("within social tolerance"); innovation, ("outside social tolerance"); social aspects of self-structure (adjustment, attitudes, aspirations, authoritarianism); self-feedback process (social movements, change in social structure); postulates and models (operational models, assumptions about the nature of man). I find it impossible to find a pattern or even logical sequence in this table of contents.

The basic fault of the book is that it presents no theory. In fact there are scarcely even any speculative chapters until the last section. The book can be used as a teaching device only if the instructor himself offers a coherent theoretical framework and then assigns chapters in a sequence of his own choosing. The editor opens each of the eight sections with a page or two of introduction; these do a good job of telling what is coming, but they do nothing to clarify what the ensuing researches mean in terms of any theory of social psychology or even how they relate to the outline of his own book. The list of selected readings at the close of each section is more useful in its function of guiding the student to additional social psychological literature.

In sum, the editor has done a superb job of selecting readings, but he has done a poor job of constructing a book.

ARNOLD M. ROSE
University of Minnesota

Identity, Mental Health and Value Systems. Edited by KENNETH SODDY. Chicago: Quadrangle Books, 1962. Pp. xii+271. $6.75.

This book is the culmination of a number of meetings sponsored by the Scientific Committee of the World Federation for Mental Health. It summarizes the attempt by various mental health specialists from several countries to clarify important mental health concepts and to indicate fertile research areas.

Like other summaries of group discussions, this small volume has many shortcomings. No attempt was made to seek out literature relevant to the discussions, and such basic sources as Jahoda, Goffman, Szasz, and Sullivan, among others, are ignored. Also, no serious attempt is made to validate the adequacy of the generalizations made, and the report of the discussion flows on oblivious to relevant empirical work. It would be unfair, however, not to recognize that the editor is modest in his claims and cognizant of the deficiencies noted above. Although he does "not presume to judge the accuracy or objectivity" of statements reproduced, he does indent them to distinguish them from those for which there is presumably greater agreement.

The first part of the book is directed at the concept of identity and various related terms: ego, self, empathy, identification, etc. In large part a social psychological role model is utilized, but it is an oversimplified one. Although there is a rich fund of research results dealing with perception, self-esteem, empathy, and the like, there is never the slightest indication in this book that such research exists.

The second half of the book is an attempt to illuminate a particularly significant area for those interested in cross-cultural studies in mental health: the relationships between concepts of mental health and religious ideologies. The most interesting chapter—the one on mental health assumptions and implications—is primarily concerned with the difficulties in developing universal criteria of positive mental health. Although the problem is attacked in a conceptual fashion that does not allow for a clear or empirically useful resolution, the difficulties in arriving at such a resolution are nicely illustrated. A second chapter that deals with relevant material from various religions and ideologies suffers from the usual defects associated with culture and personality writings and descriptions of national character. There is a great deal of glib speculation about a variety of ideological systems and their relevance to mental health. Among those discussed are Buddhism, Christianity, Confucianism, Marxism, Hinduism, Islamism, Judaism, Naziism, and Taoism.

In short, the book is a morass of statement and speculation without substantiation or documentation. However, it raises a number of important and interesting questions. The chapter on mental health assumptions and implications (pp. 70–117) especially is stimulating reading, and although the book fails to provide an adequate framework for viewing mental health, the discussion may be very useful to those who are working toward a more adequate cross-cultural perspective.

DAVID MECHANIC
University of Wisconsin

Students under Stress: A Study in the Social Psychology of Adaptation. By DAVID MECHANIC. New York: Free Press of Glencoe, 1962. Pp. viii+231. $5.00.

This book deals with reactions of twenty-two graduate students to Ph.D. preliminary examinations. Most of the book is concerned with how these students handle anxieties associated with preparing for prelims. Other chapters deal with the actual taking of the examinations, and reactions to them afterward. The two conclud-

ing chapters summarize research on stress and the relation of this study to such research.

Mechanic's major concern is to document the effects of sociological factors—particularly interaction and communication—on anxiety. These factors, he asserts, have been neglected as compared with physiological or psychological ones.

The students' problem was how to handle their anxiety. The author's problem was to describe the ways the students did this. Mechanic intensively interviewed students, spouses, and faculty during a period from three months before the examinations to a month after. Four weeks before and a month after the prelims, questionnaires based on the unstructured interviews were administered to the students. Comparable questionnaires were completed by the faculty. Mechanic uses these data to document student reactions and to contrast the relatively non-objective outlook of the students with the more objective viewpoint of the faculty.

The author discusses modes of adaptation to anxiety under two headings: coping, behavior that has consequences relevant to situational demands; and defense, handling the feelings aroused by the situation or by coping with it. Mechanic demonstrates that both modes of adaptation are related to communication and interaction. Both anxiety-arousing and comforting beliefs were communicated. Anxiety itself was communicated. Social comparisons often resulted in depression when students perceived that they were studying less effectively and learning less than their friends. Contact with an extremely anxious friend tended to heighten the individual's anxiety.

One is at first inclined to doubt the relevance of studying a group as atypical as this one. Two factors help justify a study of graduate students under stress. First, studies of stress under "natural" conditions are not common; this work contributes one useful case study. Second, Mechanic compares his results with reports of research on other groups, notably Grinker's data on air crews. Where Mechanic's results are similar to others, some confidence may be placed in their generality.

Mechanic's description of the stress situation would have profited by an analysis of the general characteristics of the situation. For example, the students share an important characteristic with Grinker's air crews: the stress situation applies equally to all members of the group. Of course, this situation can be contrasted with ones in which an individual faces stress and in which he may or may not receive social support. A discussion of the implications of this distinction would have been welcome.

Within the acknowledged limits posed by dealing with a small, atypical group, Mechanic does a creditable job. His handling of the data is cautious and conservative. One cannot cavil with the descriptive nature of the study; it is avowedly descriptive. And where would the Keplers be without the Brahes?

JOE L. SPAETH

Survey Research Center
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Berkeley

The Role of Schools in Mental Health. By WESLEY ALLINSMITH and GEORGE W. GOETHALS. New York: Basic Books, 1962. Pp. xiv+337. $7.50.

In a sense, this work consists of two somewhat separate books housed in one binding. As such, two entirely different sets of comments are applicable. This book is seventh in a series of ten monographs that will comprise the basis of a report containing findings and recommendations for a national health program. Its direct goal is to answer the question: What, if anything, ought schools to do about mental health?

To this end, Part I, a commentary on issues and practices, discusses the effects of the school experience on mental and emotional illness in general, curriculum organization, philosophies of teacher training, differential approaches to subject matter, programs of prevention and cure, and finally, an evaluation of nursery schools.

Two prime strengths in this first section seem to be thoroughness of treatment and liberal use of a multiple-disciplinary approach. Its weaknesses appear to be a lack of an underlying unification of disparate items and a shortage of explicit definitions of such terms as "life adjustment," "positive mental health," "healthy personality," and "effectiveness in living."

The second section of the book (roughly half) is an attempt to explore some of the issues found in the literature and in the first section of the book through the use of a field study to uncover "the potential sources of conflict and, by implication, areas of possible stress and threat to the mental health of teachers and students."

The authors orient the field research to an

examination of relatively normal, healthy situations with a view to taking preventive action, rather than to an observation of the abnormal or unhealthy for the purpose of treatment programs or generalizations from the abnormal to the normal.

In this section, the authors provide a good deal of basic data on teachers, their backgrounds, perceptions, and values. This should be of interest to many primary- and secondary-school teachers. However, the two samples, one of students and the other of teachers, appear to suffer from a lack of representativeness.

In general, there appears to be little continuity between the two sections of the book. The data, although extensive and illuminating, do little to provide evidence on the points raised in the first part of the book or the professed object of the work as a whole. Although the authors attempt to join their exposition and empirical data in the final chapter, the book seems to suffer from this apparent disjointing of focus and lack of unification.

Following the second section is a series of appendixes including what should prove to be an extremely complete bibliography for a researcher or scholar interested in the possible relationships between educational, institutional, and personality development.

It is entirely possible that placing this book in the context of the nine other works in the monographic series will minimize its shortcomings and amplify the many strengths.

GEORGE K. HESSLINK
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American Youth Culture: Group Life in Teenage Society. By ERNEST A. SMITH. New York: Free Press of Glencoe, 1962. Pp. vii+264. $5.00.

Ernest Smith has undertaken the task of collecting and critically summarizing a wealth of research reports, interpretations, and conjectures in an effort to document the existence of "a series of informal institutions that govern the behavior and standards of youth in the period between childhood and adulthood" (p. 220). It is the author's thesis that "this series of institutions meets the sociological criteria for a youth culture" (p. 220).

Focusing special attention upon the adolescent's peer relations as he undertakes the social journey leading to the establishment of his own marital family unit, the author surveys a diverse body of relevant material, some of it two or more decades old. A sizable number of citations have been culled from non-academic sources (e.g., *Ladies Home Journal*, *Time*, *New York Times*, etc.), a fact that at once reflects the author's realistic approach to the sprawling contours of the literature in this field and makes especially timely an attempted synthesis and set of guidelines for future research.

The framework of analysis is structural-functional (and a special appendix offers a definition of this approach, presumably for those unfamiliar with the term). Consistently emphasized are those patterns of adolescent behavior that demonstrate high internal organization and a notable lack of integration with adult culture. Given this focus of interest, it is not surprising that the final chapters exploring the shifting patterns of heterosexual adolescent relationships represent the most informative and well-organized sections of this work. The author's survey of the unwieldy literature on dating, his typology of dates, and his analysis of the transition to courtship and marriage merit special attention.

Although the analysis is initially restricted to the behavior of youth "who are American, white, urban, middle class, and post-pubertal" (p. 4), Smith was tempted (for sake of contrast) to include a self-contained chapter devoted to the behavior of lower-class gangs. He surrendered to this temptation with unfortunate results. In this section the citations and much of the discussion imply that little of value has been published since Thrasher and Shaw. The recent work of Walter Miller is especially relevant but unmentioned, and the attention given to Albert Cohen is painfully brief.

Yet any specific comments or criticisms that can be made necessarily take second place to a consideration of the basic assumption from which the analysis proceeds. Smith contends that the frequently noted gulf between the focal concerns of youth and those of adults mirrors a fundamental "underlying conflict of the two cultures" (p. 19). While any honest appraisal of the relationship between adolescents and their elders must inevitably come to grips with those conflict situations that do occur, Smith's repeated and insistent use of the term (pp. 18–22 and *passim*) implies that all other types of accommodation are virtually nonexistent. In fact, one is led to believe that it is this presumed state of affairs which *alone* reveals youth culture

as a "new fact in and of itself" (p. 1, quoting Durkheim) and thus provides justification for the present analysis.

Regrettably, the author's consideration of the one very direct attempt to force a reappraisal of this common assumption is brief (two sentences) and unsatisfactory (p. 6).

It should be obvious that direct conflict or rigid withdrawal does not exhaust the range of processes by which a subculture may maintain its boundaries. There are a variety of possible accommodations that may be made; self-maintenance and a measure of independent existence are properties of many cultural systems that retain vital links to their larger milieu. What is necessary (but completely lacking in Smith's work) is an appreciation of the fact that adult-adolescent relations are diverse, yet patterned, and worthy of generous and careful attention in any study of adolescent behavior.

RAMON JOHN RIVERA

National Opinion Research Center

Aging and Personality: A Study of 87 Older Men. By SUZANNE REICHARD, FLORINE LIVSON, and PAUL G. PETERSON. New York: John Wiley & Sons, 1962. Pp. xv+237. $7.95.

This is a detailed study of the personality patterns of eighty-seven white males, aged 55–84, who were interviewed on the average of nine hours and who also took a battery of eleven psychological tests. The study population was almost equally divided between retirees and those gainfully employed; the participants were a highly selective group as they were all volunteers. The authors state that the participants were in better health and in better spirits than persons who refused to take part in the research. Two raters, who also were among the six interviewers in the study, coded and classified the research data.

The major focus of the study was the exploration of age changes and successful and unsuccessful adjustment to later maturity. In the first part of the book statistical tables were used to present the data along with tests of significance and correlation coefficients. The research precision implied by the use of the various statistical techniques should not mislead the reader because the basic study data have a number of shortcomings. In many instances the investigators employ only the cases that fall in the extreme categories. Furthermore, the basic coding of the data relied heavily upon the subjective ratings of the two judges. The authors state, "personality characteristics were rated inferentially by evaluation of the total interview." Reliability checks were made of the data, but in the case of the 115 personality characteristics that are a major part of the study of personal adjustment, ten cases were used to make a reliability test, and the median value of the correlation coefficient of the two raters was only .52. The authors were concerned principally with the subjective aspects of retirement and adjustment to aging. This is clearly indicated by the fact that they did not obtain, for example, systematic information on the financial resources of about one-half of the study group. The investigators did not include questions on finances in the interview guide and the conclusions concerning income are based on information apparently volunteered by 51 per cent of the cases. Yet the authors state that economic insecurity, "one of the major hazards of old age," was greater "among the maladjusted."

The title of the book, *Aging and Personality: A Study of 87 Older Men*, implies that the findings reported are based on eighty-seven cases. However, this is not the case, because in the last part of the book seventeen cases that had a middle adjustment rating to aging were excluded and an additional twenty-three cases were dropped because they did not fit any of the personality types.

The volume was jointly authored, but there seems to have been little attempt made to coordinate the various sections of the book. We note, for example, that in the first half of the book, thirty cases were rated poor in adjustment to aging and twenty-one as good in adjustment; in the last half of the book, twenty cases were rated as poorly adjusted and twenty-seven were rated as good. Yet no attempt is made to explain the discrepancy nor to explain the relationship of the different criteria that were employed.

Despite the rich data that were obtained in this study, sociologists will find it of limited interest, for the authors rely primarily on psychoanalytic concepts in analyzing their data. In view of this approach, one of the surprising gaps in such a detailed study of personality types is that there is no discussion of sexual attitudes or behavior. Sex does not appear in the index and there is no explicit mention of sex in

the detailed interview guide, presumably printed in its entirety.

The main body of the book and the eleven appendixes are filled with many facts about the older person interviewed in the study. Detailed tables are presented showing, for example, the way in which education or occupation is related to health, or social activities, or political outlook, but no attempt is made to interpret the significance of the findings. Findings are reported in abundance, but the essential job of fitting the pieces together is left to the reader. This is perhaps too much to ask of even the most interested and conscientious of readers.

GORDON F. STREIB

Cornell University

Aging in a Changing Society. Edited by RUTH E. ALBRECHT. Gainesville: University of Florida Press, 1962. Pp. xii+187. $3.75.

The number of conferences that have taken place and of publications that have appeared on the topic of aging within the past five years is nothing short of amazing. This is all the more true in view of the almost complete absence of literature on the sociology of aging if one goes back more than fifteen years. Thus the book under consideration can be considered to have a venerable history: it is the report of the *Eleventh* Annual Southern Conference on Gerontology held at the University of Florida. T. Lynn Smith was the editor of the first two volumes, and Irving L. Webber was the editor of several of the intervening ones. With this leadership the conferences have generally had a high quality, and the standards have been maintained in this work directed by Professor Ruth Albrecht.

The format seems to be to have a leading national figure present a major paper and then have it discussed by a local worker in the field. The book is divided into four parts: The first deals with concepts and philosophy in the field of aging, and presents James E. Birren on "Behavior: Cause or Effect in Aging" and Clark Tibbitts on "Developments in Gerontology over the Past Ten Years." Part 2 deals with special problems of automation and forced free time, by Robert W. Kleemeier; older people left behind in rural areas (of Wisconsin) when younger ones move out, by Douglas G. Marshall; and the facts about the low-average income level of the elderly, by Dorothy McCamman. The third part takes advantage of the recent appearance of the 1960 Census, and offers a general analysis of population changes by Henry D. Sheldon, a measurement of changes in the number and distribution of the aged by T. Lynn Smith, and a description of the aging population of Florida by W. Kennedy Upham. Part 4 has the topic "Rehabilitation Factors for the Aging" and presents an overview of rehabilitation by E. B. Whitten, a description of facilities, the sheltered workshop, and the role of the physician as these pertain to rehabilitation of the handicapped aged, by three Florida leaders in the field.

Naturally, these papers show varying degrees of preparation and usefulness. Tibbitts' offers the best summary of the recent history of gerontology with which I am familiar. Marshall opens up discussion of the coming problem of population-debilitated counties, whose remaining residents are mainly the elderly. McCamman presents a definitive statement about the income levels of the aging, and lays to rest the American Medical Association's claims that most of the elderly have incomes adequate to take care of their own medical needs. Sheldon's overview of the changes shown by the 1960 population census has nothing to do with the elderly (Smith handles that), but it is a comprehensive and most useful reference.

The annual Florida conferences, like those at Michigan and elsewhere, show the high vitality of research and social action in the field of social gerontology.

ARNOLD M. ROSE

University of Minnesota

Society, Culture, and Drinking Patterns. Edited by DAVID J. PITTMAN and CHARLES R. SNYDER. New York: John Wiley & Sons, 1962. Pp. xvii+616. $9.75.

In the field of alcohol and alcoholism, speculation and myth have run far ahead of and contrary to empirical research. This book fills an important need in bringing together some of the recent social science research in this field.

Its five major sections cover both non-pathological drinking patterns and alcoholism. The editors have introduced the topics of the various subsections with their own comments about the significance of the papers to each other and to the field of alcohol research in general.

It is a splendidly well-organized collection. Yet it is because of this organization that the uneveness of development in alcohol research becomes apparent. Although they were largely able to do so, it was not always possible for the editors to complete their broad outline with existing studies of high quality. The gaps are most apparent in the sections on "normal" drinking patterns.

Dollard's comments on social class and styles of drinking, for example, although written more than two decades ago, remain the starting point for two of the papers in the subsection on "Class and Status." One of these, "Drinking Styles and Status Arrangements," is a secondary analysis in which drinking information was but a byproduct of data collected for other purposes.

Still, if the editors' outline is not always satisfactorily filled in, many of the papers serve well in indicating the kinds of research and analyses possible through the study of alcohol. Gusfield's discussion of temperance movements, for example, traces the early and liberal trends that preceded some of today's prohibitionist groups. On the contemporary scene, Rubington reveals Skid Row to be something other than totally anomic.

A number of review articles are exceedingly well done. Keller's paper on the definition of alcoholism and estimations of prevalence is a joy of reasoning. Jackson's review of "Alcoholism and the Family" is a fine example of how a wide range of studies, many not directly associated with alcoholism, can be used to add to our knowledge of the impact of alcoholism on the family.

This book is a valuable guide to the current state of alcohol research and a source of information on some significant social phenomena as well.

Raymond Fink

Health Insurance Plan of Greater New York

The Other America: Poverty in the United States. By Michael Harrington. New York: Macmillan Co., 1962. Pp. 191. $4.00.

The Other America is not intrinsically a very important book. At the same time, it is a very necessary book. One characteristic of unimportant books on important topics is that they become important when there are virtually no substitutes. Professional sociology, it might be added, has done little to fill this gap.

Harrington, an able journalist, has brought together a highly readable treatment of a number of instances of what Galbraith has called "insular poverty." Stylistically, the major fault is to be found in the marked difficulty Harrington has in curbing an excessively sentimental tone when describing his personal encounters with poverty.

The major points made by Harrington are that poverty characterizes a greater segment of our population than the prevailing images of our society lead one to suspect, and that a "culture of poverty" has emerged in these frequently near-invisible pockets of distress that cannot be treated symptom by symptom. He writes: "Being poor is not one aspect of a person's life in this country; it is his life. Taken as a whole, poverty is a culture. . . . These are people who lack education and skill, who have bad health, poor housing, low levels of aspiration and high levels of mental distress. . . . Each disability is the more intense because it exists in a web of disabilities." Yet it is just such a segmented treatment of poverty that marks most of the recent social-problems texts; a strategy not unrelated to the theoretical commitments and biases of the larger field.

One important contribution this book makes is that it abstracts Galbraith's treatment of the new and unique form that poverty takes in an affluent society from the larger argument where it is frequently lost. Here is both a problem and approach worthy of systematic consideration.

Harrington concludes his discussion of poverty with the observation that, unlike an earlier era when one could say poverty is no disgrace, but is annoying, today poverty is not annoying (being almost invisible) but is a disgrace. Nor can one say that it has been more visible in the spectrum of recent sociological interests. There is hope, however, that the continued flow of research funds into areas like delinquency and dependency will soon inspire a theoretical and substantive legitimacy for confronting this problem more directly.

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VALUES AND GANG DELINQUENCY: A STUDY OF STREET-CORNER GROUPS[1]

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ABSTRACT

Deduced from three theoretical positions on gang delinquency, hypotheses concerning the values of gang, non-gang lower-class, and non-gang middle-class boys were tested with a semantic differential. Contrary to expectation, the data indicated no differences between gang, lower-class, and middle-class boys, both Negro and white, in their evaluation and legitimation of behaviors representing middle-class prescriptive norms. These middle-class behaviors were also rated higher than deviant behaviors governed by middle-class proscriptive norms. The samples differed most in their attitude toward the deviant behaviors, tending to form a gradient, with gang boys most tolerant, middle-class boys least tolerant.

Three recent theories of juvenile-gang delinquency view values as an important link in a causal chain leading from social status to illegitimate behavior.[2] The theories are seemingly in agreement as to what they mean by "values," and they differ only slightly in the content of the values which they ascribe to members of three relatively distinct social categories: lower-class gang, lower-class non-gang, and middle-class non-gang. There are, however, important differences between the theories in the assumptions underlying these values. As a result, competing, if not always mutually exclusive, hypotheses are implied. This paper attempts to further refine thinking in this area by empirically testing some hypotheses that might reasonably be deduced from the three theories. Accordingly, relevant data gathered from both Negro and white adolescent members of each of the social categories are presented.

THE SAMPLE

Samples of Negro and white males were drawn from each of the following social categories, making a total of six populations under study.

[1] This paper reports research conducted under grants from the National Institute of Mental Health (Research Grant M-3301) and the Ford Foundation. The authors wish to express their gratitude to Kenneth I. Howard for assistance and counsel during all phases of the research, to Albert K. Cohen, Stanton Wheeler, Maria Gordon, and Patricia Leavey Hodge for helpful suggestions, to Alan E. Hendrickson for programming some of the computations for Univac I, and to Fred Hubbard and Charles N. Cooper of the YMCA for their splendid support.

[2] These theortical statements are by Albert K. Cohen, *Delinquent Boys* (Glencoe, Ill.: Free Press, 1955); Walter B. Miller, "Lower Class Culture as a Generating Milieu of Gang Delinquency," *Journal of Social Issues*, XIV, No. 3 (1958), 5–19; and Richard A. Cloward and Lloyd E. Ohlin, *Delinquency and Opportunity* (Glencoe, Ill.: Free Press, 1960). For further elaboration of the Cohen point of view see Albert K. Cohen and James F. Short, Jr., "Research in Delinquent Subcultures," *Journal of Social Issues*, XIV, No. 3 (1958), 20–37. Miller brings his viewpoint to bear on empirical data in Walter B. Miller, Hildred Geertz, and Henry S. G. Cutter, "Aggression in a Boys' Street-Corner Group," *Psychiatry*, XXIV, No. 4 (1961), 283–98.

Gang.—The gang boys studied are members of nine Negro and six white gangs assigned workers by the Program for Detached Workers of the YMCA of Metropolitan Chicago. The samples contain 163 Negroes and 58 whites, and constitute from a third to a half of the total membership of these gangs. Police record data were obtained for all nine Negro gangs and four of the six white gangs.[3] For the total memberships, the number of offenses known to the police per boy averaged 3.17 for Negroes and 2.91 for whites; for boys in the samples these figures are 3.29 and 3.39, respectively. Thus,

TABLE 1

MEAN AGES AND STANDARD DEVIATIONS FOR SIX SAMPLES

Group	Mean Age	Standard Deviation
Negro:		
Gang	17.2	1.9
Lower class	16.5	1.4
Middle class	17.3	0.9
White:		
Gang	18.2	2.1
Lower class	16.8	1.1
Middle class	16.1	1.2

boys from whom data were collected do not appear to be less delinquent than the average member of their gangs. A comparison of the ages of boys in the samples with those not included reveals that the included Negroes are 0.57 years, and the included whites 0.16 years younger than members of their gangs not included. A check of rosters of gang members prepared in advance of collecting these data gave no sign that detached workers were able to produce only their more tractable gang boys for research. If newspaper headlines are any criterion, these gangs include all but one of the most notorious in Chicago during 1960–61.

Lower class.—Boys residing in the same neighborhoods as the gang boys but not themselves members of gangs were contacted through Y's and settlement houses. Six Negro and two white groups or clubs constitute the samples, for a total of 69 Negroes and 37 whites. The search of police records revealed these boys to have had a moderate amount of official involvement in delinquency, indicating that these samples are not composed of boys who are unusually good. The mean number of offenses per boy known to the police was 0.33 for Negroes and 0.22 for whites.

Middle class.—Non-gang middle-class boys were reached through two YMCA's known to serve a middle-class clientele and located in areas of Chicago judged to be middle class according to conventional demographic criteria. A total of 24 Negro and 41 white boys—from two Negro and two white clubs—is included. Just one boy within each race was known to police for delinquent activity, for a combined total of three offenses, all minor; the corresponding means were 0.08 for Negroes and 0.03 for whites. No examples of a middle-class gang could be found locally.

The sample as a whole.—Mean ages and standard deviations for the six samples, in years, are shown in Table 1. The white gang sample includes the two oldest persons, one 24.4 and one 26 years old. Although most of the age differences between samples were statistically significant, an examination of the correlations between the main data and age indicated that none of the interpretations to be presented could be accounted for by differences in age.[4] Although all the gang

[3] The police record search was conducted by John M. Wise, who furnished the data upon which these figures are based (see his "A Comparison of Sources of Data as Indexes of Delinquent Behavior" [unpublished M.A. thesis, University of Chicago, 1962]).

[4] Some selected 1960 Census statistics for the Chicago community areas and tracts from which these samples were drawn are presented in a table (Table A), which is one of three tables (indicated by alphabetic references in this paper) that, along with certain methodological notes, have been deposited with the American Documentation Institute. For a discussion of the examination of relations with age see Note A. Order Document No. 7468 from ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D.C. remitting in advance $1.25 for 35-mm. microfilm or $1.25 for 6 × 8-inch photocopies. Make checks payable to Chief, Photoduplication Service, Library of Congress.

boys are definitely lower class, for convenience this report will distinguish the gang from non-gang lower-class samples by the use of the terms "gang" and "lower class," respectively.

THE INSTRUMENT AND PROCEDURES

The data were gathered by means of a semantic differential, which consists of a number of seven-point, bipolar, adjectival scales against which any set of concepts or descriptive images may be rated.[5] This instrument measures what Osgood terms "connotative meaning" which, for a variety of populations, has been found to have two main orthogonal dimensions when a large number of scales and concepts are administered and the scales then intercorrelated and factor-analyzed. To obtain adequate measures of these dimensions, only a small number of scales, found to have high correlations with the appropriate dimensions, are required.

A score for a dimension is obtained by averaging the appropriate scale values, which ranged from 1 to 7. These dimensions and the corresponding scales used in this study are:

Evaluation	*Potency*
clean-dirty	hard-soft
good-bad	large-small
kind-cruel	strong-weak
fair-unfair	brave-cowardly
pleasant-unpleasant	rugged-delicate

Three additional scales, derived from Miller's "focal concerns" of lower-class culture, were also included. These were "smart-sucker," "lucky-unlucky," and "exciting life-boring life."[6]

The images (see Table 2) to be rated were chosen to represent salient examples of instrumental or dominant goal activity, leisure-time activity, and ethical orientation

[5] See Charles E. Osgood, George J. Suci, and Percy H. Tannenbaum, *The Measurement of Meaning* (Urbana: University of Illinois Press, 1957).

[6] Miller, *op. cit.*, p. 6.

TABLE 2

SEMANTIC DIFFERENTIAL IMAGES

Subculture	Label	Images: "Someone who . . ."
Middle class:		
Dominant goal activity	GRAD	works for good grades at school
Leisure activity	READ	likes to read good books
Ethical orientation	SAVE	saves his money
Lower class:		
Dominant goal activity	SJOB	has a steady job washing and greasing cars
Leisure activity	HANG	likes to spend his spare time hanging on the corner with his friends
Ethical orientation	SHAR	shares his money with his friends
Conflict:		
Dominant goal activity	TUFF	is a good fighter with a tough reputation
Leisure activity	HANG	(See lower class)
Ethical orientation	STIK	sticks by his friends in a fight
Criminal:		
Dominant goal activity	FENC	knows where to sell what he steals
Leisure activity	HANG	(See lower class)
Ethical orientation	CONN	has good connections to avoid trouble with the law
Retreatist:		
Dominant goal activity	PIMP	makes easy money by pimping and other illegal hustles
Leisure activity	DRUG	gets his kicks by using drugs
Ethical orientation	COOL	stays cool and keeps to himself
Additional images:		
	GIRL	makes out with every girl he wants
	SELF	Myself as I usually am
	IEGO	Myself as I would like to be
	GANG	is a member of (enter group name or if none, "your friendship group")

for each of five theoretically significant subcultures—middle class, lower class, conflict, criminal, and retreatist.[7] Leisure activity appeared to be essentially the same for three of the subcultures, and is therefore represented for all three by a single image.

Although they do not figure prominently in this analysis, the three aspects of subcultural roles did provide a basis for sampling widely within each domain. Of four additional images included because of their theoretical interest, only the one identified by the label "GIRL" requires comment. This image was included to furnish responses relevant to sexual demonstrations of masculinity. Hopefully, images were phrased so as to be as concrete as possible and yet personify the values hypothesized to distinguish the subcultures.

Administration of the semantic differential to small numbers of subjects at a time took place in an old, rather shabby one-time apartment building, where the subjects were fed hot dogs and soft drinks. The tester was quite permissive toward all departures from normally decorous behavior that did not jeopardize the validity of measures. Considerable care was taken to explain directions and check the boys' responses. A few boys, unable to read, had the semantic differential read to them as they responded.

Seventeen factor analyses of the evaluation and potency scales, performed for seventeen of the gangs and clubs studied, revealed evaluation and potency factors for all six populations matching those previously found by Osgood.[8] This rules out all but the most ingenious and most coincidentally patterned types of deliberately meaningless, falsified responding. It also justifies the scoring procedure.

[7] The last three subcultures refer to types of delinquent gangs postulated by Cohen and Short, *op. cit.*, and by Cloward and Ohlin, *op. cit.*

[8] See Robert A. Gordon, "The Generality of Semantic Differential Factors and Scales in Six American Subcultures" (unpublished M.A. thesis, University of Chicago, 1962).

STATISTICAL TREATMENT

The data consist of the mean scores for both evaluation (Table 3 below) and the "smart-sucker" scale (Table 5 below) accorded to each of the seventeen images by each of the six populations. Three-way (image by race by social category) analyses of variance (Tables B and C) have indicated high levels of over-all significance for these data.[9] The sources of this significance are investigated further by comparing all six of the individual sample means for an image with each other, using two-tailed *t*-tests. Although this procedure carries a high risk of a Type I error[10]—because it inevitably compares the most extreme values in any set of six—it was felt that, because differences are theoretically more interesting here than similarities, this method is preferable to alternative tests having high risk of a Type II error. Important additional constraints upon interpretation are exerted, however, by (1) the fact that the three social categories are ordered with respect to presumed similarity (gang, lower class, middle class); (2) the presence of data for two races. Thus, any ordering of the data which is similar to that of the three categories, and which appears in both races, will strongly supplement the presence of statistical significance. This organization of the data has the advantage of possibly suggesting attitudinal trends in American society that may prove to be more useful in understanding delinquency than single comparisons holding constant race or class.

INFERENCES FROM THEORIES

Cohen.—As an explanation of juvenile-gang delinquency, the hypothesis of a reaction formation against the standards of

[9] American Documentation Institute, *op. cit.* For a justification of the use of parametric statistics with semantic differential data, see either Note B, *ibid.*, or Robert A. Gordon, "Values and Gang Delinquency" (unpublished Ph.D. dissertation, University of Chicago, 1963).

[10] See Thomas A. Ryan, "Multiple Comparisons in Psychological Research," *Psychological Bulletin*, LVI, No. 1 (1959), 26–47.

middle-class society has been proposed by Albert K. Cohen. According to Cohen, reaction formation serves as a defense against the anxiety of status frustration, common to lower-class youth and especially severe for those who join gangs. Although Cohen's theory holds that the wholesale repudiation of middle-class values "does its job of problem-solving most effectively when it is adopted as a group solution,"[11] and that "group interaction is a sort of catalyst which releases potentialities not otherwise visible,"[12] thus seeming at times to leave unsettled the question of whether private values are similarly affected, the logic of the mechanism of reaction formation requires that middle-class values be submerged in the consciousness of individuals as well as in the culture of the group. His point seems to be that the group experience, in which individuals come together with the common problem of status frustration, is necessary for the full unfolding and elaboration of a latent common solution, namely, total repudiation of middle-class standards. Once exposed to the mutual self-recognitions and reinforcements of collective acting out, negative attitudes that were only latent in the individual's value processes become manifest. It is reasonable to expect that the resulting modification in values, while undoubtedly subject to intensification during group interaction, remains as a relatively enduring feature of an individual's personality, even when he is apart from the group. This interpretation is consistent with Cohen's emphasis upon the over-reactive quality of much delinquent behavior. Thus, although Cohen's theory asserts that middle-class values are in fact internalized by gang boys, he clearly implies that they persist only as a repressed and unacknowledged source of anxiety.[13]

An instrument as baldly direct as the semantic differential would not be expected to bypass such a firmly established system of neurotic defenses. Accordingly, the explicit and highly developed negativism described by Cohen should characterize the conscious private values of the gang boy and be reflected in his evaluation of middle-class images. As it was constructed, the instrument afforded subjects an opportunity to express bitterness and contempt toward rather tempting middle-class figures (see GRAD, READ, and SAVE in Table 2); they had only to avail themselves of the negativistic ends of the evaluative scales. Hence, if the hypothesis of reaction formation is correct, these evaluation scores for gang boys should be low.

In contrast, gang boys should evaluate images that are antithetical to middle-class morality higher than the middle-class images. This follows from Cohen's statement: "The hall-mark of the delinquent subculture is the explicit and wholesale repudiation of middle-class standards and the adoption of their very antithesis."[14] Strictly speaking, only TUFF meets Cohen's specification that the negativism of the reaction formation is also non-utilitarian. Yet, it would seem that FENC, CONN, PIMP, and GIRL are sufficiently violative of middle-class expectations to serve also as vehicles for the expression of negativism so presumably global (DRUG is perhaps too special a case to merit consideration). Whereas Cohen asserts that utility does not constitute the chief motivation of delinquent-gang boys, there is nevertheless nothing in his theory to suggest that such negativism would be inhibited if it happened to lead to a utilitarian end. For these reasons, all of these images should be evaluated higher than the middle-class images, but special attention should be paid to TUFF. The gang boys should also evaluate these images higher than do middle-class boys.

Although not directly connected with the reaction-formation hypothesis, at least two of the lower-class images, SHAR and HANG, should, according to Cohen, be acceptable to the gang boys; the first, because it represents the lower-class ethic of reciprocity, and

[11] Cohen, *op. cit.*, pp. 134–35.

[12] *Ibid.*, p. 136.

[13] *Ibid.*, p. 132.

[14] *Ibid.*, p. 129.

the second, because it is an activity favored by both stable lower-class and delinquent boys. Whether gang boys would perceive the third lower-class image, SJOB, as but another form of subservience to middle-class standards rather than as an admissible lower-class occupation is not indicated in Cohen's theory.

Miller.—The proposition that the lower class possesses a relatively distinct and autonomous value system is suggested, although not stated explicitly, by Walter B. Miller.[15] He does, however, clearly assert that the delinquent acts of lower-class gang members have as their "dominant component of motivation" the "directed attempt by the actor to adhere to forms of behavior, and to achieve standards of value as they are defined by that community,"[16] the reference being to the lower-class community. He characterizes these standards as "focal concerns," and it is clear that, although they may be present to some degree in other strata, they receive radically different emphasis in the lower class than they might in the middle class. While it follows from this that lower-class and gang values emphasize elements not emphasized in middle-class values, Miller leaves unclear the weighting that lower-class and gang values would accord to elements that do receive great emphasis in the middle class (unless one is willing to conclude that Miller intends his description of lower-class values to be practically exhaustive, in which case elements emphasized in the middle class would be absent entirely from lower-class culture). Despite this ambiguity, it seems reasonable to infer the following expectations from Miller's statement: lower-class and gang boys should (1) not evaluate the middle-class images as high as do middle-class boys, (2) evaluate lower-class images higher than middle-class images, (3) evaluate the lower-class images higher than do the middle-class boys, (4) evaluate images that accord with lower-class focal concerns, such as the retreatist, conflict, and criminal images, higher than do middle-class boys.

Miller and others[17] also postulate the existence of a sex-identity problem for lower-class males growing out of early socialization experiences in households in which adult male figures are not consistently present. According to this "female-based household" hypothesis, attempts by lower-class males to achieve masculine identity are characterized by an exaggerated emphasis on sexual and aggressive exploits. Three images offer possibilities for testing this hypothesis: TUFF, GIRL, and PIMP, the last because it emphasizes a relationship with women in which the woman is controlled, exploited, and degraded. It was hypothesized that the order of evaluation of these images would run Gang > Lower Class > Middle Class, and Negro > White within each of the three social levels. These orderings simply reflect the extent to which female-based households were assumed to occur in the family histories of members of each social category.

Because the focal concerns have themselves a dimensional character—consider, for example, *toughness*, *smartness*, and *excitement*—along which behaviors may be implicitly ordered, it might be questioned whether the evaluative responses of lower-class respondents should reflect the same ordering. Miller, however, makes it quite clear that he expects evaluation and desirability to be linear functions of the focal concerns, rather than orthogonal to them, giving as one reason for preferring to speak of "focal concerns" rather than "values" his feeling that the former is neutral with respect to the implied direction of positive evaluation.[18] This indicates that the two run generally parallel to each other in his thinking.

Cloward and Ohlin.—Two fundamental orientations of lower-class youth have been distinguished by Richard A. Cloward and Lloyd E. Ohlin.[19] One is based upon attitude

[15] *Op. cit.*

[16] *Ibid.*, p. 5.

[17] E.g., Roger V. Burton and John W. M. Whiting, "The Absent Father and Cross-sex Identity," *Merrill-Palmer Quarterly*, VII, No. 2 (1961), 85–95.

[18] *Op. cit.*, p. 7.

[19] *Op. cit.*, pp. 90–97.

toward membership in the middle class, the other upon attitude toward improvement in one's economic position; a person may desire either, both, or none of these two objectives. The possible combinations of indifference or aspiration toward these objectives yield a typology—inspired by Merton's typology[20] of individual adaptation—of four kinds of lower-class youth. Cloward and Ohlin hold that it is from Type III of their typology, those indifferent toward membership in the middle class but eager for improvement in their economic position, that the "principal constituents of delinquent subcultures" are drawn.[21] When legitimate avenues of opportunity are blocked for such boys, delinquent subcultures of different types emerge according to the pattern of illegitimate opportunities locally available.

It will be noted, however, that the middle-class images used in the semantic differential appear to stand for striving, self-improvement, and sacrifice far more than for the "big cars," "flashy clothes," and "swell dames," that Cloward and Ohlin suggest epitomize the goals of Type III youth.[22] Therefore, insofar as the middle-class images represent the style of life characteristic of actual membership rather than simply middle-class economic position, it may be inferred that delinquents would be relatively cool toward them. Accordingly, they should evaluate GRAD, READ, and SAVE lower than does the middle-class sample. But if they do evaluate the middle-class images high it can be argued on a fortiori grounds that they would also evaluate images standing for middle-class consumption patterns high. Indeed, despite the typology, it would be surprising if anyone did not. Thus, if gang boys evaluate the images GRAD, READ, and SAVE high it would constitute a conservative test in favor of Cloward and Ohlin's hypothesis concerning their attitudes toward economic position. But simultaneously this would bring into question either the separate existence of the two orientations on which the typology is founded or the supposition that gang delinquents emerge mainly from Type III. (It may be that if presented with them, gang members would evaluate images representing middle-class consumption patterns extremely high, higher even than GRAD, READ, and SAVE, and higher also than would middle-class boys. If so, there would then be reason to continue to regard the two orientations as relatively independent and distinct.) In either case it would then seem that the emphasis which Cloward and Ohlin give to exclusively economic motivation may require qualification.

Hypotheses concerning the deviant subcultural images are complicated by the fact that, according to Cloward and Ohlin, members of gangs would be expected to indorse highly the images standing for the subcultural adaptation into which their own gang best fits. There is thus no reason to believe that a gang boy would evaluate all deviant subcultural adaptations high. Since this paper makes no attempt to distinguish gangs according to this subcultural typology, any hypothesis dealing with the evaluation of illegitimate images by gang boys must be regarded as tentative.[23] In general, it might be hypothesized that gang boys would evaluate illegitimate images higher than non-gang boys.

TYPES OF COMPARISON AND SUMMARY OF HYPOTHESES

Types of comparisons.—Implicit in the inferences from theory are two types of comparisons concerning the image means in Tables 3 and 5, below. One compares the six populations for a single image to detect *differentials between populations for the same image;* this comparison focuses on one *row* of a table. The other type of comparison examines different images for the same population to detect *differences in relative level of the images;* it focuses on one *column.* All of the comparisons for rows have been made,

[20] Robert K. Merton, "Social Structure and Anomie," *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1957).

[21] *Op. cit.*, p. 96.

[22] *Ibid.*

[23] Efforts to delineate value and behavior patterns of the gangs are under way, and will be reported separately.

and the results are presented in Tables 4 and 5, below. Each image affords fifteen possible comparisons between the six populations, or a total of 255 comparisons for all seventeen images in each table. Of the 255 for evaluation, for example, 103 (over 40 per cent) were statistically significant, many at a very high level. The greater attention will be paid to these row comparisons. However, some reference will be made to column comparisons, which, when especially relevant, have been calculated.[24] As an aid to interpretation, the range of potential significance for column comparisons has been given in Tables 3 and 5 (below), with figures below each column showing the magnitude of the smallest difference possibly significant at the .05 level, as well as the smallest difference definitely significant at the .05 and .01 levels. All differences less than the former are not significant; all equal to or greater than the latter are. These boundaries tend to be extreme. On the average, a difference intermediate between these limits would probably mark the threshold of significance.

Summary of hypotheses.—On the basis of quite different assumptions, each of the theories leads to the expectation that gang boys will evaluate deviant or illegitimate images higher than do middle-class boys (Cohen's theory because of reaction formation, Miller's because these images correspond to the focal concerns of lower-class culture, and Cloward and Ohlin's because the images represent adaptations to the relative unavailability of legitimate opportunities for members of the lower class), but only Cohen's theory carries the stronger implication that gang boys will value deviant images even higher than the middle-class images. All three theories imply that middle-class values, as represented in the middle-class images, are not indorsed as highly by gang boys as by middle-class boys. A careful reading of these theories has led to the following explicit hypotheses:

[24] Such tests, which apply to several observations on the same sample, take into account the correlation between observations.

1. Gang boys evaluate the middle-class images lower than illegitimate images such as PIMP, FENC, CONN, GIRL, and TUFF (Cohen). A column comparison.
2. Gang boys evaluate the middle-class images lower than do lower-class and middle-class boys (Cohen). A row comparison.
3. Gang and lower-class boys evaluate the middle-class images lower than do middle-class boys (Miller). A row comparison.
4. Gang boys evaluate the middle-class images lower than do middle-class boys (Cloward and Ohlin). A row comparison.
5. Gang boys evaluate SHAR and HANG higher than the middle-class images (Cohen). A column comparison.
6. Gang and lower-class boys evaluate lower-class images higher than middle-class images (Miller). A column comparison.
7. Gang and lower-class boys evaluate the lower-class images higher than do the middle-class boys (Miller). A row comparison.
8. PIMP, GIRL, and TUFF are evaluated higher (*a*) by Negroes than whites, (*b*) by gang boys than lower-class and middle-class boys, and (*c*) by lower-class boys than middle-class boys (Miller and others). All row comparisons.
9. Gang boys evaluate illegitimate images higher than do non-gang boys (Cohen; Cloward and Ohlin). A row comparison.
10. Gang and lower-class boys evaluate illegitimate images higher than do middle-class boys (Miller). A row comparison.

DATA AND INTERPRETATION

The images are discussed in the order in which they figure in the hypotheses. This leads first to a discussion of the middle-class images (where the distinction between the moral validity and the legitimacy of norms is invoked in an effort to account for the findings). The remaining images are discussed in clusters bearing upon particular hypotheses and interpretations suggested by regularities in the data.

Middle-class images.—Of forty-five differences between the six populations in evaluation of the middle-class images, only two were significant (see Tables 3 and 4). Both Negro lower-class and white lower-class boys evaluated GRAD higher than did white gang

boys, in both instances at the .05 level. This is almost precisely the number of significant findings out of forty-five totally independent tests (which these are not) to be expected at this level on the basis of chance alone. In view of the high risk of a Type I error in this statistical treatment, it is fair to describe the picture presented by these data as one of overwhelming homogeneity. *All six populations evaluated images representing salient features of a middle-class style of life equally highly.*

Furthermore, no image representing the other four subcultures was evaluated significantly higher than the middle-class images by any one of the six populations. Of the sixty means for non-middle-class subcultural images, five were slightly higher than some of the means for middle-class images. In every such instance the image involved was SHAR, standing for lower-class reciprocity, an image that could not be characterized as illegitimate.

In fact, the middle-class images were evaluated significantly higher by every one of the populations than nearly all other subcultural images, especially those that are unquestionably illegitimate.[25] None of the theories would have led one to expect these findings.

An explanation for the disparity between the theories and these particular data might be found in the distinction between moral validity and legitimacy. Cloward and Ohlin,

[25] As checked by means of the definitely significant difference (see Table 3).

TABLE 3

EVALUATION MEANS

IMAGES	NEGRO			WHITE		
	Gang	Lower Class	Middle Class	Gang	Lower Class	Middle Class
Middle class:						
GRAD	5.58	5.72	5.68	5.35	5.79	5.61
READ	5.33	5.48	5.30	5.30	5.34	5.54
SAVE	5.30	5.33	5.18	5.12	5.17	5.15
Lower class:						
SJOB	4.25	4.26	3.93	4.26	3.71	3.60
HANG	4.05	4.29	3.52	4.23	4.02	2.98
SHAR	5.38	5.51	5.52	5.03	5.28	5.39
Conflict:						
TUFF	3.38	3.33	2.42	3.59	3.52	2.56
STIK	4.65	4.75	4.58	4.61	4.92	4.41
Criminal:						
FENC	3.03	2.53	2.38	2.88	2.18	2.31
CONN	4.22	3.62	3.28	3.98	2.99	2.40
Retreatist:						
PIMP	3.49	3.04	2.67	2.59	2.02	1.76
DRUG	2.65	2.70	2.09	2.46	2.04	2.39
COOL	4.85	4.72	4.55	5.03	4.78	4.57
Additional:						
GIRL	5.32	5.09	4.96	4.32	4.24	3.52
IEGO	5.84	6.23	6.35	5.75	6.28	6.40
GANG	4.63	5.24	5.92	4.56	5.00	5.81
SELF	5.26	5.64	5.92	4.88	5.24	5.50
LSD:[a]						
$p = .05$	0.17	0.16	0.17	0.21	0.20	0.17
DSD:						
$p = .05$	0.38	0.55	0.99	0.58	0.86	0.60
$p = .01$	0.50	0.73	1.34	0.77	1.15	0.80

[a] For each column lowest significant differences (LSD) are such that any lower are not significant; definitely significant differences (DSD) are such that any equal or higher are significant at the given level.

TABLE 4

IMAGES EVALUATED SIGNIFICANTLY HIGHER BY ROW SAMPLE THAN BY COLUMN SAMPLE (.05 LEVEL OR BETTER)[a]

	Negro			White		
	Gang	Lower Class	Middle Class	Gang	Lower Class	Middle Class
Negro:						
Gang		_FENC*_ _CONN*_ PIMP	SJOB HANG* TUFF*** FENC* _CONN*_ _PIMP**_ DRUG	SHAR PIMP*** GIRL*** SELF	SJOB* FENC*** CONN*** PIMP*** DRUG* GIRL**	SJOB*** HANG*** TUFF*** FENC*** CONN*** PIMP*** GIRL***
Lower class	_IEGO*_ _GANG**_ _SELF*_		HANG** TUFF** DRUG	_GRAD_ SHAR* _PIMP_ _GIRL**_ _IEGO_ _GANG*_ _SELF***_	_SJOB*_ CONN PIMP** DRUG* GIRL* _SELF_	SJOB** HANG*** TUFF** CONN*** PIMP*** GIRL***
Middle class	_IEGO*_ _GANG***_ _SELF**_	_GANG**_		_SHAR_ _GIRL_ _IEGO*_ GANG*** SELF***	PIMP GIRL GANG*** SELF*	_HANG_ CONN* PIMP** GIRL*** _SELF_
White:						
Gang			_SJOB_ HANG* TUFF*** FENC _CONN_		_SJOB*_ FENC* CONN*** PIMP	SJOB** HANG*** TUFF*** FENC* CONN*** PIMP*** COOL GIRL**
Lower class	_IEGO*_		_TUFF**_	_GRAD_ _IEGO_		HANG*** _TUFF**_ _STIK_ _CONN_ _GIRL_
Middle class	IEGO*** GANG***	GANG**		_IEGO*_ GANG*** SELF**	GANG**	

[a] Italicized images are significant for evaluation, but not for smartness. Compare with Table 6.

* $p < .01$. ** $p < .001$. *** $p < .0001$.

for example, speak of "the legitimacy of social rules," which may be questioned by members of a socially disadvantaged population quite apart from their "moral validity." They assert that gang members no longer accord legitimacy to middle-class norms because of social barriers obstructing their access to the opportunities implied by the norms.[26] Cohen too recognizes the importance of legitimacy when he states:

> For the child who temporizes with middle-class morality, overt aggression and even the conscious recognition of his own hostile impulses are inhibited, for he acknowledges the *legitimacy* of the rules in terms of which he is stigmatized. For the child who breaks clean with middle-class morality, on the other hand, there are no moral inhibitions on the free expression of aggression against the sources of his frustration.[27]

Even Miller may be responding to the legitimacy aspect of attitudes toward norms when he asserts that lower-class culture is relatively autonomous. If it were true that the evaluation dimension reflects moral validity rather than legitimacy, these data would not constitute a proper test of the theories.

It seems reasonable to infer that anyone who complies with norms that lack legitimacy from the viewpoint of someone else faces the prospect of being branded a "sucker," that is, someone who is taken in and fooled by superficial appearances. This line of reasoning, coupled with Miller's assurances that "smartness," in exactly this sense, is a criterion of behavior to which lower-class and gang members are sensitive, led to the "smart-sucker" scale as a measure of legitimacy.

It might be argued that the "smart-sucker" scale is subject to being construed as an "intelligent-unintelligent" scale, and that since the three middle-class images suggest rather cerebral types of performance, all the boys will rate these images high on smartness. On the other hand, the scale was always read aloud to the boys as part of an example of how to fill out the instrument: "Is he smart? Or is he a sucker?" Calling attention in this way to the presence of "sucker" at one end of the scale should be effective in defining its dimensionality, especially, according to Miller, for lower-class and gang boys. Since these are the boys who presumably are motivated to withhold legitimacy from such images, the combination of this motivation with their sensitivity should be reflected in differential responses, even (or perhaps especially) if the middle-class boys, lacking both the sensitivity and motivation, construe the scale as an intelligence measure.[28] Actually, there is no reason to suppose that middle-class boys would not understand quite well a continuum delineated by "smart" and "sucker."

But the hypotheses placed in question by the evaluative findings enjoy only a brief respite. The smartness scores for all three middle-class images for all populations are also virtually identical. Of forty-five comparisons, only one is significant; Negro middle-class boys rated SAVE smarter than white lower-class boys, at the .05 level (see Tables 5 and 6). The smartness ratings of middle-class images by all populations are higher than those for any other subcultural image. Some readers may note that both gang samples rated READ noticeably lower on smartness than members of the other populations (the differences are not significant), and also that the three strata of both races were ordered for GRAD so that gang boys are lowest and middle-class boys highest. However, this ordering is offset by the fact that both gang samples rated the third image, SAVE, higher than the white middle-class boys. While slightly suggestive, this evidence falls short of the dramatic differences that the Cloward-Ohlin theory would seem to require, especially in view of the absolutely high ratings of these images as compared to the deviant images for all six populations. On the basis of significance tests, one must conclude that there is evi-

[26] Cloward and Ohlin, *op. cit.*, pp. 16-20, 136-37.

[27] *Op. cit.*, p. 132.

[28] For a more thorough discussion of this point see American Documentation Institute, *op. cit.*, Note C.

dence of neither differential nor low legitimation of the behaviors represented by the middle-class images by any population.

A sharp difference is to be noted between the smartness ratings, for all populations, of SHAR and SAVE; this difference was *not* reflected in the evaluation scores. All populations feel that it is much smarter to save than to share, while five out of the six evaluated SHAR higher than SAVE. (All six smartness differences between SHAR and SAVE are significant at the .0001 level.) Smartness is thus a more sensitive indicator than evaluation of behavior that a person would actually indorse after a realistic appraisal of its material consequences and the justice of its attendant social expectations. Hence its use as a measure of legitimacy in the sense intended by Cloward and Ohlin appears to be justified. Intuitively, it would seem that if the smartness score registers the difference in utility of the behaviors represented by SAVE and SHAR, it should also reflect any tendency by gang boys to view the middle-class behaviors as deficient in utility.

TABLE 5

SMARTNESS MEANS

IMAGES	Negro			White		
	Gang	Lower Class	Middle Class	Gang	Lower Class	Middle Class
Middle class:						
GRAD	6.35	6.61	6.58	6.43	6.54	6.66
READ	6.03	6.22	6.21	6.07	6.27	6.22
SAVE	6.37	6.49	6.67	6.55	6.03	6.29
Lower class:						
SJOB	4.99	4.70	4.17	4.69	4.30	4.10
HANG	4.34	3.99	3.17	4.40	4.14	2.61
SHAR	4.59	4.72	3.12	3.97	3.54	4.12
Conflict:						
TUFF	4.15	4.03	3.21	4.79	3.86	3.27
STIK	5.20	5.51	4.96	5.64	5.46	4.98
Criminal:						
FENC	4.99	4.38	3.75	4.98	3.24	3.15
CONN	5.75	5.33	4.96	5.71	3.70	3.80
Retreatist:						
PIMP	4.39	3.57	3.62	3.69	2.08	1.61
DRUG	2.37	2.04	1.17	1.93	1.19	1.15
COOL	5.63	5.26	4.12	5.90	4.54	3.76
Additional:						
GIRL	6.08	5.59	5.75	5.24	4.46	3.83
IEGO	6.21	6.45	6.33	6.64	6.43	6.76
GANG	5.58	5.71	6.00	5.40	5.43	6.05
SELF	5.79	5.49	5.75	5.14	5.14	5.63
LSD:[a]						
$p=.05$	0.20	0.14	0.11	0.24	0.22	0.13
DSD:						
$p=.05$	0.54	0.84	1.69	0.93	1.27	1.08
$p=.01$	0.72	1.12	2.29	1.23	1.71	1.44

[a] For each column lowest significant differences (LSD) are such that any lower are not significant; definitely significant differences (DSD) are such that any equal or higher are significant at the given level.

Hypotheses 1 through 4 are not supported by these data. And even when "legitimate" is substituted for "evaluate" in these hypotheses, they are still not supported by data based on "smartness" as a measure of legitimacy.

Lower-class images.—Hypotheses 5, 6, and 7 all deal with lower-class images. Contrary to hypothesis 5, neither white nor

TABLE 6

IMAGES RATED SIGNIFICANTLY SMARTER BY ROW SAMPLE THAN BY COLUMN SAMPLE (.05 LEVEL OR BETTER)[a]

	NEGRO			WHITE		
	Gang	Lower Class	Middle Class	Gang	Lower Class	Middle Class
Negro:						
Gang		PIMP* _GIRL_	SJOB* HANG** _SHAR_*** TUFF* FENC DRUG*** _COOL_**	SHAR PIMP GIRL* SELF*	SJOB* SHAR* FENC*** CONN*** PIMP*** DRUG*** _COOL_* GIRL*** _SELF_*	SJOB** HANG*** TUFF* FENC*** CONN*** PIMP*** _DRUG_*** _COOL_*** GIRL***
Lower class			HANG _SHAR_*** TUFF DRUG*** _COOL_*	SHAR	_SHAR_* _FENC_ CONN** PIMP*** DRUG** GIRL*	SJOB HANG*** TUFF _FENC_* CONN PIMP*** _DRUG_*** _COOL_*** GIRL***
Middle class				GANG SELF	_SAVE_ _CONN_ PIMP* GIRL* GANG SELF	CONN PIMP*** GIRL***
White:						
Gang	TUFF _IEGO_*	_TUFF_ _COOL_	HANG* _SHAR_ TUFF*** _STIK_ FENC _DRUG_* _COOL_***		_TUFF_* FENC** CONN*** PIMP*** _DRUG_* _COOL_**	SJOB HANG*** TUFF*** _STIK_ FENC*** CONN*** PIMP*** _DRUG_* COOL***
Lower class			_HANG_			HANG**
Middle class	IEGO*** GANG*	_IEGO_ GANG	_SHAR_	GANG* SELF	_IEGO_ GANG* _SELF_	

[a] Italicized images are significant for smartness, but not for evaluation. Compare with Table 4.
* $p < .01$. ** $p < .001$. *** $p < .0001$.

Negro gang boys evaluated SHAR and HANG higher than middle-class images. Contrary to hypothesis 6, neither gang nor lower-class boys evaluated any of the lower-class images significantly higher than any of the middle-class images.

Before interpreting these results, the relevant row comparisons for hypothesis 7, involving SHAR, SJOB, and HANG, must be considered. The evaluation means for SHAR show no interpretable pattern (although all Negro samples evaluated sharing significantly higher than white gang boys). However, both gang samples evaluated SJOB and HANG significantly higher than both middle-class samples; both lower-class samples evaluated HANG significantly higher than their racially matched middle-class sample; and the Negro lower class evaluated both SJOB and HANG significantly higher than the white middle class.

Joined with the patterns of these data, these significant findings strongly support hypothesis 7, derived from Miller. In effect, this supports Miller's general contention that the values of the lower class are distinguishable from those of the middle class. The failure of hypotheses 5 and 6 suggests that these differences are based more heavily on attitudes toward lower-class norms than on those toward middle-class norms.

These findings suggest that the idea of sharing money with friends taps a set of normative expectations that is more nearly universal than those associated with work and leisure, so that SHAR differentiates the samples only when the smartness scores, raising considerations of legitimacy or practicality, are inspected. As a matter of general interest, attention is called to the fact that gang boys of both races—together with Negro lower-class boys—evaluated and legitimated higher than anyone else the idea of having a humble job in a gasoline station.

The nature of the remaining images.—The remaining subcultural images (plus GIRL) were chosen to represent behaviors that the theories hypothesize as deviant alternatives to a respectable style of life, either middle class or lower class. Not all of these behaviors are technically illegal or necessarily indicative of antisocial intent. The behavior described by COOL and STIK is intrinsically innocuous, and in the latter case even commendable. TUFF and GIRL, if perhaps more clearly at variance with middle-class codes, nevertheless entail no necessary legal violation. And DRUG suggests behavior that, although illegal, is often more self-injurious than harmful to others. As a result, although the images were employed principally to aid in the identification of delinquent subcultures among gangs, they also represent points in what might be regarded—from a middle-class standpoint—as the middle and lower ranges of an evaluative continuum. It is in this range of such a continuum that the most striking differences between samples appear.

Some consistent racial differences and masculinity.—Inspection of the main diagonal of the upper right quadrant (which compares the races holding social level relatively constant) of Table 4 discloses three images that Negro boys evaluated significantly higher than white boys within each one of the three social levels. These images are PIMP, GIRL, and SELF. The consistent reappearance of this constellation is slightly suggestive of a narcissistic syndrome among Negro adolescent males. That two of these images figure in the sex-identity hypothesis supplements rather than precludes this possible interpretation. However, the higher evaluation of SELF by Negroes (GANG follows the same pattern) could also be a defense against low racial self-esteem, such as was recently suggested by James W. Vander Zanden.[29] Sexual self-indulgence, narcissism, and defensive self-esteem all tend to shade into one another, and to disentangle these concepts would require more discriminating measures. Although tentative, such interpretations are of interest, however, in view of E. Franklin Frazier's description of Negro middle-class males as tending to "cultivate their 'person-

[29] See his "The Non-Violent Resistance Movement against Segregation," *American Journal of Sociology*, LXVIII, No. 5 (1963), 544–50, and the literature cited there.

alities,' " a phrase suggestive of narcissistic concern, and evidence that Negroes spend more for food, clothing, and automobiles than whites at the same income level.[30] Negro consumer habits are apt to be attributed to their lack of other economic outlets and status-seeking, but it may be that such behavior reflects a deeper and more pervasive kind of self-indulgence (perhaps also compensatory) that manifests itself also in noneconomic behavior.

Frazier's observations are especially reassuring concerning findings, for example, for PIMP and GIRL, that show the Negro middle class to be deviant in some respects from the white middle class and perhaps even from white gang boys. Although hypothesis 8 predicted such results between the two middle-class samples, the magnitude of the differences at first aroused strong misgivings as to the representativeness of the Negro middle-class sample.

Nevertheless, the data are consistent with other information indicating that these Negro middle-class boys were definitely active sexually. Their sexual success is easy to account for in view of their strong competitive position, based on polish, money, and cars, and the sexual permissiveness of Negro lower-class girls. Probably, it is difficult for Negro middle-class girls to compete under these conditions without becoming themselves sexually accessible.

An impression of the sexual activity of these boys can be gained from an incident which occurred after one of the testing sessions. One boy raised his hand politely to inquire whether they could now ask the tester some questions. Anticipating curiosity about the tests, the tester invited any questions the boys might wish to pose. The first question, put with sincere concern, was, "If you do it (sexual intercourse) too much, is it true that you give out young?" The question drew little laughter.

Final doubts concerning the possible representativeness of this sample were then erased by Frazier, whose description of the Negro middle class emphasizes mediocre aspirations (the boys spoke of being physical education teachers, not doctors or lawyers), overcommitment to material satisfactions (they dressed extremely well, and arrived driving their own family cars), sexual promiscuity (already indicated), and involvement in recreation (one admitted, "all *we* do is party").[31] Everything pictured by Frazier seems to fit, even down to the fact that the two clubs to which these boys belonged were the only groups to refer to themselves by Greek-letter names and to order "pledges" around in a semi-autocratic manner.

In interpreting the racial differences, however, it must be kept in mind that the Negroes in each social category really are socioeconomically lower than the whites in the corresponding category (see Table A).

Turning to the sex-identity hypothesis proper, all the various predictions from that compound hypothesis hold for the evaluative patterns of PIMP and GIRL; most of them are statistically significant as well. Furthermore, the mean scores for these two images display similar gradients, running from left to right across all six populations in Table 3. In each case Negro gang boys are highest, each succeeding sample being lower until white middle-class boys appear as the lowest. Both race and social level thus produce differences in the evaluation of PIMP and GIRL that accord with the sex-identity hypothesis; quite unexpected, however, is the finding that at every social level the Negro boys evaluated these two images higher than *any* sample of white boys.

With slight exceptions, the pattern for smartness of these two images is much the same. One noteworthy change is that non-gang Negro boys drop slightly below white gang boys in the legitimation of PIMP; since this places all non-gang boys now lower than all gang boys it suggests that the non-gang Negroes have reservations about pimping and illegal "hustling" that are not reflected in their evaluation of PIMP.

The third image included in the mascu-

[30] See *Black Bourgeoisie* (Glencoe, Ill.: Free Press, 1957), p. 220. On the Negro consumer see, e.g., "The Negro Market," *Time*, February 9, 1962, pp. 80–81.

[31] *Op. cit.*

linity hypothesis was TUFF. This image was evaluated significantly higher by gang and lower-class boys of both races than by either of the middle-class samples. Within each stratum, the whites were higher, although not significantly so. This is contrary to the expectation stated in hypothesis 8 that the Negroes would evaluate TUFF higher at each social level, as they did PIMP and GIRL. However, since both gang samples also evaluated TUFF higher than did their racially matched lower-class samples—although not significantly—the strata are ordered in accordance with the hypothesis. The smartness ratings produced a similar ordering of strata, but no sign of consistently higher ratings by Negroes. In fact, white gang boys legitimated TUFF significantly higher than Negro gang boys; this is the only instance in which a deviant image received a significantly higher rating from white gang boys than from Negro gang boys.

While it is felt that the preponderance of this evidence is consistent with the hypothesis dealing with problems of sex identity (though by no means proving it), the failure of TUFF to parallel the differences for race exhibited by PIMP and GIRL is puzzling. Whether sex identity or simply subcultural norms are responsible, these results suggest a degree of independence between attitudes toward the sexual and the aggressive expression of masculinity.[32] (It is interesting to note that between PIMP and TUFF, Negro gang boys favor PIMP, whereas white gang boys significantly favor TUFF at the .0001 level for evaluation and the .01 level for smartness.) Vander Zanden has also called attention to the historical necessity for Negroes to suppress aggression; possibly this accounts for the fact that for TUFF five out of six comparisons of evaluation and smartness means, within stratum, show Negroes lower then whites, despite the tendency for Negroes to be generally more tolerant than whites toward the other deviant images.[33]

The narcotics image.—Of all images, DRUG received the lowest evaluation from both gang samples, and the lowest legitimation from everyone. (Both lower-strata Negro samples evaluated DRUG significantly higher than Negro middle-class and white lower-class boys, and both gang samples and the Negro lower class legitimated it significantly higher than all three remaining populations.)

In view of the consistently low tolerance shown toward most other deviant images by the white middle class, their evaluation of DRUG seems rather high compared to the Negro middle class and the white lower class. However, these three samples are virtually identical in rejecting DRUG's legitimacy; here, the white middle class accords it the lowest smartness score in the entire table. Personal knowledge gained in working with the white middle-class boys suggests that their relatively higher evaluation score may be a reflection of sophisticated compassion. If so, this provides another indication of meaningful independence between the two scores.

Criminal images, utility, and legitimacy.—The two images representing the criminal subculture were FENC and CONN. With minor imperfections, both of these images manifest a gradient that appears repeatedly among the deviant images for both evaluation and smartness: Gang > Lower Class > Middle Class. (Perfect examples of this gradient may be noted for PIMP, GIRL, and COOL.) Negro gang boys evaluated FENC and CONN significantly higher than all four non-gang samples; white gang boys evaluated them higher than all non-gang boys except those in the Negro lower class. Both

[32] A plausible case for race differences in sexual permissiveness may be derived from the comparison of sex norms as described for Negro gang boys and the Negro middle class, on the one hand, and the white lower class, on the other. For Negro gang boys see James F. Short, Jr., Fred L. Strodtbeck, and Desmond S. Cartwright, "A Strategy for Utilizing Research Dilemmas: A Case from the Study of Parenthood in a Street Corner Gang," *Sociological Inquiry*, XXXII, No. 2 (1962). For the Negro middle class see Frazier, *op. cit*. For somewhat dated accounts of the white lower class as contrasted with the white middle class see William Foote Whyte, "A Slum Sex Code," *American Journal of Sociology*, XLIX, No. 1 (1943), 24–31, and Arnold W. Green, "The 'Cult of Personality' and Sexual Relations," *Psychiatry*, IV (1941), 343–48.

[33] *Op. cit.*, pp. 545–46.

gang samples, and the Negro lower class, legitimated FENC and CONN significantly higher than the non-gang whites. The Negro middle class evaluated and legitimated CONN significantly higher than the white middle class.

The two criminal images are among those that differentiate gang boys from lower-class boys: PIMP, FENC, and CONN for both races and, in addition, SJOB for whites. The gang and lower-class Negroes both evaluated SJOB at virtually the same high level. These four images have in common a utilitarian emphasis, indicating that the role of material gain in the values of gang boys is by no means negligible. That gang members do not repudiate the possibility of legitimate gain is indicated by the presence of SJOB. The prominence of the illegitimately gainful images suggests, however, that a choice between legal and illegal means is determined to a lesser degree in favor of legal means for gang boys than for lower-class boys.

It can be shown, too, that the consideration of legitimacy or practicality appears even more conducive than that of evaluation to a choice of illegal means, especially for gang boys. For example, all six samples *evaluated* SJOB significantly higher than FENC (each at the .0001 level). However, only the non-gang whites *legitimated* SJOB significantly higher than FENC (white lower class, .01; white middle class, .02), while Negro gang boys now tied the two images, and white gang boys legitimated FENC higher than SJOB.

A similarly revealing comparison concerns SJOB and CONN. SJOB was evaluated significantly higher than CONN by all four non-gang samples (Negro and white lower class, .01; Negro middle class, .05; white middle class, .001), and the gang samples showed the same tendency. However, except for the non-gang whites, all samples legitimated SJOB and CONN in reverse order, with both gang samples now rating CONN significantly higher than SJOB (Negro gang, .0001; white gang, .001; and Negro lower class, .10).

Not only the respectable image, SJOB, declines relative to criminal images when the basis of comparison is shifted to smartness. The conflict image, TUFF, follows the same pattern, thus indicating that it is not respectability per se that is the determinant, but rather differential practicality or utility. All samples evaluated TUFF higher than FENC; for the four lower strata these differences are significant (Negro and white lower class and white gang, .0001; Negro gang, .01). This order of the images is reversed for smartness by all but the non-gang whites, with the Negro gang sample now attaining significance (.001) in the new direction.

These last findings corroborate the importance that Miller attaches to smartness and Cloward and Ohlin to legitimacy. In some respects, however, they are not fully congruent with the expectations generated by these theorists. The tendency for smartness to rearrange the orderings of images for the Negro middle class, but not for the white lower class—in the examples given above—is not consistent with Miller's locating the salience of this dimension chiefly in the lower class. And the finding that gang boys grant greater legitimacy to deviant images, while not withdrawing legitimacy from middle-class images, does not accord with Cloward and Ohlin's emphasis on middle-class norms as sensitive to considerations of legitimacy. Their contention would now seem to apply to middle-class proscriptive norms, but not to middle-class prescriptive norms.

A quick review of the statistical findings and patterns for deviant images will indicate that hypotheses 9 and 10 are supported, and in this respect all three theories are correct. However, where none of the theories specified differences between each possible pairing of the three social levels, these data strongly indicate a gradient for attitudes toward deviant behaviors, such that the acceptability of these behaviors is inversely related to social level.

Another gradient.—The images GANG, SELF, and IEGO display a social level gradient opposite to that for deviant images; the higher the level, the more highly these

three images are evaluated (see Tables 4 and 6 for significance levels). For the image GANG, this trend suggests that the gang is not the close-knit, highly cohesive entity which some might expect.[34] Conceivably, the trend for IEGO reflects superego strength.

The images SELF and IEGO serve to indicate the direction of preference for these scores, thus ruling out the possibility that gang boys completely invert the evaluative dimension while continuing to describe behavior verbally much as middle-class people might. All six samples wanted to be significantly better (Negro middle class, .001; all others, .0001) and smarter (Negro gang, .01; Negro middle class, .10; all others .001) than they usually are. An analysis of the five individual evaluative scales for seventeen of the groups making up the total sample showed this directionality for SELF and IEGO to prevail throughout, with the scale "good-bad" always contributing its proportionate share of the gain within each of the six samples.

DISCUSSION

The finding that delinquent boys order behaviors as to their goodness much the same as do non-delinquents is not new, having been demonstrated as early as 1940 by Ruth Bishop.[35] Although Bishop was able to show that both delinquent and non-delinquent populations divide good from bad behaviors at the same neutral point, her technique leaves one in doubt as to whether her data reflect the affective preferences of her populations or merely their equal ability to perform a cognitive judgment task.[36]

Osgood, however, has come to the conclusion after years of experience that semantic differential responses have an affective character, apparently coinciding in dimensionality with universal dimensions of affective meaning applicable to all sensory modalities. He also feels that these dimensions typify ways in which people respond or react to their environment, rather than ways in which they receive and organize incoming stimuli.[37] This would seem to imply a greater relevance for behavior than if semantic differential responses merely recorded the passive categorizing of external stimuli. In addition, the global connotative richness of the five evaluative scales, the direction of the differences between IEGO and SELF, and the fact that for college students rating Morris' "ways to live" on a semantic differential the correlation between a heavily evaluative factor and preference was .66 for individual scores and .93 for group means[38]—all indicate strongly that evaluation and preference are closely related.

Although such considerations do much to clarify the meaning of the observed responses, it is nevertheless difficult to comprehend their full significance until the data are tied into a complex net of additional evidence. For despite the specificity of the images, the behaviors they represent were necessarily judged entirely apart from the contexts in which they are normally encountered by members of the six populations. The responses, therefore, must be viewed as having an "in principle" quality,[39] which, from the standpoint of assessing values, is not at all inappropriate, although it does imply that the information so obtained may be seriously incomplete for the purpose

[34] Yablonsky has also called into question the cohesiveness of gangs (see Lewis Yablonsky, "The Delinquent Gang as a Near-Group," *Social Problems*, VII [Fall, 1959], 108–17).

[35] "Points of Neutrality in Social Attitudes of Delinquents and Non-Delinquents," *Psychometrika*, V, No. 1 (1940), 35–45.

[36] The ambiguities involved in making this determination are discussed by Warren S. Torgerson, *Theory and Methods of Scaling* (New York: John Wiley & Sons, 1958), pp. 48–49.

[37] Charles E. Osgood, "Studies on the Generality of Affective Meaning Systems," *American Psychologist*, XVII (January, 1962), 10–28.

[38] Charles E. Osgood, Edward E. Ware, and Charles Morris, "Analysis of the Connotative Meanings of a Variety of Human Values as Expressed by American College Students," *Journal of Abnormal and Social Psychology*, LXII, No. 1 (1961), 62–73.

[39] This "in principle" quality corresponds to the idea of "potential demand" in values as used by Cyril S. Belshaw, who elaborates further its implications for behavior ("The Identification of Values in Anthropology," *American Journal of Sociology*, LXIV, No. 6 [1959], 555–62).

of explaining behavior. For example, some of the populations may view their own *real* school experiences in a highly unfavorable light, for a variety of both objective and subjective reasons, and yet maintain an essentially positive attitude toward the idea of education in general. This does not imply that it is any less important to know what these more abstract attitudes are.

The data imply that acceptance of middle-class prescriptive norms (the middle-class images) is quite general, while middle-class proscriptive norms (the deviant images) either decline in force or are rejected more strongly as social level goes down. The former alternative suggests a weakening of inhibitory mechanisms as social level declines, perhaps ultimately traceable to a superego construct, such as was suggested by the IEGO gradient for evaluation. The latter alternative, of rejection, raises somewhat more strongly the possibility of a "rationally" motivated choice, as indicated by the sensitivity of the images CONN and FENC to the practical emphasis of smartness for gang boys. The two alternatives need not be mutually exclusive.

In any case, the delicacy of the prescriptive-proscriptive balance achieved in their evaluations by gang boys raises the question of whether it indicates ambivalence toward middle-class culture as a whole of the sort claimed by Cohen. Certainly, that would be a plausible interpretation. However, given that the hypothesis of reaction formation does not seem to be supported,[40] and that ambivalence can be said to exist whenever competing alternatives are present, the concept of ambivalence by itself lacks explanatory force.

In addition, it remains to be demonstrated that gang boys perceive legitimate and illegitimate behaviors as being in some sense mutually exclusive, so that a choice of one has strong implications for their realization of the other. Without such a demonstration, even equal evaluation of both kinds of behavior would not constitute sufficient evidence for ambivalence.

The implication in these data that gang boys evaluate highest behavior that appears as remote from their actual conduct as that depicted by GRAD, READ, and SAVE will undoubtedly strike many persons as an absurdity. Certainly, if the finding is valid, three separate theoretical formulations failed to make sufficient allowance for the meaningfulness of middle-class values to members of gangs. To others, the apparent pervasiveness of middle-class values in American life may come as no surprise; Cloward and Ohlin, it will be recalled, actually postulated that gang boys share middle-class consumption values, although contrary to this paper's indications they also held that middle-class norms are not legitimated by gang boys. Miller has given reason to believe that he would dismiss these findings as indicative merely of "official" ideals.[41]

A number of points, bearing also on the more general problem of accounting for the disparity between theories and these findings, can be made in response to such a criticism. For one thing, the allegation that gang boys mirror official ideals in their responses is consistent neither with the finding that they rated images which are highly deviant, such as PIMP, significantly higher than non-gang boys, nor with the social level gradients for deviant images.

A second point concerns both the role of values in social theory and the methodology by means of which values are identified. Unless it is to be seriously maintained that values strictly determine behavior, or vice versa, one must be prepared for findings such as these. The discrepancy between these findings and the values reported for lower-class culture (with respect to middle-class values) by Miller may be related to his anthropological methodology. The anthropologist often assesses values by inferring them from extended observation of a population's spontaneous behavior, including verbal behavior. Since Miller has described the focal

[40] For other evidence against the reaction-formation hypothesis see Albert J. Reiss, Jr., and Albert Lewis Rhodes, "Delinquency and Social Class Structure," *American Sociological Review*, XXVI, No. 5 (1961), p. 729.

[41] *Op. cit.*, p. 7.

concerns as more readily derivable from direct observation than values, and also reflecting "actual behavior," it follows that this was also his method.[42]

When studying an entire primitive society in this way one can be fairly certain of having witnessed the full range of behavior that members of that society hold in high regard, given the relatively constant constraints of the physical environment. However, when this method is applied to subcultures contained within a single society, it is apt to lead to fallacious results; for, in such an instance, the values of the populations studied can never be reported as other than those implied by their behavior.

Within a complex society, the existence of a differentiated segment of population may result from the operation of processes that constrain behavior in ways independent of, and in addition to, the constraints imposed by the values of that particular segment's members. To deny this is to favor an overly simple model of society. Such constraining processes can be either external or internal to a subculture. The limitation on opportunity that the larger society imposes on certain minority groups would be an example, from the Merton tradition, of an external constraint. The hypothesized female-dominated household would be an internal constraint. This hypothesis asserts that within lower-class culture there exists a self-maintaining process that leads males to behave in self-defeating ways; this, in turn, implies the frustration of tendencies to behave in ways that may actually be held in high regard.

Not even the anthropologist's use of verbal behavior, especially public or spontaneous verbal behavior, is free from this criticism if it is granted that what members of a subculture verbalize may itself reflect or even constitute a basis for their being differentiated from the larger population, despite their own deepest preferences. This criticism gains plausibility when it is noted that behaviors readily available to gang and lower-class members and hence visible to the anthropologist, such as criminality, promiscuity, and pimping, are ones upon which Miller and the semantic differential are in accord; behaviors whose realization may be limited for lower-class persons—as represented by the middle-class images—are ones over which Miller and the semantic differential disagree.

In view of the unexpected nature of some of these findings, additional efforts to test their validity are being undertaken. If they are valid, the interpretations that may prove most important to the refinement of delinquency theory are the following: (1) For all six populations, the indorsement "in principle" of middle-class prescriptive norms is uniformly high. (2) Gang, lower-class, and middle-class boys differ most in their attitudes toward behaviors proscribed by the middle class, and they tend to be ordered as listed with respect to their tolerance toward these behaviors. (3) Legitimacy or practicality, as measured by a "smart-sucker" scale, seems to be a meaningful basis for distinguishing behavior. There is some evidence that gang boys, more than other boys, may be led by this distinction to a choice of criminal behavior over legitimately gainful behavior. (4) The hypothesis of a sex-identity problem for lower-class and gang boys appears worth pursuing further.

Since these interpretations are not derived from true probability samples, it will be necessary for readers to employ discretion in applying them to other universes. A consideration of the degree to which other universes might reasonably be expected to differ in these respects from this paper's samples should be of some guidance.

It is anticipated that the implications of these findings will be better understood if their further development is deferred until other relevant data have been analyzed.

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[42] *Ibid.*

DIFFERENTIAL ASSOCIATION AND THE REHABILITATION OF DRUG ADDICTS

RITA VOLKMAN AND DONALD R. CRESSEY

ABSTRACT

Five social psychological principles for the rehabilitation of criminals, formulated in this *Journal* by Cressey in 1955, have unwittingly been used in a program for rehabilitating drug addicts. Sixty-six per cent of the addicts who stayed in the program for at least three months, and 86 per cent of those who remained for at least seven months, are still not using drugs or alcohol.

In 1955 Cressey listed five principles for applying Edwin Sutherland's theory of differential association to the rehabilitation of criminals.[1] While this article is now frequently cited in the sociological literature dealing with group therapy, "therapeutic communities," and "total institutions," we know of no program of rehabilitation that has been explicitly based on the principles. The major point of Cressey's article, which referred to criminals, not addicts, is similar to the following recommendation by the Chief of the United States Narcotics Division: "The community should restore the former addict to his proper place in society and help him avoid associations that would influence him to return to the use of drugs."[2]

Cressey gives five rules (to be reviewed below) for implementing this directive to "restore," "help," and "influence" the addict. These rules, derived from the sociological and social-psychological literature on social movements, crime prevention, group therapy, communications, personality change, and social change, were designed to show that sociology has distinctive, non-psychiatric, theory that can be used effectively by practitioners seeking to prevent crime and change criminals. Sutherland also had this as a principal objective when he formulated his theory of differential association.[3]

Assuming, as we do, that Cressey's principles are consistent with Sutherland's theory and that his theory, in turn, is consistent with more general sociological theory, a test of the principles would be a test of the more general formulations. Ideally, such a test would involve careful study of the results of a program rationally designed to utilize the principles to change criminals. To our knowledge, such a test has not been made.[4] As a "next best" test, we may study rehabilitation programs that use the principles, however unwittingly. Such a program has been in operation since 1958. Insofar as it is remarkably similar to any program that could have been designed to implement the principles, the results over the years can be viewed as at least a crude test of the principles. Since the principles are interrelated, the parts of any program implementing them must necessarily overlap.

"Synanon," an organization of former drug addicts, was founded in May, 1958, by a member of Alcoholics Anonymous with the assistance of an alcoholic and a drug addict. In December, 1958, Volkman (a non-addict) heard about the two dozen ex-addicts living

[1] Donald R. Cressey, "Changing Criminals: The Application of the Theory of Differential Association," *American Journal of Sociology*, LXI (September, 1955), 116–20 (see also Cressey, "Contradictory Theories in Correctional Group Therapy Programs," *Federal Probation*, XVIII [June, 1954], 20–26).

[2] Harry J. Anslinger, "Drug Addiction," *Encyclopaedia Britannica*, VII (1960), 677–79.

[3] Edwin H. Sutherland and Donald R. Cressey, *Principles of Criminology* (6th ed.; Philadelphia: J. B. Lippincott Co., 1960), pp. 74–80.

[4] See, however, Joseph A. Cook and Gilbert Geis, "Forum Anonymous: The Techniques of Alcoholics Anonymous Applied to Prison Therapy," *Journal of Social Therapy*, III (First Quarter, 1957), 9–13.

together in an abandoned store, and she obtained permission of the Synanon Board of Directors[5] to visit the group daily and to live in during the weekends. In July, 1959, she moved into the girls' dormitory of the group's new, larger quarters and continued to reside at Synanon House until June, 1960. Cressey (also a non-addict) visited the House at Volkman's invitation in the spring of 1960; for one year, beginning in July,

TABLE 1

AGE AND SEX*

Age (In Years)	Males		Females		Total	
	No.	Per Cent	No.	Per Cent	No.	Per Cent
18–20....	0	0	1	7	1	2
21–30....	17	44	11	79	28	54
31–40....	18	48	2	14	20	38
41–50....	1	3	0	0	1	2
51–60....	2	5	0	0	2	4
Total..	38	100	14	100	52	100

* Median ages: males, 31.0; females, 27.5.

1960, he visited the organization on the average of at least once a week. He deliberately refrained from trying to influence policy or program, and his theory about the effects of group relationships on rehabilitation were unknown to the group. Most of the interview material and statistical data reported below were collected by Volkman during her 1959–60 period of residence and were used in the thesis for her Master's degree, prepared under the direction of C. Wayne Gordon.[6] As both a full-fledged member of Synanon and as a participant observer, Volkman attended about three hundred group sessions, a few of which were recorded. She was accorded the same work responsibilities, rights, and privileges as any other member, and she was considered one of Synanon's first "graduates."

THE SUBJECTS

Background data were available on only the first fifty-two persons entering Synanon after July, 1958. These records were prepared by a resident who in July, 1959, took it upon himself to interview and compile the information. We have no way of determining whether these fifty-two persons are representative of all addicts. However, we believe they are similar to the 215 persons who have resided at Synanon for at least one month.

Age and sex distributions are shown in Table 1: 44 per cent of the fifty-two were Protestant, 35 per cent Catholic, 8 per cent Jewish.[7] Racially, 27 per cent were Negro, and there were no Orientals; 19 per cent of the Caucasians were of Mexican origin and 13 per cent were of Italian origin. Educational attainment is shown in Table 2. Although the data on early family life are poor because the resident simply asked "What was your family like?" it may be noted that only five of the fifty-two indicated satisfaction with the home. Words and phrases such as "tension," "arguing," "bickering," "violence," "lack of warmth," "went back and forth," and "nagged" were common.[8]

TABLE 2

EDUCATIONAL ATTAINMENT

	No.	Per Cent
Part grade school............	1	2
Completed grade school.......	3	6
Part high school.............	24	46
Completed high school........	11	21
Part college.................	13	25
Completed college............	0	0
Total........................	52	100

[5] The Board at first was composed of the three original members. It is now made up of the founder (an ex-alcholic but a non-addict) and seven long-term residents who have remained off drugs and who have demonstrated their strict loyalty to the group and its principles.

[6] Rita Volkman, "A Descriptive Case Study of Synanon as a Primary Group Organization" (unpublished Master's thesis, Department of Education, University of California, Los Angeles, 1961).

[7] In May, 1961, 20 per cent of the residents were Jewish.

[8] Cf. Research Center for Human Relations, New York University, *Family Background as an Etiological Factor in Personality Predisposition to Heroin Addiction* (New York: the Author, 1956).

The sporadic and tenuous occupational ties held by the group are indicated in Table 3. This table supports the notion that addicts cannot maintain steady jobs because their addiction interferes with the work routine; it suggests also that these members had few lasting peer group contacts or ties, at least so far as work associations go. In view of their poor employment records, it might be asked how the addicts supported their addictions, which cost from $30 to $50 a day and sometimes ran to $100 a day. Only four of the men reported that they obtained their incomes by legitimate work alone; thirty (79 per cent) were engaged in illegitimate activities, with theft, burglary, armed robbery, shoplifting, and pimping leading the list. One man and seven women were supplied with either drugs or money by their mates or families, and five of these females supplemented this source by prostitution or other illegitimate work. Five of the fourteen women had no income except that from illegitimate activities, and none of the women supported themselves by legitimate work only.

TABLE 3

LENGTH AND CONTINUITY OF EMPLOYMENT

No. of Years on One Job	Unsteady (Discontinuous or Sporadic)	Steady (Continuous)	Total
Under 1......	36*	4	40
2-3........	3	2	5
4-5........	1	3	4
6 or over......	2	1	3
Total.......	42	10	52

* Of this category 67 per cent defined their work as "for short periods only."

Institutional histories and military service histories are consistent with the work and educational histories, indicating that the fifty-two members were not somehow inadvertently selected as "easy" rehabilitation cases. The fifty-two had been in and out of prisons, jails, and hospitals all over the United States. Table 4 shows that ten men and one woman had been confined seven or more times; the mean number of confinements for males was 5.5 and for females 3.9. The table seems to indicate that whatever value confinement in institutions might have had for this group, it clearly did not prevent further confinements.

In sum, the pre-Synanon experiences of the fifty-two residents seems to indicate non-identification with pro-legal activities and norms. Neither the home, the armed services, the occupational world, schools, prisons, nor hospitals served as links with the larger and more socially acceptable community. This, then, is the kind of "raw material" with which Synanon has been working.[9]

TABLE 4

CONFINEMENTS IN INSTITUTIONS

No. of Confinements	No.		
	Male	Female	Total*
1- 3.........	9	6	15
4- 6.........	12	7	19
7- 9.........	8	0	8
10-12.........	0	1	1
13-15.........	2	0	2
Total confinements.....	166	59	225

* Three males indicated "numerous arrests," and four supplied no information. These seven were not included in the tally.

THE PROGRAM

Admission.—Not every addict who knocks on the door of Synanon is given admission. Nevertheless, the only admission criterion we have been able to find is *expressed willingness* to submit one's self to a group that hates drug addiction. Use of this criterion has unwittingly implemented one of Cressey's principles:

If criminals are to be changed, they must be assimilated into groups which emphasize values conducive to law-abiding behavior and, concur-

[9] Of the fifty-two members 60 per cent first heard about Synanon from addicts on the street or in jails, prisons, or hospitals; about a fourth heard about it on television or read about it in a magazine; and the remainder were told of it by members or past members.

rently, alienated from groups emphasizing values conducive to criminality. Since our experience has been that the majority of criminals experience great difficulty in securing intimate contacts in ordinary groups, special groups whose major common goal is the reformation of criminals must be created.

This process of assimilation and alienation begins the moment an addict arrives at Synanon, and it continues throughout his stay. The following are two leaders' comments on admission interviews; they are consistent with our own observations of about twenty such interviews.

1. When a new guy comes in we want to find out whether a person has one inkling of seriousness. Everybody who comes here is what we call a psychopathic liar. We don't take them all, either. We work off the top spontaneously, in terms of feeling. We use a sort of intuitive faculty. You know he's lying, but you figure, "Well, maybe if you get a halfway positive feeling that he'll stay. . . ." We ask him things like "What do you want from us?" "Don't you think you're an idiot or insane?" "Doesn't it sound insane for you to be running around the alleys stealing money from others so's you can go and stick something up your arm?" "Does this sound sane to you?" "Have you got family and friends outside?" We might tell him to go do his business now and come back when he's ready to do business with us. We tell him, "We don't need you." "You need *us*." And if we figure he's only halfway with us, we'll chop off his hair.

It's all in the *attitude*. It's got to be positive. We don't want their money. But we may just tell him to bring back some dough next week. If he pleads and begs—the money's not important. If he shows he really cares. If his attitude is good. It's all in the attitude.

2. Mostly, if people don't have a family outside, with no business to take care of, they're ready to stay. They ain't going to have much time to think about themselves otherwise. . . . Now, when he's got problems, when he's got things outside, if he's got mickey mouse objections, like when you ask him "How do you feel about staying here for a year?" and he's got to bargain with you, like he needs to stay with his wife or his sick mother—then we tell him to get lost. If he can't listen to a few harsh words thrown at him, he's not ready. Sometimes we yell at him, "You're a goddamned liar!" If he's serious he'll take it. He'll do anything if he's serious.

But each guy's different. If he sounds sincere, we're not so hard. If he's sick of running the rat race out there, or afraid of going to the penitentiary, he's ready to do anything. Then we let him right in. . . .

This admission process seems to have two principal functions. First, it forces the newcomer to admit, at least on a verbal level, that he is willing to try to conform to the norms of the group, whose members will not tolerate any liking for drugs or drug addicts. From the minute he enters the door, his expressed desire to join the group is tested by giving him difficult orders—to have his hair cut off, to give up all his money, to sever all family ties, to come back in ten days or even thirty days. He is given expert help and explicit but simple criteria for separating the "good guys" from the "bad guys"—the latter shoot dope. Second, the admission process weeds out men and women who simply want to lie down for a few days to rest, to obtain free room and board, or to stay out of the hands of the police. In the terms used by Lindesmith, and also in the terms used at Synanon, the person must want to give up drug *addiction*, not just the drug *habit*.[10] This means that he must at least *say* that he wants to quit using drugs once and for all, in order to realize his potentials as an adult; he must not indicate that he merely wants a convenient place in which to go through withdrawal distress so that he can be rid of his habit for a short time because he has lost his connection, or for some other reason. He must be willing to give up all ambitions, desires, and social interactions that might prevent the group from assimilating him completely.

If he says he just wants to kick, he's no good. Out with him. Now we know nine out of ten lie, but we don't care. We'd rather have him make an attempt and *lie* and then get him in here for thirty days or so—then he might stick. It takes months to decide to stay.

Most fish [newcomers] don't take us serious-

[10] Alfred R. Lindesmith, *Opiate Addiction* (Bloomington: Principia Press, 1947), pp. 44–66.

ly. We know what they want, out in front. A dope fiend wants dope, nothing else. All the rest is garbage. We've even taken that ugly thing called money. This shows that they're serious. Now this guy today was sincere. We told him we didn't want money. We could see he would at least give the place a try. We have to find out if he's sincere. Is he willing to have us cut off his curly locks? I imagine cutting his hair off makes him take us seriously. . . .

Although it is impossible to say whether Synanon's selective admission process inadvertently admits those addicts who are most amenable to change, no addict has been refused admission on the ground that his case is "hopeless" or "difficult" or that he is "unreachable." On the contrary, before coming to Synanon, twenty-nine of the fifty-two addicts had been on drugs for at least ten years. Two of these were addicted for over forty years, and had been in and out of institutions during that period. The average length of time on drugs for the fifty-two was eleven years, and 56 per cent reported less than one month as the longest period of time voluntarily free of drugs after addiction and prior to Synanon.

Indoctrination.—In the admission process, and throughout his residence, the addict discovers over and over again that the group to which he is submitting is antidrug, anticrime, and antialcohol. At least a dozen times a day he hears someone tell him that he can remain at Synanon only as long as he "stays clean," that is, stays away from crime, alcohol, and drugs. This emphasis is an unwitting implementation of Cressey's second principle:

The more relevant the common purpose of the group to the reformation of criminals, the greater will be its influence on the criminal members' attitudes and values. Just as a labor union exerts strong influence over its members' attitudes toward management but less influence on their attitudes toward say, Negroes, so a group organized for recreation or welfare purposes will have less success in influencing criminalistic attitudes and values than will one whose explicit purpose is to change criminals.

Indoctrination makes clear the notion that Synanon exists in order to keep addicts off drugs, not for purposes of recreation, vocational education, etc. Within a week after admission, each newcomer participates in an indoctrination session by a spontaneous group made up of four or five older members. Ordinarily, at least one member of the Board of Directors is present, and he acts as leader. The following are excerpts from one such session with a woman addict. The rules indicate the extreme extent to which it is necessary for the individual to subvert his personal desires and ambitions to the antidrug, anticrime group.

Remember,we told you not to go outside by yourself. Whenever anybody leaves this building they have to check in and out at the desk. For a while, stay in the living room. Don't take showers alone or even go to the bathroom alone, see. While you're kicking, somebody will be with you all the time. And stay away from newcomers. You got nothing to talk to them about, except street talk, and before you know it you'll be splitting [leaving] to take a fix together. Stay out of the streets, mentally and physically, or get lost now.

No phone calls or letters for a while—if you get one, you'll read it in front of us. We'll be monitoring all your phone calls for a while. You see, you got no ties, no business out there any more. You don't need them. You never could handle them before, so don't start thinking you can do it now. All you knew how to do was shoot dope and go to prison.

You could never take care of your daughter before. You didn't know how to be a mother. It's garbage. All a dope fiend knows how to do is shoot dope. Forget it.

There are two obvious illustrations of the antidrug and anticrime nature of the group's subculture. First, there is a strong taboo against what is called "street talk." Discussion of how it feels to take a fix, who one's connection was, where one took his shot, the crimes one has committed, or who one associated with is severely censured. One's best friend and confidant at Synanon might well be the person that administers a tongue lashing for street talk, and the person who calls your undesirable behavior to the attention of the entire group during a general meeting.

Second, a member must never, in any circumstances, identify with the "code of the streets," which says that a criminal is supposed to keep quiet about the criminal activities of his peers. Even calling an ordinary citizen "square" is likely to stimulate a spontaneous lecture, in heated and colorful terms, on the notion that the people who are *really* square are those that go around as bums sticking needles in their arms. A person who, as a criminal, learned to hate stool pigeons and finks with a passion must now turn even his closest friend over to the authorities, the older members of Synanon, if the friend shows any signs of nonconformity. If he should find that a member is considering "sneaking off to a fix somewhere," has kept pills, drugs, or an "outfit" with him when he joined the organization, or even has violated rules such as that prohibiting walking alone on the beach, he must by Synanon's code relinquish his emotional ties with the violator and expose the matter to another member or even to the total membership at a general meeting. If he does not do so, more pressure is put upon him than upon the violator, for he is expected to have "known better." Thus, for perhaps the first time in his life he will be censured for *not* "squealing" rather than for "squealing."[11] He must identify with the law and not with the criminal intent or act.

The sanctions enforcing this norm are severe, for its violation threatens the very existence of the group. "Guilt by association" is the rule. In several instances, during a general meeting the entire group spontaneously voted to "throw out" both a member who had used drugs and a member who had known of this use but had not informed the group. Banishment from the group is considered the worst possible punishment, for it is stressed over and over again that life in the streets "in your condition" can only mean imprisonment or death.

That the group's purpose is keeping addicts off drugs is given emphasis in formal and informal sessions—called "haircuts" or "pull ups"—as well as in spontaneous denunciations, and in denunciations at general meetings. The "synanon," discussed below, also serves this purpose. A "haircut" is a deliberately contrived device for minimizing the importance of the individual and maximizing the importance of the group, and for defining the group's basic purpose—keeping addicts off drugs and crime. The following is the response of a leader to the questions, "What's a haircut? What's its purpose?"

When you are pointing out what a guy is doing. We do this through mechanisms of exaggeration. We blow up an incident so he can really get a look at it. The Coordinators [a coordinator resembles an officer of the day] and the Board members and sometimes an old timer may sit in on it. We do this when we see a person's attitude becoming negative in some area.

For a *real* haircut, I'll give you myself. I was in a tender trap. My girl split. She called me on the job three days in a row. I made a date with her. We kept the date and I stayed out all night with her. Now, she was loaded [using drugs]. I neglected—or I refused—to call the house. By doing this I ranked everybody. You know doing something like that was no good. They were all concerned. They sent three or four autos looking for me because I didn't come back from work. You see, I was in Stage II.

X found me and he made me feel real lousy, because I knew he worked and was concerned. Here he was out looking for me and he had to get up in the morning.

Well, I called the house the next morning and came back. I got called in for a haircut.

I sat down with three Board members in the office. They stopped everything to give the haircut. That impressed me. Both Y and Z, they pointed out my absurd and ridiculous behavior by saying things like this—though I did not get loaded, I associated with a broad I was emotionally involved with who was using junk. I jeopardized my *own* existence by doing this. So they told me, "Well, you fool, you might as well have shot dope by associating with a using addict." I was given an ultimatum. If I called her again or got in touch with her I would be thrown out.

("Why?")

Because continued correspondence with a

[11] See Lewis Yablonsky, "The Anti-Criminal Society: Synanon," *Federal Probation*, XXVI (September, 1962), 50–57; and Lewis Yablonsky, *The Violent Gang* (New York: Macmillan Co., 1962), pp. 252–63.

using dope fiend is a crime against *me*—it hurts *me*. It was also pointed out how rank I was to people who are concerned with me. I didn't seem to care about people who were trying to help me. I'm inconsiderate to folks who've wiped my nose, fed me, clothed me. I'm like a child, I guess. I bite the hand that feeds me.

To top that off, I had to call a general meeting and I told everybody in the building what a jerk I was and I was sorry for acting like a little punk. I just sort of tore myself down. Told everyone what a phony I had been. And then the ridiculing questions began. Everybody started in. Like, "Where do you get off doing that to us?" That kind of stuff. When I was getting the treatment they asked me what I'd do —whether I would continue the relationship, whether I'd cut it off, or if I really wanted to stay at Synanon and do something about myself and my problem. But I made the decision before I even went in that I'd stay and cut the broad loose. I had enough time under my belt to know enough to make that decision before I even came back to the house. . . .

Group cohesion.—The daily program at Synanon is consistent with Cressey's third principle, and appears to be an unwitting attempt to implement that principle:

> *The more cohesive the group, the greater the members' readiness to influence others and the more relevant the problem of conformity to group norms. The criminals who are to be reformed and the persons expected to effect the change must, then, have a strong sense of belonging to one group: between them there must be a genuine "we" feeling. The reformers, consequently, should not be identifiable as correctional workers, probation or parole officers, or social workers.*

Cohesion is maximized by a "family" analogy and by the fact that all but some "third-stage" members live and work together. The daily program has been deliberately designed to throw members into continuous mutual activity. In addition to the free, unrestricted interaction in small groups called "synanons," the members meet as a group at least twice each day. After breakfast, someone is called upon to read the "Synanon Philosophy," which is a kind of declaration of principles, the day's work schedule is discussed, bits of gossip are publicly shared, the group or individual members are spontaneously praised or scolded by older members. Following a morning of work activities, members meet in the dining room after lunch to discuss some concept or quotation that has been written on a blackboard. Stress is on participation and expression; quotations are selected by Board members to provoke controversy and examination of the meaning, or lack of meaning, of words. Discussion sometimes continues informally during the afternoon work period and in "synanons," which are held after dinner (see below). In addition, lectures and classes, conducted by any member or outside speaker who will take on the responsibility, are held several times a week for all members who feel a need for them. Topics have included "semantics," "group dynamics," "meaning of truth," and "Oedipus complex."

There are weekend recreational activities, and holidays, wedding anniversaries, and birthdays are celebrated. Each member is urged: "Be yourself," "Speak the truth," "Be honest," and this kind of action in an atmosphere that is informal and open quickly gives participants a strong sense of "belonging." Since many of the members have been homeless drifters, it is not surprising to hear frequent repetition of some comment to the effect that "This is the first home I ever had."

Also of direct relevance to the third principle is the *voluntary* character of Synanon. Any member can walk out at any time; at night the doors are locked against persons who might want to enter, but not against persons who might want to leave. Many do leave.

Holding addicts in the house once they have been allowed to enter is a strong appeal to ideas such as "We have all been in the shape you are now in," or "Mike was on heroin for twenty years and *he's* off." It is significant, in this connection, that addicts who "kick" (go through withdrawal distress) at Synanon universally report that the sickness is not as severe as it is in involuntary organizations, such as jails and

mental hospitals. One important variable here, we believe, is the practice of not giving "kicking dope fiends" special quarters. A newcomer kicks on a davenport in the center of the large living room, not in a special isolation room or quarantine room. Life goes on around him. Although a member will be assigned to watch him, he soon learns that his sickness is not important to men and women who have themselves kicked the habit. In the living room, one or two couples might be dancing, five or six people may be arguing, a man may be practicing the guitar, and a girl may be ironing. The kicking addict learns his lesson: These others have made it. This subtle device is supplemented by explicit comments from various members as they walk by or as they drop in to chat with him. We have heard the following comments, and many similar ones, made to new addicts lying sick from withdrawal. It should be noted that none of the comments could reasonably have been made by a rehabilitation official or a professional therapist.

"It's OK boy. We've all been through it before."

"For once you're with people like us. You've got everything to gain here and nothing to lose."

"You think you're tough. Listen, we've got guys in here who could run circles around you, so quit your bull——."

"You're one of us now, so keep your eyes open, your mouth shut and try to listen for a while. Maybe you'll learn a few things."

"Hang tough, baby. We won't let you die."

Status ascription.—Cressey's fourth principle is:

> *Both reformers and those to be reformed must achieve status within the group by exhibition of "pro-reform" or anti-criminal values and behavior patterns. As a novitiate . . . he is a therapeutic parasite and not actually a member until he accepts the group's own system for assigning status.*

This is the crucial point in Cressey's formula, and it is on this point that Synanon seems most effective. The house has an explicit program for distributing status symbols to members in return for staying off the drug and, later, for actually displaying antidrug attitudes. The resident, no longer restricted to the status of "inmate" or "patient" as in a prison or hospital, can achieve any staff position in the status hierarchy.

The Synanon experience is organized into a career of roles that represent stages of graded competence, at whose end are roles that might later be used in the broader community. Figure 1 shows the status system in terms of occupational roles, each box signifying a stratum. Such cliques as exist at Synanon tend to be among persons of the same stratum. Significantly, obtaining jobs of increased responsibility and status is almost completely dependent upon one's attitudes toward crime and the use of drugs. To obtain a job such as Senior Coordinator, for example, the member must have demonstrated that he can remain free of drugs, crime, and alcohol for at least three to six months. Equally important, he must show that he can function without drugs in situations where he might have used drugs before he came to Synanon. Since he is believed to have avoided positions of responsibility by taking drugs, he must gradually take on positions of responsibility without the use of drugs. Thus, he cannot go up the status ladder unless his "attitudes" are right, no matter what degree of skill he might have as a workman. Evaluation is rather casual, but it is evaluation nevertheless—he will not be given a decent job in the organization unless he relinquishes the role of the "con artist" and answers questions honestly, expresses emotions freely, co-operates in group activities, and demonstrates leadership. In a letter to a public official in May, 1960, the founder explained the system as follows:

> Continued residence [at Synanon], which we feel to be necessary to work out the problem of interpersonal relationships which underlie the addiction symptom is based on adherence by the individual to standards of behavior, thinking, and feeling acceptable to our culture. There is much work to be done here, as we have no paid help, and each person must assume his share of the burden. Increased levels of responsibility are sought and the experience of self-satisfaction
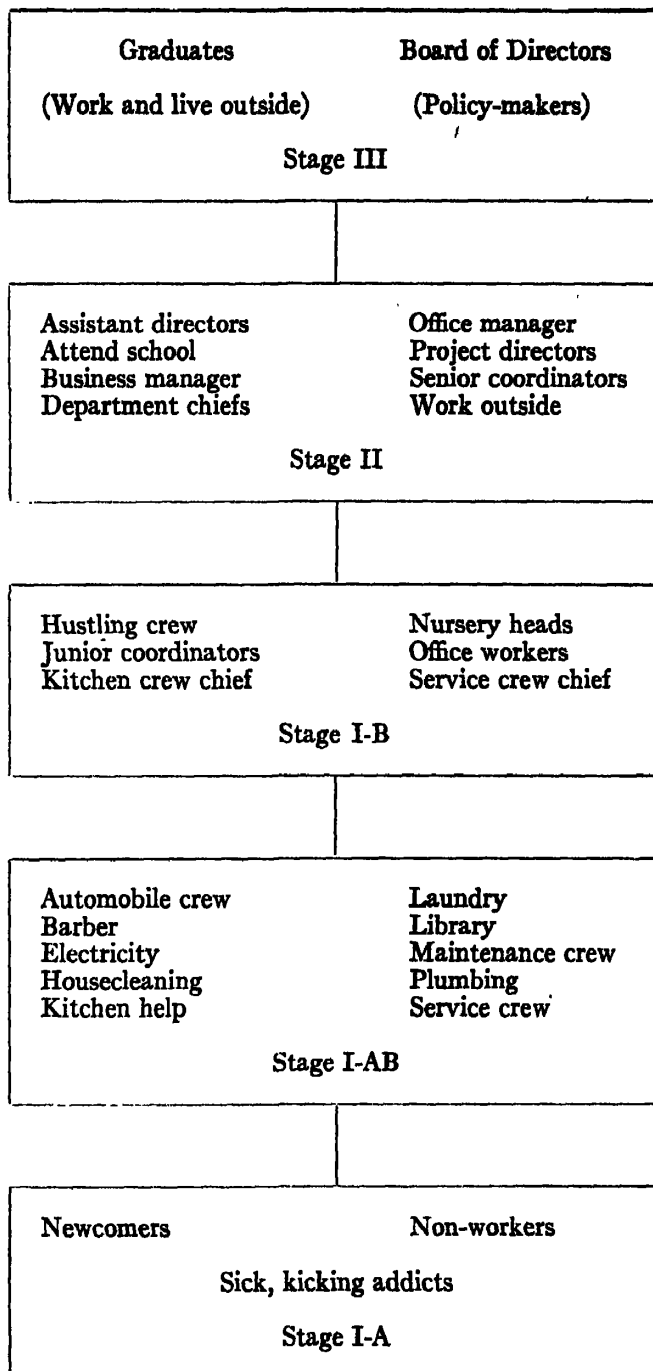
FIG. 1.—Division of labor and stratification system, Synanon, June, 1962.

comes with seeking and assuming these higher levels and seems to be an extremely important part of emotional growth.[13]

An analogy with a family and the development of a child also is used. Officially, every member is expected to go through three "stages of growth," indicated by Roman numerals in Figure 1. Stage I has two phases, "infancy" and "adolescence." In the "infancy" phase (I-A) the member behaves like an infant and is treated as one; as he kicks the habit "cold turkey" (without the aid of drugs) in the living room, he is dependent on the others, and he is supervised and watched at all times. When he is physically and mentally able, he performs menial tasks such as dishwashing and sweeping in a kind of "preadolescent" stage (I-AB) and then takes on more responsible positions (I-B). In this "adolescence" phase he takes on responsibility for maintenance work, participates actively in group meetings, demonstrates a concern for "emotional growth," mingles with newcomers and visitors, and accepts responsibilities for dealing with them. In work activities, for example, he might drive the group's delivery truck alone, watch over a sick addict, supervise the dishwashing or cleanup crews, or meet strangers at the door.

TABLE 5

LENGTH OF RESIDENCE AND "STAGE" OF MEMBERS, JUNE, 1962

Length of Residence (In Months)	Stages			No.	Per Cent
	I	II	III		
1- 3........	20	0	0	20	19
4- 6........	15	0	0	15	14
7- 9........	7	3	0	10	9
10-12........	2	0	0	2	2
13-15........	3	4	0	7	7
16-18........	3	0	2	5	5
19-21........	4	1	0	5	5
22-24........	0	4	1	5	5
25 and over...	0	12	24	36	34
Total.......	54	24	27	105	100

Stage II is called the "young adult stage." Here, the member is in a position to choose between making Synanon a "career," attending school, or going to work at least part time. If he works for Synanon, his position is complex and involves enforcing policy over a wide range of members. In Stage III, "adult," he moves up to a policy-making position in the Board of Directors or moves out of Synanon but returns with his friends and family for occasional visits. He can apparently resist the urge to resort to drugs in times of crisis without the direct help of Synanon members. One man described this stage by saying, "They go out, get jobs, lose jobs, get married, get divorced, get married again, just like everyone else." However, the group does maintain a degree of control. Graduates are never supposed to cut off their ties with their Synanon "family," and they are expected to return frequently to display themselves as "a dope fiend made good."

From Table 5 it is apparent that seniority in the form of length of residence (equivalent to the number of "clean" days) is an important determinant of status. As time of residence increases, responsibilities to the group, in the forms of work and leadership, tend to increase. In June, 1962, twenty-seven of the 105 members of Synanon were in Stage III. It should be noted that while stage is associated with length of residence, advancement through the stages is not automatic. The longer one lives at Synanon, the "cleaner" he is, the more diffuse the roles he performs, and the higher his status.

It is also important to note that high status does not depend entirely upon one's conduct within the house. Before he graduates to Stage III a member must in some way be accorded an increase in status by the legitimate outside community. This is further insurance that status will be conferred for activities that are antidrug in character. In early 1960, the members began to take an active part in legitimate community activities, mostly in the form of lectures and discussion groups. Since Synanon's inception, more than 350 service groups, church groups, political groups, school and college classes,

[13] See Volkman, *op. cit.*, pp. 90–96.

etc., have been addressed by speakers from Synanon. Such speeches and discussions gain community support for the organization, but they further function to give members a feeling of being important enough to be honored by an invitation to speak before community groups. Similarly, members are proud of those individuals who have "made good" in the outside community by becoming board members of the P.T.A., Sunday-school teachers, college students, and members of civic and service organizations. Over thirty-five Synanon members are now working full or part time in the community, holding a wide range of unskilled (janitor, parking attendant), skilled (truck driver, carpenter, electrician), white-collar (secretary, photographer), and executive (purchasing agent) posts.

Further, the legitimate status of the *group* has increasingly risen during the last two years. Since the summer of 1960, an average of 100–150 guests have attended open-house meetings, and the guests have included distinguished persons from all walks of legitimate life. Well-known psychiatrists, correctional workers, businessmen, newspapermen, and politicians have publicly praised the work of the group. There have been requests for Synanon houses and for Synanon groups from several communities, and Synanon projects are now being conducted at Terminal Island Federal Prison and the Nevada State Prison. Recently, the group has been featured in films, on television and radio shows, and in national magazines. At least two books and a movie are being written about it. Over five hundred citizens have formed an organization called "Sponsors of Synanon." Even strong attacks from some members of the local community and complicated legal battles about zoning ordinances have served principally to unite the group and maximize the *esprit de corps*.

The "synanon."—Synanon got its name from an addict who was trying to say "seminar." The term "Synanon" is used to refer to the entire organization, but when it is spelled with a lower-case *s* it refers only to the meetings occurring in the evenings among small groups of six to ten members. Each evening, all members are assigned to such groups, and membership in the groups is rotated so that one does not regularly interact with the same six or ten persons. The announced aim of these meetings is to "trigger feelings" and to allow what some members refer to as "a catharsis." The sessions are not "group therapy" in the usual sense, for no trained therapist is present. Moreover, the emphasis is on enforcing anticriminal and antidrug norms, as well as upon emotional adjustment.[18] These sessions, like the entire program, constitute a system for implementing Cressey's fifth principle, although they were not designed to do so.

> *The most effective mechanism for exerting group pressure on members will be found in groups so organized that criminals are induced to join with noncriminals for the purpose of changing other criminals. A group in which criminal A joins with some noncriminals to change criminal B is probably most effective in changing criminal A, not B; in order to change criminal B, criminal A must necessarily share the values of the anticriminal members.*

In the house, the behavior of all members is visible to all others. What a member is seen to do at the breakfast table, for example, might well be scrutinized and discussed at his synanon that evening. The synanon sessions differ from everyday honesty by virtue of the fact that in these discussions one is expected to *insist on* the truth as well as to tell the truth. Any weapon, such as ridicule, cross-examination, or hostile attack, is both permissible and expected. The sessions seem to provide an atmosphere of truth-seeking that is reflected in the rest of the social life within the household so that a simple question like "How are you?" is likely to be answered by a five-minute discourse in which the respondent searches for the truth. The following discussion is from a tape recording of a synanon session held in June, 1961. It should be noted that an "innocent" question about appearance, asked by an older member who has become a non-criminal and a

[18] See Cressey, "Contradictory Theories in Correctional Group Therapy Programs," *op. cit.*

non-addict, led to an opportunity to emphasize the importance of loyalty to the antidrug, anticrime group.

"What are you doing about losing weight?"

"Why? Is that your business?"

"I asked you a question."

"I don't intend to answer it. It's not your business."

"Why do you want to lose weight?"

"I don't intend to answer it."

"Why?"

"Because it's an irrelevant and meaningless question. You know I had a baby only three weeks ago, and you've been attacking me about my weight. It's none of your business."

"Why did you call your doctor?"

"Why? Because I'm on a diet."

"What did he prescribe for you?"

"I don't know. I didn't ask him."

"What did you ask for?"

"I didn't. I don't know what he gave me."

"Come on now. What kind of pills are they?"

"I don't know. I'm not a chemist. Look the doctor knows I'm an addict. He knows I live at Synanon. He knows a whole lot about me."

"Yeah, well, I heard you also talking to him on the phone, and you sounded just like any other addict trying to cop a doctor out of pills."

"You're a goddamned liar!"

"Yeah, well X was sitting right there. Look, does the doctor know and does the Board know?"

"I spoke to Y [Board member]. It's all been verified."

"What did Y say?"

"I was talking to . . ."

"What did Y say?"

"Well, will you wait just a minute?"

"What did Y say?"

"Well, let her talk."

"I don't want to hear no stories."

"I'm not telling stories."

"What did Y say?"

"That it was harmless. The doctor said he'd give me nothing that would affect me. There's nothing in it. He knows it all. I told Y."

"Oh, you're all like a pack of wolves. You don't need to yell and scream at her."

"Look, I heard her on the phone and the way she talked she was trying to manipulate the doctor."

"Do you resent the fact that she's still acting like a dope fiend and she still sounds like she's conning the doctor out of something? She's a dope fiend. Maybe she can't talk to a doctor any differently."

"Look, I called the doctor today. He said I should call him if I need him. He gave me vitamins and lots of other things."

"Now wait a minute. You called to find out if you could get some more pills."

"Besides, it's the attitude they heard over the phone. That's the main thing."

"Yeah, well they probably projected it onto me."

"Then now come you don't like anyone listening to your phone calls?"

"Are you feeling guilty?"

"Who said?"

"Me. That's who. You even got sore when you found out X and me heard you on the phone, didn't you? You didn't like that at all, did you?"

"Is that so?"

(*Silence.*)

"I don't think her old man wants her back."

"Well, who would? An old fat slob like that."

"Sure, that's probably why she's thinking of leaving all the time and ordering pills."

"Sure."

(*Silence.*)

"My appearance is none of your business."

"Everything here is our business."

"Look, when a woman has a baby you can't understand she can't go back to normal weight in a day."

"Now *you* look. We're really not interested in your weight problem now. Not really. We just want to know why you've got to have pills to solve the problem. We're going to talk about that if we want to. That's what we're here for."

"Look, something's bugging you. We all know that. I even noticed it in your attitude toward me."

"Yeah, I don't care about those pills. I want to know how you're feeling. What's behind all this? Something's wrong. What is it?"

(*Silence.*)

"Have you asked your old man if you could come home yet?"

(*Softly.*) "Yes."

"What did he say?"

(*Softly.*) "He asked me how I felt. Wanted to know why I felt I was ready to come home. . . ."

(*Silence.*)

(*Softly.*) "I did it out of anger. I wasn't very happy. (*Pause.*) A day before I tried [telephoning him] and he wasn't there. (*Pause.*) Just this funny feeling about my husband being there and

me here. My other kid's there and this one's here. (*Pause.*) A mixed-up family."

"Why do you want to stay then? Do you want to be here?"

"No. I don't want to be here. That's exactly why I'm staying. I need to stay till I'm ready."

"Look, you've got to cut them loose for a while. You may not be ready for the rest of your life. You may not ever be able to be with those people."

(*Tears.*)

"I know. . . ."

After the synanon sessions, the house is always noisy and lively. We have seen members sulk, cry, shout, and threaten to leave the group as a result of conversation in the synanon. The following comments, every one of which represents the expression of a pro-reform attitude by the speaker, were heard after one session. It is our hypothesis that such expressions are the important ones, for they indicate that the speaker has become a reformer and, thus, is reinforcing his own pro-reform attitudes every time he tries to comfort or reform another.

"Were they hard on you?"

"I really let him have it tonight."

"I couldn't get to her. She's so damned blocked she couldn't even hear what I was trying to tell her."

"Hang tough, man; it gets easier."

"One of these days he'll drop those defenses of his and start getting honest."

"Don't leave. We all love you and want you to get well."

At Synanon, disassociating with former friends, avoiding street talk, and becoming disloyal to criminals are emphasized at the same time that loyalty to non-criminals, telling the truth to authority figures, and legitimate work are stressed. We have no direct evidence that haircuts, synanons, and both formal and spontaneous denunciations of street talk and the code of the streets have important rehabilitative effects on the actor, as well as (or, perhaps even "rather than") on the victim. It seems rather apparent, however, that an individual's own behavior must be dramatically influenced when he acts in the role of a moral policeman and "takes apart" another member. It is significant that older members of Synanon like to point out that the "real Synanon" began on "the night of the big cop out" (confession). In its earliest days, Synanon had neither the group cohesiveness nor the degree of control it now has. Some participants remained as addicts while proclaiming their loyalty to the principle of antiaddiction, and other participants knew of this condition. One evening in a general meeting a man spontaneously stood up and confessed ("copped out") that he had sneaked out for a shot. One by one, with no prompting, the others present rose to confess either their own violations or their knowledge of the violations of their friends. From that moment, the Board of Directors believe, the organization became a truly antidrug group; there has been no problem of drug use since.

THE RESULTS

Of the fifty-two residents described earlier, four are "graduates" of Synanon, are living in the community, and are not using alcohol or drugs. Twenty-three (44.2 per cent) are still in residence and are not using alcohol or drugs. Two of these are on the Board of Directors and eleven are working part or full time. The remaining twenty-five left Synanon against the advice of the Board and the older members.

Information regarding the longest period of voluntary abstinence from drugs after the onset of addiction but prior to entering Synanon was obtained on forty-eight of the fifty-two persons. Eleven reported that they were "never" clean, six said they were continuously clean for less than one week, ten were continuously clean for less than one month. Thirty-nine (81 per cent) said they had been continuously clean for less than six months, and only two had been clean for as long as a one-year period. Twenty-seven (52 per cent) of the fifty-two residents have now abstained for at least six months; twelve of these have been clean for at least two years and two have been off drugs continually for over three years.

Between May, 1958 (when Synanon

started), and May, 1961, 263 persons were admitted or readmitted to Synanon. Of these, 190 (72 per cent) left Synanon against the advice of the Board of Directors and the older members. Significantly, 59 per cent of all dropouts occurred within the first month of residence, 90 per cent within the first three months. Synanon is not adverse to giving a person a second chance, or even a third or fourth chance: of the 190 persons dropping out, eighty-three (44 per cent) were persons who had been readmitted. The dropout behavior of persons who were readmitted was, in general, similar to first admissions; 64 per cent of their dropouts occurred within the first month, 93 per cent within the first three months after readmission.

Of all the Synanon enrolees up to August, 1962, 108 out of 372 (29 per cent) are known to be off drugs. More significantly, of the 215 persons who have remained at Synanon for at least one month, 103 (48 per cent) are still off drugs; of the 143 who have remained for at least three months, 95 (66 per cent) are still non-users; of the 87 who have remained at least seven months, 75 (86 per cent) are non-users. These statistics seem to us to be most relevant, for they indicate that once an addict actually becomes a member of the antidrug community (as indicated by three to six months of participation), the probability that he will leave and revert to the use of drugs is low.

CONCLUSIONS

Synanon's leaders do not claim to "cure" drug addicts. They are prone to measure success by pointing to the fact that the organization now includes the membership of forty-five persons who were heroin addicts for at least ten years. Two of these were addicted for more than thirty years and spent those thirty years going in and out of prisons, jails, the U.S. Public Service Hospital, and similar institutions. The leaders have rather inadvertently used a theory of rehabilitation that implies that it is as ridiculous to try to "cure" a man of drug addiction as it is to try to "cure" him of sexual intercourse. A man can be helped to stay away from drugs, however, and this seems to be the contribution Synanon is making. In this regard, its "success" rate is higher than that of those institutions officially designated by society as places for the confinement and "reform" of drug addicts. Such a comparison is not fair, however, both because it is not known whether the subjects in Synanon are comparable to those confined in institutions, and because many official institutions do not concentrate on trying to keep addicts off drugs, being content to withdraw the drug, build up the addicts physically, strengthen vocational skills, and eliminate gaps in educational backgrounds.[14]

We cannot be certain that it is the group relationships at Synanon, rather than something else, that is keeping addicts away from crime and drugs. However, both the times at which dropouts occur and the increasing antidrug attitudes displayed with increasing length of residence tend to substantiate Sutherland's theory of differential association and Cressey's notion that modifying social relationships is an effective supplement to the clinical handling of convicted criminals. Drug addiction is, in fact, a severe test of Sutherland's sociological theory and Cressey's sociological principles, for addicts have the double problem of criminality and the drug habit. The statistics on dropouts suggest that the group relations method of rehabilitation does not begin to have its effects until newcomers are truly integrated into the antidrug, anticrime group that is Synanon.

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[14] Cf. Harrison M. Trice, "Alcholism: Group Factors in Etiology and Therapy," *Human Organization*, XV (Summer, 1956), 33–40 (see also Donald R. Cressey, "The Nature and Effectiveness of Correctional Techniques," *Law and Contemporary Problems*, XXIII [Fall, 1958], 754–71).

PARENTAL AUTHORITY AND JOB CHOICE: SEX DIFFERENCES IN THREE CULTURES[1]

ROBERT J. SMITH, CHARLES E. RAMSEY, AND GELIA CASTILLO

ABSTRACT

A study of the codes of conduct of high-school seniors in defying or yielding to parental authority in choosing an occupation was conducted in Japan, the Philippines, and the United States. Respondents in all three cultures allow greater freedom from parental control for young men than young women. This double standard is accepted by both sexes. Female respondents allowed more freedom to both sexes than did male respondents. With respect to the double standard, the differential is greatest in Japan, next in the Philippines, and least in the United States. Responses concerning only young women were least permissive in Japan, next in the Philippines, and most permissive in the United States. In responses concerning young men, the Japanese were considerably more permissive than the Filipino students, but less permissive than Americans.

An important concomitant of the process of industrialization is a decline in the authority of the family over its younger members. Children come increasingly to make decisions for themselves with less and less reference to family considerations.[2] However, the attitudes of the young toward the legitimacy of the exercise of parental authority are by no means clear-cut, and many studies report a marked ambivalence on the subject.[3] Too, there appear to be important sex differences; parents seem to yield more readily to male offsprings than to females. Also, males appear to anticipate greater freedom of choice than do females in instances involving parental consent to a decision.[4]

Relating as it does to the industrial context, the analysis of attitudes toward parental influences in occupational choice by both boys and girls in the United States, Japan, and the Philippines affords an opportunity to answer some important questions about parent-child relations and sex differences cross-culturally: (1) To what extent is there a double standard, applied differentially to young men and young women, in according them freedom of choice of an occupation? (2) If a double standard exists, to what extent is it accepted by both sexes? (3) What cross-cultural differences are found with respect to the double stand-

[1] Without the assistance of Toichiro Koitabashi and Kazuko S. Smith, the Japanese phase of the study could never have been carried out. The assistance of Perla O. Tagumpay and the administrators of each of the six high schools was indispensable in obtaining the Philippine data. The study was supported in part by a grant from the National Institutes of Health, Public Health Service, Department of Health, Education, and Welfare, and by the following sources at Cornell University: The Social Science Research Center and the Faculty Research Grants Committee.

[2] Marion J. Levy, Jr., *The Family Revolution in Modern China* (Cambridge, Mass.: Harvard University Press, 1949). For a contrary view of the Japanese situation see Ezra F. Vogel, "The Democratization of Family Relations in Japanese Urban Society," *Asian Survey*, I, No. 4 (1961), 18–24. Vogel finds very little sign of adolescent rebellion and no major differences of attitude between children and their parents.

[3] For a concise summary see L. Joseph Stone and Joseph Church, *Childhood and Adolescence* (New York: Random House, 1957), esp. pp. 275–81.

[4] There are conflicting theories concerning the extent to which both sexes accept this differential: see Talcott Parsons, "Age and Sex in the Social Structure of the United States," *American Sociological Review*, VII (October, 1942), 604–16; Takashi Koyama, *The Changing Social Position of Women in Japan* (Paris: UNESCO, 1961), pp. 15–32; and Chester Hunt *et al.*, *Sociology in the Philippine Setting* (1954), pp. 71–73. The evidence is unclear, which probably results from acceptance of inferior status in some areas of life and not from others: see e.g., Ronald P. Dore, *City Life in Japan* (London: Routledge & Kegan Paul, 1958), chap. xi; Koyama, *op. cit.*, p. 50, p. 63, and esp. p. 73 (Table 28).

ard and with respect to the consensus regarding it?

The ideology of individualism and the subordinate status of women in the United States is well documented in the literature.[5] A brief summary of the situation in Japan and the Philippines will give further orientation to the problem.

In Japan, marked differentiation traditionally obtained between male and female roles.[6] The Confucian doctrine exhorts the daughter to be obedient to her father, the wife to her husband, and the mother to her adult son. A woman is expected to give up any occupational aspirations when she is married. How females accept their subordinate status is not known, but impressionistic evidence indicates preference for a husband who is "manly," a cluster of traits that includes an assured male confidence of his superiority.

In the Philippines, subordination of the female is apparently neither as great as in Japan nor as little as in the United States.[7] Before the coming of the Spanish, a woman had rights approximately equal to a man, but during the colonial period she was not allowed to hold property in her own name nor to transact business without prior consent of her husband. Today, the new Civil Code restores much of the earlier equality enjoyed by women.

Regarding the obligation of youth, both adolescent males and females are still expected to defer to older family members on important decisions. Of even greater importance is the priority of age over sex categories; an older sister wields authority over her younger brothers.

Although changes presumably have occurred in all three cultures, the hypotheses are formulated on the basis of the traditional models.

RESEARCH PROCEDURES

The universe of study consists of high-school seniors in Japan, the Philippines, and the United States. In all three countries, the sample includes at least one high school from the largest urban center and one from a small city near the urban center. The distribution of cases was as follows: Tokyo, 178; New York City, 194; Manila, 223; Japanese small city, 358; American small city, 199; and Filipino small city, 230.

The distribution and sexual differentiation of responses were compared on the basis of size of city, occupation of father, educational attainment of each parent, as well as by individual school within a culture. Insofar as no significant differences were obtained, these factors were not controlled in the analysis presented below.

The use of a questionnaire in two languages raises the question of the common meaning of items, Since there are no checks of validity in each culture, there is no test

[5] Parsons, *op. cit.*; W. F. Ogburn and M. F. Nimkoff, *Technology and the Changing Family* (New York: Houghton Mifflin Co., 1955), chap. viii; Margaret Park Redfield, "The American Family: Consensus and Freedom," *American Journal of Sociology*, LII (April, 1946), 175–83; Ernest W. Burgess, "The Family in a Changing Society," *American Journal of Sociology*, LIII (May, 1948), 417–23; Mirra Komarovsky, "Cultural Contradictions and Sex Roles," *American Journal of Sociology*, LII (April, 1946), 184–89; and Robert F. Winch, *The Modern Family* (New York: Henry Holt & Co., 1952), pp. 291–96.

[6] See John W. Bennett, Herbert Passin, and Robert K. McKnight, *In Search of Identity: The Japanese Overseas Scholar in America and Japan* (Minneapolis: University of Minnesota Press, 1958), pp. 154–76; Ruth Benedict, *The Chrysanthemum and the Sword* (Boston: Houghton Mifflin Co., 1946), pp. 253–96; *The Status of Women in Postwar Japan* (Tokyo: Women's and Minors' Bureau, Ministry of Labor, 1956); and Koyama, *op. cit.*

[7] *Area Handbook on the Philippines*, Vol. I (New Haven, Conn.: Human Relations Area Files, 1956), pp. 415–22; Gerald D. Berreman, *The Philippines: A Survey of Current Social, Economic, and Political Conditions* (Data Paper No. 19 [Ithaca, New York: S.E. Asia Program, Department of Far Eastern Studies, Cornell University, April, 1956]), pp. 13–15; Hunt *et al.*, *op. cit.*; Serafin Macaraig *et al.*, *Philippine Social Life* (Manila: Macaraig Publishing Co., 1954), p. 326; Maria Paz Mendoza-Guazon, *The Development and Progress of the Filipino Woman* (Manila: Kiko Printing Press, 1951), p. 72; Pura Santillan-Castrence, *Talking Things Over with the Growing Filipina* (a project of the Philippine Association of University Women [Manila: Bardanon Book Co., 1951]), p. 192.

of common meaning from one culture to another. It can only be said that in each language the questions have face validity and that the translation was done with all possible care.

The dependent variable in the present study was obtained from the following questions:

Now we are going to ask you to put yourself in someone else's place.

A *young man* has been offered a job which he wants to take, but of which his parents disapprove. What do you think he should do? (Check the answer which you think is right.)

——a. Refuse to take the job under any circumstances.
——b. Try to convince his parents, but refuse the job if he cannot change their opinion.
——c. Take the job no matter how strong his parents' objections are, if he feels he has a good reason to do so.

Now suppose that the person involved in the situation above is a *young woman*. What do you think she should do? (Check the answer which you think is right.)

——a. Refuse to take the job under any circumstances.
——b. Try to convince her parents, but refuse the job if she cannot change their opinion.
——c. Take the job no matter how strong her parents' objections are, if she feels she has a good reason to do so.

ANALYSIS

Hypothesis 1: Codes of conduct in defying or yielding to parental authority in choosing an occupation differ for the sexes, allowing greater freedom from parental control for males than for females. The data clearly bear out the hypothesis. The null hypothesis that the uncorrelated answers do not depart from a 1:1 ratio is rejected at the 0.001 level of significance in each of the three cultures (Table 1). Further, the direction of the difference is as hypothesized.[8]

Approximately two-thirds of the Japanese boys feel that young men should ignore parents' wishes in occupational choice if they feel they have reason to do so, while less than one-third feel that girls should exercise the same right. Approximately the same proportions obtain, in reverse order, for the response "Try to convince the parents, but refuse the job if he cannot change their opinion." That is, over two-thirds of the combined responses of boys and girls feel that this behavior is required of young women. Extremely few boys or girls feel that either young men or women should simply refuse the job if the parents object. However, even these small percentages show greater restriction for women than for men.

In the American case, slightly more than three-fourths of the respondents feel that young men should take the job if they have good reason to do so. The American respondents, like the Japanese, hold considerably more restrictive attitudes toward young women. However, unlike the Japanese, the modal response concerning a young woman's reaction to parental objection is that she should take the job anyway if she feels she has good reason to do so. Nevertheless, the mode for young men is much larger than that for young women, and the American data thus support the first hypothesis.

The first hypothesis is also corroborated

[8] In testing the hypotheses of a double standard within each culture, the application of the usual χ^2 test of significance of difference to the distributions of responses concerning young women and young men is questionable. The reason for this is that the same persons answered both questions and therefore answers are correlated. The tests of differences with a culture therefore are based upon a comparison of those answers that are not correlated to see if they depart from a 1:1 ratio:

	Answers concerning Young Women	
	Restrictive	Permissive
Answers concerning young men:		
Restrictive.....	N 11	N 12
Permissive.....	N 21	N 22

$$\chi^2 = \frac{(n_{12} - n_{21})^2}{n_{12} + n_{21}}$$

See Robert G. D. Steel and James D. Torrie, *Principles and Procedures of Statistics* (New York: McGraw-Hill Book Co., 1960), p. 381.

by the Filipino data. The large modal response for young men faced with the problem is that they should take the job anyway, while an almost equally large mode for young women indicates that they should refuse the position if they cannot persuade their parents.

In terms of the first hypothesis, some cross-cultural uniformities may be noted. In all three cultures, attitudes are more restrictive of women than of men. The modal responses are large—numerical majorities, in fact—even though three response categories were available. For both the Japanese and the Filipino data, these modes are categorically restrictive of women. In the American case, however, the attitudes are only proportionately more restrictive of women, since the modal response for both males and females is in the category of greatest freedom. In all three cultures, only a very small percentage of high-school seniors felt that children should unhesitatingly obey their parents' wishes.

Hypothesis 2: The double standard is accepted by both sexes. This hypothesis is also borne out by the findings. In the Japanese case, what was true for the group as a whole is also true for each of the sexes, with only slight variation. For each sex, the null hypothesis is rejected at the 0.001 level (see Table 1). The large modal categories of responses by both boys and girls are that a young man should take a job over his parents' objections if he feels that he has good reasons to do so. The majority of each sex also expresses the opinion that a young woman should try to convince her parents, but refuse the job if she cannot persuade them. As in the case of the first hypothesis, the modes are numerical majorities.

TABLE 1

JUDGMENTS OF APPROPRIATE REACTION OF EACH SEX TO PARENTAL OBJECTIONS TO CHOICE OF OCCUPATION, BY SEX OF RESPONDENT

(Per Cent)

Responses	Japanese Boys' Responses		Japanese Girls' Responses		American Boys' Responses		American Girls' Responses		Filipino Boys' Responses		Filipino Girls' Responses	
	Concerning Young Men	Concerning Young Women	Concerning Young Men	Concerning Young Women	Concerning Young Men	Concerning Young Women	Concerning Young Men	Concerning Young Women	Concerning Young Men	Concerning Young Women	Concerning Young Men	Concerning Young Women
Refuse to take the job under any circumstances	1	3	1	1	1	5	0	1	3	7	2	3
Try to convince parents, but refuse the job if he cannot change their opinion	34	73	22	64	27	54	19	32	44	62	30	56
Take the job no matter how strong his parents' objections are, if he feels he had good reason to do so	65	24	77	35	72	41	81	67	53	31	68	41
Total per cent	100	100	100	100	100	100	100	100	100	100	100	100
Total N	(246)	(232)	(289)	(285)	(173)	(170)	(219)	(219)	(224)	(213)	(235)	(241)

In the American case, both boys and girls accord greater freedom from parental authority to males than to females. For each sex the null hypothesis that the uncorrelated answers do not depart from a 1:1 ratio is rejected at the 0.001 level (see Table 1). A slight majority (54 per cent) of the boys feel that a young woman should refuse the job after failing to convince her parents, a re-

sponse given by only one-third of the girls. However, girls give greater latitude to young men than to young women.

In the Filipino case, the responses again are consistent with the second hypothesis, but resemble the Japanese case more than the American in that all modal categories are those predicted by the hypothesis. For each sex, the null hypothesis is rejected at the 0.001 level (see Table 1). The modal category for both boys and girls concerning young men is to accord them the greatest degree of freedom allowed by the question, while the modal category for both boys and girls concerning young women is to say that they should refuse the job if unable to convince their parents. Again, all modes are numerical majorities.

Hypothesis 3: Any significant deviation from the above statement will be in the direction of assignment of greater freedom to one's own sex. In the Japanese case, just over one-third of the girls feel that a young woman should ignore her parents' wishes if she has a good reason, while slightly less than one-fourth of the boys gave this answer (Table 2).[9] Thus, the response regarding young women is that predicted by the third hypothesis. However, the girls are more permissive to a young man in this situation than are the boys, the converse of the prediction of the third hypothesis. Indeed, the difference in percentages between boys' and girls' responses is strikingly similar to that found with regard to acceptance of the double standard. The null hypothesis of no difference is rejected at the 0.01 level regarding the freedom of young men and at the 0.02 level regarding the freedom of young women.

American girls also accord more freedom to both sexes than do boys. However, the difference between their responses is greater in the freedom allowed young women than that allowed young men. The null hypothesis of no difference is rejected at the 0.05 level regarding the behavior expected of a young man and at the 0.001 level regarding the behavior expected of a young woman. Thus, the findings are more nearly those predicted by the third hypothesis than are the findings for the Japanese. Nevertheless, the third hypothesis states that any difference should be in the direction of assigning more freedom to one's own sex, and the data do not fully support the hypothesis. Boys accord less freedom to young men than do girls.

As in the other two cultures, the Filipinas accord themselves more freedom than do boys, but they also accord more freedom to young men than do boys. The null hypothesis of no difference between male and female

TABLE 2

SIGNIFICANCE OF DIFFERENCES IN COMPARISONS

	Attitude concerning Behavior of Young Men			Attitude concerning Behavior of Young Women		
	x^2	Degrees of Freedom	P	x^2	Degrees of Freedom	P
Difference in sex of respondent:						
United States....	5.97	2	.05	33.19	2	.001
Philippines.......	10.52	2	.01	6.48	2	.05
Japan...........	9.64	2	.01	8.43	2	.02
Cross-cultural comparisons:						
Males...........	20.27	4	.001	18.36	4	.01
Females.........	17.17	4	.01	61.50	4	.001

[9] All comparisons between the sex of respondents as well as cross-cultural comparisons are based upon an analysis summarized in Table 2.

respondents was rejected at the 0.01 level regarding the behavior of young men and at the 0.05 level regarding the behavior of young women.

It is clear that with respect to the third hypothesis the data in all three cultures must be interpreted with caution. It is not that girls accord more freedom to their own sex, but that they allow significantly more freedom to both sexes.

Hypothesis 4: With respect to freedom from parental restriction on the selection of an occupation, the attitudes of Youth in Japan are least permissive, of those in the Philippines next, and of those in the United States most. Regarding the behavior of young women, the Filipino respondents are somewhat more permissive than the Japanese, but considerably less so than the Americans. The null hypothesis of no significant cross-cultural difference is rejected at the 0.001 level. However, the data concerning young men do not fall in the order predicted by the hypothesis. The Japanese are only slightly less permissive for young men than are the Americans, and both are considerably more permissive than Filipino students. The null hypothesis of no significant cross-cultural differences is rejected at the 0.001 level. The same findings concerning both young men and young women also obtains when sex of respondent is controlled.

Hypothesis 5: With respect to the double standard in parental control over the choice of an occupation, the differential is greatest in Japan, next in the Philippines, and least in the United States. The hypothesis is borne out by the findings. The double standard is most evident in the Japanese data, least in the American. However, it should be noted that, within this difference, young men in both cultures are accorded about the same degree of freedom of decision, whereas young women in Japan are given very much less independence than are their American counterparts. Perhaps the differing degrees to which parental authority is exercised over young men and young women in Japan reflect the continuing influence of the traditionally dependent status of women in that country. The double standard is only slightly more evident in the Philippines than in the United States, but it is noteworthy that independence of choice is less frequently accorded both sexes.

There are several ways in which these differences could be expressed more precisely. One informative way would be to take into account both the differences and the degrees of permissiveness. In the American case, about one-fourth more respondents check the permissive response for young men than do so for young women. In the Philippines almost three-fourths again as many check the permissive response for young men. In Japan almost one and one-half times again as many check the permissive response for young men. The striking differences among the three cultures on this crude ratio are only slightly reduced when sex of respondent is controlled, and in no case are there reversals.

SUMMARY AND CONCLUSIONS

The findings may be summarized by a statement of the hypotheses that were confirmed or the revision of those not confirmed:

1. Codes of conduct in defying or yielding to parental authority in choosing an occupation differ for the sexes, allowing greater freedom from parental control for males than females.

2. The double standard is accepted by both sexes.

3. Female respondents allowed more freedom to both sexes in choosing an occupation against parents' wishes than did male respondents. (It was hypothesized that any significant deviation from the above statements will be in the direction of the assignment of greater freedom to one's own sex.)

The next two hypotheses predicted cross-cultural differences based on the traditional view of each of the three societies, the Philippines, Japan, and the United States:

4. With respect to freedom from parental restriction on the selection of an occupation by young women, the attitudes of youth in Japan are least permissive, in the Philippines next, and in the United States most. How-

ever, the data concerning young men do not fall in the order predicted by the traditional views of the culture; rather, Japanese are considerably more permissive of young men than are the Filipino students.

5. With respect to the double standard in parental control over the choice of an occupation, the differential is greatest in Japan, next in the Philippines, and least in the United States.

The postwar reforms affecting the position of women in Japanese society included enfranchisement, equal inheritance rights, coeducation, new divorce laws, and a whole emphasis on equal opportunities for women in education and business. All of this, coupled with the very strong stand taken for women's rights in the public press, films, and novels, and, most particularly, by high-school teachers had led us to expect more militancy on the part of girls, and less militancy on the part of boys. The fact that the Japanese still manifest greater sex-role differentiation may reflect, to some extent, the strong economic pressure on young people to remain at home after finishing school and through the early years of employment.

The same background of public pressure for equal rights has characterized the Philippines. However, there is little reflection of a greater liberation of young women from parental authority, although such liberation is somewhat more pronounced in the Philippines than in Japan. In the case of young men, the Philippines were the least permissive.

Perhaps the crucial point to pass in relations between parents and the young person is the acceptance of a job against parental wishes. An attempt to dissuade parents involves merely discussion, while the acceptance of a job over strong parental objections has long-range consequences and is, in a sense, a break in relations. We would expect the answers to questions in which the responses represent such different approaches to family life to be uniform in traditional cultures. However, the uniformity of response on this issue is less than might be expected from frequently found descriptions of Asian cultures. In fact, the amount of agreement is greater among the American students than among the students in either of the two cultures, with the single exception of the Japanese boys in responses concerning young women.

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INSTITUTIONS OF PRIVACY IN THE DETERMINATION OF POLICE ADMINISTRATIVE PRACTICE

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ABSTRACT

Legal institutions in modern societies define private and public places and define appropriate police activity differently for crimes occurring in private and public places. Since urban and rural areas differ in the distribution of private and public places, with denser public places in cities, police practice in urban and rural areas is different. And crimes of different types are systematically distributed; some, like murder, are almost entirely committed in private places, and others, such as traffic offenses, entirely in public. Statistics on arrest and conviction rates for different types of crime show the influence of the social location of the types of crime on the differential character of police practice. Too, the structural characteristics of subsections of police departments are derived from the social location of the type of crime the subsection deals with. Hypotheses on the effects of introducing new crimes into the responsibilities of different subsections of police departments are outlined for future research.

Legal institutions in general depend on rare events, such as arrests or civil court cases, to structure the field in which frequent events take place. This makes the operation of legal institutions very difficult to study, except when fairly comparable types of frequent events operate in strikingly different legally structured fields. The institutions of liberty generally, and of legally protected privacy in particular, have the characteristic that rare events, such as a case being thrown out of court because the law of search and seizure has been violated, structure the field in which the everyday activity of police is carried out. The many types of crimes that police deal with are very differently situated with respect to the legal institutions of privacy, as we shall try to demonstrate in the first part of this paper.

Certain statistics on the arrest and conviction rates for different types of crimes as well as pieces of common knowledge can be used to explore the different characteristics of police administrative practice in these different legally structured fields. We will then try to show that police administrative practice with respect to different types of crime varies strikingly and systematically with the relation of the crimes to the legal institutions of privacy. This paper then tries to conduct an empirical study of that kind of "structural effect" on which the regulatory power of the law depends, namely, an effect on the behavior of many of an action by the courts that specifically applies only to the action of a few.

The order of presentation will be as follows: first I shall outline some well-known characteristics of the legal institutions by which "private places" are defined, and the effects of the growth of large cities on the social structure of "public places." Then I shall outline the relation of certain types of crime to these legal institutions and show how the location of crimes with respect to private and public places affects the organized activities of the police in handling these crimes. Since the police "organization" is made up of organized activities with respect to crime, this is really an indirect approach to studying the effect of social structure on organizational structure. To show this more clearly, in the final part of the section I shall summarize the way that the socially determined activities of police for particular types of crime are organized into police subsections with different characteristics.

LEGAL RELATIONS OF POLICE POWER TO PRIVACY

Most of our daily life is lived in a number of small, bounded social systems, such as families, schools, factories, clubs, etc.,

that have their own norms, goals, and facilities. The maintenance of the boundaries of these systems is necessary to their free and autonomous development. If agents of the state or strange private citizens could enter these systems arbitrarily and interfere with interaction within them, they cannot develop freely.

The central practical boundaries are such mundane things as walls, doors, window shades, and locks. But in modern society few of these are made to withstand a concerted effort by a group of men to breach them (in contrast to feudal societies, for example). Yet these fragile doors and windows effectively prevent police or private citizens from interfering with our sleep, our classrooms, our toolbenches, or our bars, at least most of the time. This is because a door is a legal entity of great importance: legitimate concerted social efforts to break down a door may only take place on legally defined occasions. These occasions are defined in the law of arrest[1] and the law of search and seizure,[2] and therefore, derivatively, in the criminal law.

The legal defense of doors and walls and windows means that small social systems which have legal possession of a place can maintain *continuous, discretionary* control over who crosses their boundaries. And this discretion may be enforced against agents of the state unless they have legal cause to penetrate the system or are invited in. Whenever such continuous discretionary control is maintained, the law speaks of "private places." The legal existence of "private places," then, is the main source of the capacity of small social systems to maintain their boundaries and determine their own interaction without interference from the outside. The distinctive feature of a modern *liberal* state is that it uses the monopoly of violence (which all modern industrial states have) to guarantee the boundaries of small, autonomous social systems.

The central importance in our society of the private places created in this way is indicated by two facts. First, in Maryland, a state not much less free than others, a man entirely without access to private places is legally unfree:

> Every person, not insane, who wanders about in this state and lodges in market houses, market places, or in other public buildings [note that some of these "public buildings" are "private property"] or in barns, outhouses, barracks, or in the open air, without having any lawful occupation in the city, town, or county in which he may so wander, and without having any visible means of support, shall be deemed to be a tramp, and to be guilty of a misdemeanor, and shall be subject to imprisonment, at the discretion of the Court or Justice of the Peace hearing the charge, for a period of not less than thirty days nor more than one year. This section not to apply to Allegany County.[3]

That is, if a man is not a member of some organization or family or other group that has control over a "private place" (which may, of course, be "public property," as for instance a county hospital), then he has to *satisfy a policeman* that his occupation in the area is lawful, and has to make visible his means of support (except in Allegany County). Access to private places is itself sufficient evidence that a man has a legitimate relation to the social structure; without that evidence, special evidence of legitimate occupation has to be provided. "Occupation" here means any legitimate activity, not specifically a job.

The second fact indicating the importance of the legal definition of private places is that unless continuous discretionary control of access to a piece of property is maintained (creating a "private place"), police may freely enter and supervise interaction and arrest without a warrant:

> An officer in uniform or in citizen's clothes, may enter any public house, if open, as other

[1] A good summary of the law of arrest is R. M. Perkins, "The Law of Arrest," *Iowa Law Review*, XXV (1940), 201–89.

[2] See E. W. Machen, Jr., *The Law of Search and Seizure* (Chapel Hill: University of North Carolina Press, 1950).

[3] H. E. Flack (ed.), *The Annotated Code . . . of Maryland, 1951*. Art. 27, Sec. 666.

people enter, for the purpose of detecting or suppressing crime, and having peaceably entered, may arrest for any offense committed in his presence. [Apparently a common law rule, as cases are cited for authority rather than statutes.][4]

A man's affairs are never, then, legally free of police supervision except within private places. Police may not legally, of course, forbid actions in public places that are not prohibited by law. But there is a fundamental difference between conducting the affairs of a small social system in such a manner that no crimes committed shall *come to the attention* of the police, and conducting them so that a physically present policeman will approve. In the first case, the problem is to prevent complaints, perhaps by agreement; in the second, the problem is to satisfy the police, rather than other members of the system, that all is as it should be. Few of us ever see a policeman in those places where we spend most of our time; a "tramp" sees one wherever he goes, and the policeman has the discretionary power to "run him in."

DISTRIBUTION OF PRIVATE PLACES AND URBAN-RURAL POLICE PRACTICE

The concentration of the population into cities concentrates intensively used "public places" within a small geographical area, thus greatly reducing the amount of "public" area per person and making professional control of public places much more economical. At the same time the size and anonymity of the city decrease the chance of small social systems to control the behavior of their members in public. In a small village, activity in public places easily comes to the attention of the family, the priest, the employer, and the peers of the offender. Further, in large cities there are much stronger norms about "deliberately not noticing" the behavior of other people. This means that in cities, much more behavior is *only* inquired into by the police.

That is, in cities it is economically possible to patrol public places, and at the same time it is functionally necessary. City police can therefore depend much more on their own presence and information for the detection of crime (especially certain types of crime) than can a rural police force. To a large degree (except for the patrol of main highways) rural police depend on complaints from people who are injured or know of a crime rather than on their own patrol.

Besides leading to different structural conditions of police practice, intensively used public places pose new problems. The most important are, of course, traffic jams and accidents. But also, extensive traffic creates opportunities for the use of public places for private profit in ways that create a "nuisance." Soliciting for prostitution, begging, street vending, speech-making, all become profitable uses of public places when the traffic gets heavy enough. The control of these "nuisances" is easily done without access to private places, along with other patrol duties.

The increasing predominance of patrol of public places means that policemen act much more on their own initiative. Much or all of the evidence that justifies arrest will be collected by the policeman on the spot. The arrest often need not involve any invasion at all of private places. Consequently these arrests are much more likely than are those in rural areas to be arrests without a warrant, and therefore without prior check by the judiciary, or to be a direct summons to appear in court (as in traffic cases).

ARRESTS INVOLVING ENTRY INTO PRIVATE PLACES

Private autonomy alone cannot guarantee liberty in the sense in which we understand it today. Feudal manors or plantations in the ante bellum South were much more autonomous and free of state inter-

[4] *Instructions . . . and Digest of the Statutes, Ordinances and Decisions* (Baltimore: Baltimore Police Department, 1939), p. 13.

ference than modern factories and business places. But this private autonomy did not create liberty in the modern sense because the police power was privately appropriated and consequently not exercised according to "due process of law." The practical implication of this is that besides *not* entering small systems *except* on legally defined occasions, *entering* them *on* those occasions is a duty of the police in liberal societies. A primary function of the criminal law is the limitation of coercion within small social systems.

But once the small systems are entered by the state, "due process of law" means mainly a set of procedures which guarantee that the autonomy of individuals and small social systems will be restored as quickly as possible if a crime has not in fact been committed. And it means that the process of investigating and legally establishing the existence of a crime shall not so far damage the small social system that they cannot function after they have been found innocent.

Due process thus involves a grading of coercion applied by the police, into arrest and the seizure of evidence, coerced appearance in court, and coerced paying of the penalty of crime. Each of these grades of coercion changes the legal status of the presumed offender. Each of these changes of the legal condition of the presumed offender must be justified by evidence of a probability that a crime has been committed and probability that the defendant committed the crime. The probabilities increase with the increase in severity of coercion. The probability required to justify arrest (by a police officer at least) is not as high as that required to coerce an appearance in court (a "prima facie case"). To justify conviction the commission of a crime must be established "beyond reasonable doubt." We now have sufficient background to discuss the structural location of different types of crime with respect to the institutions of privacy and the effect of this structural distribution on police practice.

THE EFFECT OF THE SOCIAL LOCATION OF DIFFERENT CRIMES ON POLICE PRACTICE

It is immediately evident that different types of crime will be distributed differently with respect to the institutions of privacy. For instance, wife-beating rarely goes on in public places. Soliciting for prostitution requires some systematic contact with an anonymous public through a pimping apparatus. Except for call girls and prostitutes in well-known houses, this requires soliciting (often by the woman herself) in some public places. But prostitution takes place in private places. Burglary consists in the invasion of the private place of another, generally in secret, which results in a complaint against an unknown person. The person's identity has to be established by the police. Murder generally takes place within small social systems, behind doors. Riots take place in streets and other public places.

Of course these variations in the social location of crimes imply differences in police practice. Information relevant to each stage of the criminal process comes to the police in different ways, different degrees and kinds of coercion have to be applied for different purposes, different things have to be proved with the evidence. There are differences in the number and types of private places that have to be penetrated, and in the amount of preparation of the case previous to this penetration. The kinds of people who commit different types of crime have different stability of social ties, which makes due process work differently.[5] We may distinguish the following types of crime.

a) *Coercion in private life.*—The application of force or other coercion in private life is either controlled and adjusted within

[5] A statistical reflection of this could be obtained by computing the proportion of appeals to higher courts of convictions for those types of crime that indicate weak connection to the social structure, such as vagrancy, and comparing this with the rate of appeals for other types of crimes with approximately the same penalties.

the small social system (as when a wife puts up with a beating), or it is not. When it is not, the crimes are, generally speaking, "crimes of passion." Probably the legally defined crimes with the highest proportion originating in this way are incest and murder,[6] but some unknown proportion of other crimes against persons (rape, aggravated assault) originate the same way.

Such crimes generally either result in complaint to the police or in such heinous crimes that access to private places is hardly a difficulty, at least for investigation. Because the people who participate in small social systems are highly visible once the system is penetrated, and because often the complainant knows perfectly well who coerced whom, arrests are fairly easy to make. Crimes against persons generally have a high proportion of "crimes known to the police" that are "cleared by arrest" (see Table 1).

TABLE 1

PROPORTION OF CRIMES KNOWN TO THE POLICE "CLEARED BY ARREST," AND PROPORTION OF THOSE CHARGED WHO ARE CONVICTED, FOR VARIOUS CRIMES*

Crime	Percentage Cleared by Arrest	Percentage Convicted
Crimes indicating coercion in private life:		
Murder, including non-negligent manslaughter	92.7	59.4
Forcible rape	73.6	43.0
Aggravated assault	64.7	43.9
Crimes indicating coercion in public places:		
Robbery	42.5	64.8
Crimes indicating invasion of private places of another:		
Burglary	30.7	71.4
Larceny (over $50)	20.9	72.6
Auto theft	26.2	67.5
Crimes indicating individual public disorder:		
Vagrancy	N.a.	77.5
Drunkenness	N.a.	86.5
Disorderly conduct	N.a.	69.7

* Computed from Federal Bureau of Investigation, *Uniform Crime Reports-1959* (Washington, D.C.: Government Printing Office, 1960), Tables 12 and 14.

But the same conditions that produce easy arrests create another characteristic of enforcement against these crimes, namely, that arrests quite often do not result in conviction. Legal responsibility of the assailant must be established. His intentions are not immediately obvious from the nature of the act (as they are, for example, in burglary). The kind of passionate conflicts that lead to murders rarely make the motives of the crime absolutely clear. In the highly intensive interaction between the presumed offender and the victim the crime may have been provoked, for example, by requiring self-defense, or by consent before the presumed rape of a woman.

In addition, conviction before a jury generally requires that the defendant be judged not only legally but also morally responsible for the crime. In spite of the legal tradition, rape of a prostitute or murder of a really oppressive husband, seems to be a lesser crime. Evidence of moral responsibility is much harder to produce in crimes of passion. Finally, complaint to the police is something of a betrayal of those to whom we have close personal ties. Once the complaint is made, and the immediate danger and anger past, the personal ties or embarrassment of the complainant quite often reassert themselves, and the main source of evidence refuses to testify further.

The processing of the presumed offender then typically takes the following form: the arrest may be made on a warrant after preliminary investigation, but fairly often the offender is still at the scene and is arrested without a warrant; after the arrest supporting evidence must be collected by skilled investigation, both questioning and examination of physical clues; this is generally challenged very carefully and fully

[6] For instance, in a study in Denmark, 57.0 per cent of murder victims were relatives, 30.8 per cent acquaintances (ranging from "close" to "met the day before"), and only 12.2 per cent strangers (see K. Svalastoga, "Homicide and Social Contact in Denmark," *American Journal of Sociology*, LXII [1956], 37–41).

in court. A fairly general administrative pattern then is for uniformed patrol police to be called first; they come and take control of the scene and of the relevant evidence, and perhaps make arrests. Then the work passes to the detective force, which tries to establish the case (and generally takes credit for the conviction or blame for the poor case).

Crimes against persons in public places tend to depart from this pattern in several respects. In the first place, the officer is much more likely to happen on the scene, so that his own information is sufficient to justify arrest. Second, the assailant is generally unknown and not intimately tied to the victim. Consequently the location of the assailant may be more of a problem. In the case of strange assailants, however, the establishment of legal and moral responsibility is easier if they are located, and there is less motivation for the complainant to drop the case. The only crime for which statistics are given that uniformly falls in this category is robbery. As expected, the proportion cleared by arrest is somewhat smaller (42.5 per cent) and the proportion of charges leading to conviction considerably higher (64.8 per cent). ("Robbery" refers to taking things from persons, generally strangers, by violence or threat of violence.)

Crimes against persons, then, normally take place within the boundaries of morally dense small social systems. They come to the attention of the police typically through complaint, and arrests are usually easy to make. But conviction is very problematic, and requires detailed investigation of high skill.

b) Illegitimate businesses and "dangerous" organizations.—Illegitimate businesses, such as prostitution, the illegal narcotics trade, and gambling in most of the United States, use the institutions of privacy to cloak activities that the society chooses to suppress. "Dangerous" organizations such as revolutionary or conspiratorial political parties stand in approximately the same relation to the institutions of privacy. These types of crime do not have "victims" in the same sense as do theft or murder. Instead the "victim" is an active participant in the system, either as a customer or party member. Consequently complaints do not come from within the system to the same degree that they do with crimes against persons. On the other hand, illegitimate businesses do not generally (directly) create disorder in public places. This means that patrol alone does not produce evidence justifying arrest or conviction. And although revolutionary political organizations must produce speech in public places, the speech is generally tempered enough (or if not tempered, ineffective enough) not to threaten the peace.

These conditions set the stage for the characteristic police practices in this field: "undercover" work, harassment, and regulation of the (relatively epiphenomenal) public aspects of the business. The prevalence of "undercover" work follows directly from the nature of the offense: the fact that it does not produce complaints, and that it takes place behind the barriers that protect privacy. Secret police activity is more prevalent *in societies* where the government routinely invades and distorts the functioning of small social systems; it is more prevalent *within* liberal societies when the suppression of illegitimate business or "dangerous" organizations is demanded by law and police policy.

By harassment I mean periodic or continuous enforcement of laws not normally enforced against the general public against people in illegitimate businesses or "dangerous" organizations. For instance, street soliciting of various kinds may be held to be a nuisance, but this is more likely to be enforced against prostitutes than against the Salvation Army, and the quality of evidence required to arrest prostitutes may be negligible.[7] Or a license required for "street

[7] For the situation in London, see Anonymous, *Women of the Streets* (London: British Social Biology Council, Secker and Warburg, 1955), pp. 18–23.

vendors" may be required of the salesman of radical newspapers, though no other news vendors have been required to have licenses.

Harassment is easy to carry out because the participants are quite often infamous, without many friends who will appear in public in their defense, and personally demoralized, without a conviction of their own innocence. This is, of course, more true of people who run illegitimate businesses than of those who belong to "dangerous" organizations. The pattern of harassment produces a degree of discretionary power over illegitimate business by police. This discretion may be privately appropriated by the officer or by a political machine and used to "tax" illegitimate business.

The third pattern of law enforcement activity often produced by illegitimate business or "dangerous" organizations is tight regulation of public manifestations. In Baltimore, for example, the set of bars in which solicitation for prostitution goes on is concentrated in a relatively small area, and during the evening hours this area is very heavily patrolled. This prevents street soliciting and obnoxious forms of "barking" of the strip shows (prevalent, at least several years ago, in San Francisco), prevents the bars from serving minors, keeps bars closed after hours, and generally maintains public decorum without substantially disturbing the business. The same pattern is found in many large cities in the area set aside for "lunatic fringe" speakers. Heavy patrol helps prevent speakers being attacked, prevents potential riots from getting very far, prevents audiences from interrupting traffic, and so forth.

The general problem presented to police by illegitimate business or "dangerous" organizations, then, is that "crimes" do not become "known to the police."[8] When they do become known, conviction is generally relatively easy for the demoralized employees of illegitimate businesses, and for "dangerous" organizations depends on the public temper and the mood of the Supreme Court. In liberal societies, activity by secret police posing as customers of illegitimate businesses or as members of "dangerous" organizations tends to be concentrated in this field. The barriers of privacy require that the police get permission to enter where the "crime" is being carried on, and they have difficulty getting that permission if they are openly policemen. Harassment and corruption are both latent consequences of the legal suppression of systems that function behind barriers of privacy.

c) Invasion of private places by criminals.—Burglary, much larceny, and trespass involve the invasion of the private place of another without permission, generally in secret. These crimes present distinctive enforcement problems because they very often result in complaint to the police, yet the secrecy of the act makes it difficult to locate offenders. Relatively few of the "crimes known to the police" are "cleared by arrest." The proportion is lower than that of crimes against persons, either in public (robbery) or in private (see Table 1 above).

Unless they are caught in the act (which is the main way complaints of trespass originate), the offenders can generally be caught only by physical evidence (e.g., possession of contraband), by informers, or by confession. Confession quite generally happens only when the offender is arrested on some other charge or illegally arrested "on suspicion," and some informing takes place under the same conditions.

If enough evidence has been collected to connect a particular person with the crime to justify arrest, there is generally enough evidence to justify conviction. Contraband does not change its testimony out of love or pity. The act itself communicates its intention much better than does, say, the act of killing someone. The establishment

[8] Statistics on "crimes known to the police" are not given for these offenses in the *Uniform Crime Reports*, probably for the reasons given above.

of legal and moral responsibility is therefore not so difficult, and most arrests result in convictions.

These are crimes, then, where both arrest and preparation of evidence are generally the job of specialized investigative police. These police must be highly trained in scientific analysis of physical evidence, must have contacts in the underworld for information, and may find a period of questioning arrested people "under pressure" very useful. Although relatively few arrests are made, generally on the basis of carefully collected evidence and therefore on a warrant, a large share of these result in conviction. These are the crimes, and not the murders that figure so heavily in detective stories, which produce the ideal typical pattern of "detective" activity. A latent consequence of this type of crime is the "third degree."

d) *Disorder and nuisance in public places.*—The regulation of public places is the central responsibility of patrol police. We may distinguish three main types of public disorder: individual, collective, and structural. Perhaps the ideal type of individual disorder is "drunk and disorderly." By "collective disorder" I refer primarily to riot, parades that get out of hand, and other types of collective behavior, though in unusual circumstances private military groups may create collective disorder. By "structural disorder" I refer especially to the modern phenomenon of the traffic jam. No crowd or individual "wills" a traffic jam.

1. Individual disorder in public places consists mainly of doing things that would be entirely legitimate if done in private, such as getting too drunk to stand up, or sleeping on park benches. Other individual disorder, such as soliciting for prostitution, or begging, may be illegal even if done in private, but is in fact relatively safe there.

This means first of all that there is a good deal of difference in the "commission" of these "crimes" according to the degree of access people have to private places. Homeless men are obviously more likely to commit the crime of vagrancy, which is the crime of being a homeless man. And if they get drunk, homeless men are more likely to have to sleep it off in the street. Since there is a rough correlation between social class and access to private places (particularly *enough* private places to cover most of one's social life), individual public disorder is related to social class even if the behavior of all classes is the same.

The fact that the "commission" of individual disorder is an index of the lack of connection to small social systems results in a main characteristic of these offenders, that they have not the will, the money, the friends, or the reputation to make good use of their legal right to defend themselves. For will depends on social support of intimates; money for defense often comes from the collective resources (or credit) of a small system; friends are products of intimate interaction; and reputation is generally dependent on a guaranty of good behavior by a small system. Those whose ties to small systems are weak are at a disadvantage in all these ways. Hearings before a police court magistrate in these cases are generally purely formalities; it is assumed by all concerned, including the defendant, that the presumed offender is guilty. The only question that remains to be decided is how much *noblesse oblige* the magistrate should show. As Table 1 shows, the conviction rates for these crimes are quite high, and this is not the result of the sterling qualities of the evidence.

The information on which arrest is based is generally collected entirely by the patrolman, and he has a relatively wide degree of discretion about whether behavior constitutes "disorder." Arrests are almost entirely without a warrant. The fact that police information rather than complaint starts the proceedings means that there are no "crimes known to the police" that have not been solved. The FBI has the good sense not to try to report how many people were drunk or disorderly on the streets of the nation during the year. Investigative police are hardly ever involved.

In summary, the substitution of police for small bounded social systems in the government of the streets produces discretionary power in the hands of the police, particularly over population groups that are unlikely to defend themselves vigorously and effectively in court. The arrest is rarely justified to a judicial officer before it takes place, nor afterward except by the word of the policeman. When convictions do not follow on arrest, it is generally due to *noblesse oblige* rather than to defense by the presumed offender.

2. Collective public disturbances are quite often the immediate stimulus to the formation of quasi-military uniformed police forces. The London Metropolitan Police were partly an answer to the impending Chartist agitation; many state police forces in the United States were originally designed as a more controllable and delicately adjusted mechanism than the National Guard for dealing with industrial disturbances. Since police forces are almost always in a minority in a riot, their military organization is essential in this field.

Collective public disturbances by their nature involve questions of political legitimacy, the channels of political expression, the nature and role of the military, and other very complex topics. These are beyond the scope of this paper. Briefly, however, police are appropriate tools only for temporary control of acting crowds and are rarely competent to defeat an organized military effort by a social movement (unless they have much more military organization than American police do, as the Spanish *Guardia Civil*), and rarely have much to do with the basically political process of channeling the discontent that results in riot. Their competence does extend to expressive crowds that get out of hand, but, as the reams of material on the collective disturbances surrounding the House Un-American Activities Committee hearings in San Francisco in the Spring of 1960 show, the ultimate questions are not police questions.

3. Structural disorder in public places, as opposed to collective disorder, arise from the disorderly effects of individual "innocent" actions. A traffic jam, or a smog problem, is at most attributable to "negligence" of individuals, that is, to *their* not taking account of the structural effects of their actions. More often, the traffic jam could not have been avoided by people as individuals, even if they tried. Traffic control then consists in the regulation of otherwise innocent action that would have unfortunate consequences if not regulated.

Since the "offenses" do not offend the moral sensibilities of the population, being convicted is not much of a defamation. There are few reasons to protest innocence except to avoid the penalty, but the penalty for "minor" infractions cannot be very great. Though generally preserved in fiction, the whole adversary procedure is generally dispensed with, and all the complicated changes of status of due process are done away with. Patrolmen have factual discretionary power to levy penalties for traffic infractions. The police here play the peculiar role of administrators of the anonymous masses, rather than the role of detectors and punishers of crime.

DETERMINATION OF DIVISION OF LABOR IN POLICE PRACTICE

We may briefly group these socially determined activities into the functional divisions they normally have in police departments:

1. Traffic patrol and other patrol of structural disorder, which enforce regulations not involving moral turpitude of those who break the law. Typically due process of law is irrelevant to the processing of offenders, arrests are uncommon, and when made are made without a warrant. Enforcement does not involve the invasion of private places.

2. Street patrol (including radio cars), especially in downtown areas, to control individual offenses in public places and to be quickly available in cases of coercion within small social systems. The offenses

generally involve some degree of moral disapproval, but the friendlessness and lack of resources of the defendants largely eliminates due process of law. Information is obtained primarily by the patrolman's observation, on which both arrest and conviction are based. Arrests are generally without a warrant.

3. Investigative work, generally involving complaint from a small social system, which has to be turned into acceptable evidence of crime. The crimes may be against people within these small systems, in which case arrest is easy (often done by patrolmen) but conviction difficult. Or it may involve the invasion of private places by criminals, in which case arrest is difficult but conviction easy. Most arrests made on warrants and focus on due process derive from this area. But these kinds of offenses create pressures toward the "third degree," toward illegal search and seizure. It is police activity in this area that makes the police force a heroic and newsworthy enterprise, rather than merely a technical device for administering the streets of a city. And much police ideology about "hardened criminals" derives from work in this area. Many of the differences between federal police and local police, generally attributed to the federal agents' greater competence or the greater control by federal courts (e.g., the fact that federal police generally arrest on a warrant), are attributable instead to their work being almost entirely investigative.

4. Undercover work, in which fraud is used to get inside social systems otherwise protected by the institutions of privacy. Subversion and illegitimate business are the main offenses dealt with.

5. Quasi-military action, in which the problem is to apply coercion to control the public riot of some social movement. There quite often are few arrests. Perhaps most of the military trappings of police departments derive from their historical origin in the control of rioting.

CONCLUSIONS

The argument of this paper is that differences in police practice with respect to different types of crimes are closely related to the social location of these types of crime vis-à-vis the institutions of privacy. We would be on more solid ground if direct classifications of crimes of one type (such as murder) were made by social location, rather than depending on the known correlation of murder with private places. Then we could determine whether murders that took place between mutually anonymous people showed a systematically different administrative pattern than murders among kin, eliminating the possibility that other features of murder explain the differences in administration noted. Or people arrested for a crime could be systematically classified by the solidity of their connection to small social structures, to see how much of the variation in the operation of due process of law in criminal matters really derives from differences in the strength of the social ties of the offender.

Second, the indexes of differences between administrative practices for different types of crime used here are extraordinarily crude. While this may convince us that there must have been a strong difference in reality to create a difference in such crude measures, and hence indicate that further research is worthwhile, many of the more interesting hypothesized relations cannot be tested by these data. For instance, the proportion of witnesses changing their testimony or refusing to testify further for different types of crime (either the crude types we have used here, or refined social-location types as suggested above) could be computed, to see whether this indeed explains any of the variation between the conviction rates of murder and burglary. Likewise, the proportion of times the complainant named a suspected offender, the length of time between complaint and arrest, the official status of the officer making an arrest, the proportions of arrests where a warrant was previously obtained,

the number of entries into private places by police officers in the course of enforcement, and so on, could be precisely computed.

From knowledge of the characteristic social location of different types of crime, we can make predictions about the structural changes that are likely to take place in a subsection of a police department when a new crime is added to its responsibility. If the crime has a fundamentally different social location than the crimes that have traditionally been the responsibility of the subsection, then we would predict that new specialized roles will be developed in the subsection to deal with the new crime. On the other hand, if the new crime is fundamentally similar in social location to the crimes previously dealt with, responsibility for it will probably be added to the role obligations of old roles. For instance, if the control of the narcotics trade (an illegitimate business) is newly added to a police subsection which has previously dealt with burglary, murder, and other crimes of a different kind, we would predict that narcotics law enforcement will quite quickly become the responsibility of specially designated officers. If a newly serious narcotics problem is made the responsibility of a police subsection that already handles illegitimate businesses, such as a vice squad, we would expect much less pressure toward specialization. Any actual study would have to take account of other forces that help determine the degree of specialization, such as the size of police departments. Both the establishment of such a phenomenon, and an exploration of the dynamics of role differentiation under these circumstances, would increase our knowledge of the causes of the division of labor in organizations.

As for the general problem of studying the causal relations between rare events and frequent events, which is in some ways the foundation of the problem of studying authority, we have less to say. It is commonly alleged in police circles that Supreme Court cases on admissible evidence (e.g., whether evidence will be admitted that is collected in an "unreasonably long" period between arrest and appearance before a magistrate) will have a large effect on the efficiency of police practice. I am less hopeful. It seems that with our present research technology, the connection between such rare events and the frequent events on which we can collect statistics must remain a qualitative step in the research process. The definition of private and public places in the law and its application to the situations in which different types of crime are committed is an example of such a qualitative step in this paper. Presumably there is a long chain of events on a large scale, structuring rewards and constraints in police practice, and structuring criminal behavior of different types with respect to the norms established and continually enforced in a few court cases. This chain of events should, in theory, be amenable to study. But it is often a matter of historical time and of rather subtle readjustments of other rare events (such as promotions of policemen) to the new situation, so that in practice such studies are extremely difficult to carry out. It seems to me that if sociology is to enter in a systematic way into the empirical study of the sociology of law, the technique of combining qualitative judgments of which norms apply to which behavior, with statistical study of that behavior, is going to need substantial development. I hope that the strong relations among variables derived in this qualitative way, found in Table 1, help to provide motivation for more attention to this problem.

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SOME FUNCTIONAL ASPECTS OF STABILITY AND CHANGE IN VOTING

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ABSTRACT

Examined here is a seemingly erratic voting behavior of a growing suburban electorate during the 1950's that went increasingly Republican in national elections, while it simultaneously switched to Democratic on the state level, and erupted into a "townspeople" third-party split locally. Survey data are from stratified samples of party members. An index of partisanship used as a dependent variable reveals high party loyalty among the majorities of non-switchers, and low partisanship among changers. Data support and extend a theory by Berelson and Parsons that non-rational voting of individuals develops into a political system with mechanisms to insure both its stability and flexibility. Religion is seen as a major cross-cutting factor in partisanship.

Scholarly studies of American voting are but twenty years old, yet already they have largely demolished the myth of Rational Man in the democratic process: the alert voter who sees issues, seeks knowledge about them, weighs alternatives, and comes to an intellectual decision on the merits of the case.

The major studies have shown, in fact, that political campaigns have little effect on prior dispositions; that little information crosses partly lines; that the voter who is under "cross-pressures"—say, a Republican married to a Democratic spouse—often has a low interest in politics, postpones coming to a decision, and frequently ducks the dilemma by not voting at all. All studies show that about three out of four persons vote for the same party as their parents did, and seldom change from election to election. Some have documented that few voters have any real perception of issues, and, should issues be seen, it is usually in a light that makes a position pro or con compatible with the voter's previous political posture.[1]

Furthermore, each succeeding study over the two decades has largely reaffirmed the major conclusions of those that have preceded it. There has been elaboration in one or another areas, refinement of points, but even when quite different working hypotheses have been held there has been no real dispute in their findings with the principles briefly enumerated here. So the body of sociological knowledge about voting behavior grows and firms.

The present study hopes to extend the contributions of its forebears. For it has several features not found in them.

Notably, all studies cited here have been made of *single elections*, and generally at the presidential level. The Erie County, Ohio, study was in the presidential year of 1940;[2] Elmira was in 1948;[3] and even though the University of Michigan studies of national voting principally cover the years 1952 and 1956,[4] they are essentially surveys of discrete elections, using separate samples for each. This characteristic of earlier studies has caused the political scientist, V. O. Key, Jr., to term the survey instrument "static," providing only isolated microscopic findings at single points in time. "Even panel studies cover periods so brief that they are for most purposes a single point in political time. . . . Our major concern must be the population of elections, not the population of individual voters."[5]

[1] Notably, Paul F. Lazarsfeld, B. Berelson, and H. Gaudet, *The People's Choice* (New York: Duell, Sloan & Pearce, 1944); Bernard Berelson, P. Lazarsfeld, and W. McPhee, *Voting* (Chicago: University of Chicago Press, 1954); and Angus Campbell, P. Converse, W. Miller, and D. Stokes, *The American Voter* (New York: John Wiley & Sons, 1960).

[2] Lazarsfeld *et al.*, *op. cit.*

[3] Berelson *et al.*, *op. cit.*

[4] Campbell *et al.*, *op. cit.*

[5] V. O. Key, Jr., "The Politically Relevant in Surveys," *Public Opinion Quarterly*, XXIV (Spring, 1960), 55–56.

On this score, we are more fortunately endowed. For this is a study of a *constant electorate as it has reacted to a variety of national, state, and local elections.* Moreover, the inquiry spans a *series of years* at each level: four presidential elections from 1944 to 1956; three gubernatorial elections from 1950 to 1958; and three local elections from 1955 to 1959.

The second distinction is a technical one. We were interested in a situation of seemingly paradoxical behavior. And since voting is an all-or-nothing act with no shading of distinctions between being, say, "a little bit Republican" or "a lot Republican," we incorporated a priori in the study an instrument designed to reveal as a dependent variable the *degree of partisanship* that groups of voters have toward one or the other of the regular major parties.

The scene of our study is the much publicized "exurban" community of Westport, Connecticut,[6] about 50 miles from New York City. From a population of perhaps 8,000 at the close of World War II, it had burgeoned to about 20,000 in 1959, with attendant growing pains. Further, the new arrivals were sharply different from the older, characteristically Yankee "townspeople." The newcomers are ferociously educated; college attendance among some groups of our respondents was as high as 80 per cent.[7] Their incomes are accordingly high. Among the family heads who mostly commute to New York, one-fourth are in what has been termed "communications"—advertising, publishing, radio, television, the applied and performing arts. One-third are professionals. In contrast, the majority of older residents have only a high-school education, and most work in trades and services.

In addition, Westport, in contrast to some of its neighbors,[8] has become "open" to religious minorities, and approximately one-fifth of the newcomers are Jewish.

In this setting, the political behavior that caught our interest was a marked *increase* over even the normal 60–65 per cent Republican majority in the Eisenhower elections of 1952 and 1956 at the same time as the same electorate was voting increasingly Democratic on the state level, climaxed by an unprecedented Democratic majority for Governor Ribicoff in 1958, plus the abrupt emergence on the local level of a serious third-party split that in 1957 came within 41 votes out of 7,000 cast of upsetting even the traditional Republicans!

Findings of the earlier studies had emphasized the essential stability of voting from year to year, and even from generation to generation. Westport, then, offered a dramatic opportunity to explore what was *de facto* a considerable social, and apparently political, change. Yet there will be surprises in store for those who expect any important reversal of the findings of Lazarsfeld, Berelson, and Campbell and their associates.

Obviously, we can report here but a fraction of the data gathered. What follows will be only a few highlights, with occasional statements supported by findings that have been retained.[9]

The study was made among stratified samples of 9,737 registered voters on the Registry List of November, 1959. The proportions of Republicans (49 per cent), those who had declared no affiliation ("Unaffiliateds") (33 per cent)[10] and Democrats (18 per

[6] The town is a center of Fairfield County, featured prominently in A. C. Spectorsky's *The Exurbanites;* it is the locale of Max Shulman's *Rally round the Flag, Boys!* a neighbor of the setting for Eric Hodgin's *Mr. Blandings Builds His Dream House,* among others.

[7] The 1960 Census shows that, of all Westport adults twenty-five years of age and over, 46 per cent have attended college and 29 per cent have graduated, against national figures of 16 and 8 per cent, respectively.

[8] E.g., Darien, Connecticut, the scene of Laura Hobson's *Gentlemen's Agreement.* Also Westport has a thriving synagogue, which no neighboring town has.

[9] The full account is in David Wallace, "The Sociology of Stability and Change in One Suburb's Voting" (unpublished Ph.D. dissertation, Columbia University, 1962), to be published by Doubleday & Co.

[10] Until 1955 Connecticut had no primary law. Its proportion of Unaffiliated voters thus is unusually high. State registrations in 1960 were: Democrat-

cent) being known, a *n*th selection of approximately five hundred voters was taken from each group. A mail questionnaire asked about the subjects' voting history in all recent presidential, gubernatorial, and local elections for which they were eligible; 829 (54 per cent) replies were analyzed.[11]

The measure of degree of identification with the major parties was constructed from a multi-item question selected from the Gallup Poll. The question was:

As of your feeling today, which party—Republican or Democratic—do you think serves the interests of the following groups best?

Farmers
Business and professional people
Skilled workers, such as carpenters, plumbers, etc.
Unskilled workers, such as manual laborers
White-collar people, such as office workers

The response to this question is, of course, distributional, with *X* per cent naming the Republican party and *Y* per cent the Democratic party ("Both," "No opinion," etc., were eliminated as offering no contribution to partisanship). A beginning reduction of the data, then, can be made by taking the difference between the percentages. In all groups, the Democratic percentage was subtracted from the Republican percentage, giving a possible range of partisanship from 100 (all Republican mentions, no Democratic) to −100 (all Democratic, no Republican). The net scores of partisanship for the registration groups are shown in Table 1.

Note that in every row there is an unbroken regression from "Business and Professional People" to "Unskilled Workers" in the scores from "Republican best" minus "Democratic best" mentions. And in every column there is a similar drop within the occupational classes from registered Republicans to Unaffiliateds to registered Democrats.

TABLE 1

REPUBLICAN MINUS DEMOCRATIC PER CENT MENTIONS AS PARTY BEST FOR FIVE GROUPS

	Business-Professional	White Collar Workers	Farm Workers	Skilled Workers	Unskilled Workers	Mean of Row
Registered Republican	96	68	38	18	−20	40
Unaffiliated	76	30	−24	−42	−66	− 5
Registered Democrat	8	−68	−74	−86	−96	−63

Having such a smooth array of scores, we can safely take a mean of the rows for a single index of the degree of partisanship in each sample group. In this case, it is 40 for registered Republicans; −5 for Unaffiliateds; and −63 for registered Democrats (scores are skewed to the negative because of two "workingman" occupations in the battery; they thus discriminate relatively rather than absolutely). Note that the balancing point between registered Republicans and registered Democrats is −12, or substantially the mean score of −5 for Unaffiliateds who have refrained from formal ties with either party. Similar partisanship measures can be obtained as a dependent variable for other subgroups we may wish to examine. For example, registered Democrats who did, or did not, switch to Eisenhower in 1952.

The reported voting of the registration

ic, 30 per cent; Republican, 30 per cent; and Unaffiliated, 40 per cent.

[11] The response rate was slightly higher among registered Democrats and Republicans than among Unaffiliateds. Totals: Republicans, 269 (55 per cent), Democrats, 332 (60 per cent), Unaffiliateds, 228 (46 per cent). There is known and accepted bias in the response as a representation of the registration universes, but it does not affect these 829 respondents as individuals.

groups in recent presidential elections is shown in Table 2.

One thing immediately apparent is that there has been very little change in party voting for president from year to year. If one is a Republican, he votes for the Republican candidate—no matter who. If one is a Democrat, he votes for the Democrat—no matter who. About eight out of ten party members have always voted for their party's candidate, whatever the circumstance. The only notable departure occurs when there is an outstanding "personality" in the field, such as Roosevelt in 1944, and Eisenhower in 1952 and 1956.

TABLE 2

TWO-PARTY PRESIDENTIAL VOTE OF REGISTRATION GROUPS (Per Cent)

Voted for	Registered as		
	Republican	Unaffiliated	Democrat
1956:			
Eisenhower	98	71	18
Stevenson	2	29	82
1952:			
Eisenhower	97	70	22
Stevenson	3	30	78
1948:			
Dewey	89	55	12
Truman	11	45	88
1944:			
Dewey	80	37	6
Roosevelt	20	63	94
N*	(245)	(189)	(294)

* The number of eligibles declines, of course, in each election. N's shown are for 1956. Minimum N's in 1944 were: Republican, 173; Unaffiliated, 130; Democratic, 224.

To check on the point that these totals do not conceal a greater flexibility in voting—say, of individuals who may move back and forth in different elections while their aggregates remain the same—special tabulation was made of voters who *always* have voted for the same party in every presidential election for which they were eligible. Among registered Republicans, 82 per cent *always* have voted for the Republican candidates; among registered Democrats, 70 per cent *always* have voted for the Democratic candidate; while 44 per cent of the Unaffiliateds *always* have voted Republican, and 20 per cent *always* have voted Democratic.[12]

What is more, almost the same proportions of voters now support the same party that their parents did. Among both registered Republicans and Democrats, the number who have made a clean break from parents' usual voting is only about two out of ten. Some additional number of both groups say their parents were "independent," but there is strong inference from other sources that such "independence" falls short of full open-mindedness to both parties. Moreover, a detailed analysis of intergenerational change by party and religion reveals no evidence for the doctrine that "suburbs make Republicans." In fact, Protestants are a little, and Jews a lot, more Democratic than their parents. Only Catholics are more Republican.[13]

To see something of what has been called "the heavy hand of prior generations" on political behavior today, Table 3 shows how regular and crossover presidential voters report the usual voting of their parents. Also, we will add the index of partisanship to see the degrees of party commitment of these classes of voters.

In short, the most consistent party voters are those whose parents also favored the same party, while the switchers frequently have parents who supported the opposition. Parental transmission of political values is clearly seen in the partisanship index: the higher the proportion of parent voting for either party, the higher is the disposition of present voters to that party, regardless of how they are formally registered. Note that crossover voters invariably are lowest in partisan sympathies.

In presidential voting, then, the picture is one of extraordinary stability, with little change among individuals from one election to the other, and little more change across entire generations.

[12] The higher own-party voting of Republicans than Democrats is due largely to Eisenhower enthusiasts among younger, recently eligible voters.

[13] Wallace, *op. cit.*, chap. vi, pp. 94–116.

But, as mentioned at the outset, while Westport was delivering a solid 65–75 per cent Republican plurality in national voting, and had never fallen below 57 per cent Republican in any state election back through the 1930's, this same electorate in 1958 went 54 per cent Democratic to help re-elect the incumbent Governor Abraham Ribicoff.

The causes of the reversal were complex and involved elements both of "push" away from the Republicans and "pull" toward the Democrats. Also, the movement was statewide and, as noted by the local newspaper, was the most complete landslide to the Democrats in the present century. Locally, part of the shift's roots lay in an internecine squabble between the Republican party's Eisenhower and Taft wings, compounded by an unpopular routing of the new Connecticut Throughway through Westport during Governor Lodge's 1950–54 Republican administration. However, some part of the "pull" was undoubtedly the pleasing personality of Abraham Ribicoff, a proven conservative, and the first Jew to be governor of any state in twenty years.

The gubernatorial drift away from the Republicans began in 1954, and can be seen in the drop of the actual Republican vote from 73 per cent in 1950, to 66 per cent in 1954,[14] to 46 per cent in 1958. Thus, even in 1954, while polling a comfortable margin in Westport, the Republican cause had already lost almost one out of ten persons who had supported Governor Lodge in 1950. So we have a step-by-step change in which an ever increasing number of Westport Republicans became disaffected with their own party.

TABLE 3

PARTY VOTING IN ALL PRESIDENTIAL ELECTIONS BY PARENTS' VOTING AND PARTISANSHIP INDEX

	N	How Parents Usually Voted (Per Cent)*			Partisanship Index†
		Republican	Independent	Democrat	
Registered Republican:					
Always voted Republican	185	71	14	15	49
Varied	42	48	19	33	7
Always voted Democratic*	3	(33)	....	(67)	(−40)
Unaffiliated:					
Always voted Republican	78	65	16	19	26
Varied	66	35	15	50	− 9
Always voted Democratic	32	12	35	53	−58
Registered Democrat:					
Always voted Republican*	14	29	21	50	− 7
Varied	79	29	15	56	−47
Always voted Democratic	178	22	21	57	−74

* Registration of avowed Democrats in the predominant Republican party is not unknown. Reasons given are to check literature, etc. Democrats who have always voted Republican are usually younger Eisenhower voters.

† Mean of row scores for "Democratic party best" subtracted from "Republican party best," as discussed on p. 163.

For this reason, let us look especially at Republicans who stayed with the party in both elections as compared with those who broke away in 1954 and in 1958. This is done among a subsample of voters who were eligible and who did vote for governor in both years.

As Table 4 shows, even in 1954 at the first reading on the Republican decline, one out of five registered Republicans already had defected to the Democratic Ribicoff. By 1958, the Republican revolt reached such a swell that more than four out of ten Repub-

[14] Ribicoff meanwhile was elected in 1954 by a paper-thin state majority.

licans supported Ribicoff. In contrast, only one out of twelve Democrats switched loyalties in 1954, while practically none did in 1958. There was deterioration of Republican strength in all groups between the two elections, but the loss was greatest among Republicans themselves.

Let us now see who were the changers in three registration groups. To do so, we shall use three categories. First are persons who voted Republican in 1954 and also did so in 1958 (symbolized as *R-R*). Second are those who voted Republican in 1954, but switched

TABLE 4

TWO-PARTY GUBERNATORIAL VOTING OF REGISTRATION GROUPS IN 1954 AND 1958

(Per Cent)

Voted	Registered as		
	Republican	Unaffiliated	Democrat
1954:			
Republican	79	36	8
Democratic	21	64	92
1958:			
Republican	58	22	1
Democratic	42	78	99
Republican change	− 21	− 14	− 7
N =	(150)	(126)	(171)

to Democratic in 1958 (symbolized as *R-D*). Third are those who voted Democratic in both years (symbolized as *D-D*). And, as of before, our greatest interest will be in the dominant Republicans who were chiefly responsible for the Ribicoff victory in 1958.

We will first consider how the parents of present subjects usually voted (Table 5). Quite clearly, we can see that, among registered Republican and Unaffiliateds, those who voted Democratic in both 1954 and 1958 are persons who have the greatest history of non-Republican parental voting. Those who stayed Republican in 1954 but switched in 1958 are the next highest in non-Republican parental voting; while those who stayed Republican through thick and thin have the highest proportion of Republican parents.

Using the same categories of loyals : switchers, we turn to the index of partis ship; positive values indicate degrees of p tisanship to the Republican party, and ne tive values degrees of Democratic partis ship (see Table 5).

In each registration group, persons v voted Democratic in both 1954 and 1958 much lower in Republican partisanship t are those who either switched later in 19 or those who stayed Republican in b years. To put it in more useful theoret terms, those who changed most readily more nearly like the followers of the pa to which they moved. Note that desp registration labels there is a considera overlap in partisanship among the grou Registered Republicans who switched ea est in 1954 are actually much more sym thetic to the Democratic party than Unaffiliated voters who voted Republica both years. And in every parallel catego Unaffiliateds are lower in Republican pa sanship than are registered Republicans, though, as we have seen in presidential v ing, they most often vote Republican.

Moving on to the local scene, we ha inferred a conflict of interests between incoming, higher socioeconomic level "co muters" and the more modestly positio "townspeople." Politically, the educa and vocal newcomers in both parties w aggressive, with most Republicans being the Eisenhower stripe, and the Democr avid Stevensonians. During the early 195 they had largely displaced the more c servative townspeople from the machin of both parties, with attendant resentm from the latter. A several-year effort by newcomers to introduce a new town char to replace "government by crony," as local newspaper put it, brought the conf to a boil. The charter was passed agai strong opposition in July, 1957, occasion an open break in the prevailing Republic party that culminated in the first prim in Westport's history.[16] The result wa

[16] As mentioned in n. 7, Connecticut had no mary law until 1955—the last of the then fo eight states to adopt one.

hairbreadth victory for the liberal wing. With the Republicans thus split wide open, the stage was set for a third-party coalition of disaffected townspeople from both regular parties. It was titled, unambiguously, the Taxpayer party. Thus, three candidates squared off for the local Selectman contest of 1957: Republican, Democrat, and Taxpayer.

When the results were in, the count was: Republicans, 2,503 (36 per cent); Taxpayers, 2,462 (35 per cent); and Democrats, 2,004 (29 per cent). The loss of 2,462 votes from what up to then had been strictly a Republican-Democratic show gave vigorous warning that a substantial number of persons were not unwilling to leave the regular parties when personal interests were sufficiently aroused.

In 1959, the same three candidates faced each other again. As election day approached, it looked as if the race might be as close as that of 1957. At this point, a bipartisan committee of newcomers approached perplexed Democrats with the argument, "Look, Republicans and Democrats are not so much against each other as the Taxpayers are against both of us. Between the two of us, Republicans have the better chance to win. But to make sure we both don't lose, how about your voting Republican this once?" Some Democrats did, providing almost exactly the margin of Republican victory over the Taxpayers.

The local contest of 1959, then, gives us two classes of switchers. First are the dissidents who withdrew from both regular parties to form the Taxpayer coalition. Second are the Democrats who added their weight to the hard-pressed Republicans. Meanwhile, of course, there are the regulars who remained loyal to both parties regardless of the turbulent circumstances.

TABLE 5

VOTE PATTERNS OF REGISTRATION GROUPS IN 1954 AND 1958, BY USUAL VOTING OF PARENTS AND BY INDEX OF PARTISANSHIP
(Per Cent)

	Voted in 1954 and 1958							
	Registered as Republican			Unaffiliated			Registered as Democrat*	
	R-R	R-D	D-D	R-R	R-D	D-D	R-D	D-D
Parents usually voted:								
Republican	74	57	40	76	60	33	40	22
Independent	11	26	27	12	5	28	....	19
Democratic	15	17	33	12	35	39	60	59
N=	(82)	(30)	(30)	(25)	(20)	(72)	(10)	(143)
Index of partisanship	59	46	10	44	− 3	−21	−12	− 66
N=	(59)	(24)	(24)	(19)	(14)	(59)	(9)	(125)

* The R-R cell among registered Democrats is so small that it is omitted.

When we apply the index of partisanship to the switchers, it will now occasion little surprise to find that the persons who left the regular parties, either for the new Taxpayer party, or Democrats to help out the Republicans, are lowest in sympathy with the party to which they nominally belong. The 1959 election is simply another instance of change being produced by those who are least tied to party loyalties (Table 6).

The study also contained a measure of "interest in political affairs." Scores were on a five-point scale, running from 1 = very

little, to 5 = very much. Details must be omitted here, but median scores for the registration groups as a whole are: Republicans, 3.1; Unaffiliateds, 2.9; Democrats, 3.4.

When we apply the interest measure to voters in 1959, we find that in all groups, persons who dropped out of the regular parties to form the third-party coalition are lowest in political interest. Also, we find that Democrats who voted Republican on this occasion are the *next* lowest Democrats in political interest. Unaffiliateds, or those who have withheld overt support for either party, are still lower in interest regardless of how they vote. The greatest number of Taxpayer voters were drawn from the Unaffiliateds, and from the lowest interest levels among them.

TABLE 6

1959 SELECTMEN VOTE, BY INDEX OF PARTISANSHIP AND INTEREST IN POLITICAL AFFAIRS

VOTED IN 1959 AS	PARTISANSHIP		INTEREST	
	Index	N*	Median Score	N
	Registered as Republican			
Republican	45	160	3.1	199
Taxpayer	15	10	2.4	22
Democratic	21	5	3.2	10
	Unaffiliated			
Republican	7	63	2.9	85
Taxpayer	−15	42	2.7	56
Democratic	−31	18	3.5	28
	Registered as Democrat			
Republican	−37	39	3.4	46
Taxpayer	−38	30	2.8	45
Democratic	−75	159	3.7	182

* N's for index of partisanship are about 25 per cent lower than for interest in political affairs because of omission of "same" and "both" mentions in computing the index.

Having a voting record of the same persons at three different levels enables us to look at partisanship *in extremus*. To do so, we will consider the rock-ribbed, straight-party voters who in *all* national, state, and local elections, in all years, never once voted for other than the same party, despite the extraordinary opportunities to switch in the swings to Eisenhower, the reversal to Ribicoff, and the splitting off of the major local third party. These are *hyperpartisans* in any book. And now we have what we have seen before, only magnified. The most extreme partisanship scores, and the highest political interests are tied to the most rigid voting behaviors (Table 7).

TABLE 7

INDEXES OF PARTISANSHIP AND POLITICAL INTEREST OF VOTERS IN ALL NATIONAL, STATE, AND LOCAL ELECTIONS FOR WHICH ELIGIBLE TO VOTE

Voted in All Elections	Index of Partisanship	Median Interest in Politics	N
	Registered as Republican		
All Republican	58	3.2	83
Some other	31	3.0	117
	Unaffiliated		
All Republican	67	3.2	13
Some other	− 8	2.8	121
All Democratic	(−100)	(3.0)	7
	Registered as Democrat		
Some other	− 54	3.1	137
All Democratic	− 83	4.1	91

In attempting to summarize and interpret these behaviors, we find ourselves drawn to a model suggested by Berelson *et al.* in their famed "Democratic Practice and Democratic Theory,"[16] which was given later theoretical elaboration by Parsons in a

[16] *Op. cit.*, chap. xiv.

review of the *Voting* materials.[17] We have almost a complete replication and, in some ways, an extension of the findings on which they based their discussions of the functional interdependence of stability and change in an enduring democracy—the phenomenon through which essentially non-rational actions of individual voters develop into a political *system* containing mechanisms that insure both its persistence and its adaptation to a continuously changing social and technological environment.

In the abstract, the model is this. Both parties contain a broad range of partisanship within their ranks, originating from non-rational socialization influences of family, religion, and other social groupings. The degree ranges from, at one extreme, a majority of relatively partisan voters who seldom under any circumstances vote for other than their party, to, at the other extreme, a minority of mildly partisan voters who are largely indifferent to party choice, if they bother to vote at all. Our Unaffiliateds shelter a number of the latter. Strongly partisan voters are generally more interested in politics, vote more regularly, and have made themselves largely inaccessible to any virtue, communication, or logical suasion by the opposition.[18]

If, then, the strongly partisan majorities consistently and determinedly always vote for the same party (all studies of presidential voting, our own included, show that about three out of four persons do so), the political system possesses inherent stability from election to election, and from generation to generation. And having gone all out, voted the straight-party ticket again and again, the majorities gain a *satisfaction* that they have done their utmost to reaffirm their principles; they are psychically freed from whatever may result from the actions of others.

But precisely because the partisan majorities are so polarized in their rigidity, the effective power of the less interested and less partisan minorities is magnified beyond the number of indifferents. When two parties are balanced at about 50-50, as in most presidential contests, a movement of 5 per cent is a monumental force. The partisans in effect relegate control of the outcome to those who feel no special discomfort in voting for *either* side. Divisive tension is thus dissipated from the system. It is a case in which rigidity *forces* adaptive flexibility into the system.

Consider now a further element. Most voting studies, including ours, show that party voting is greatest at the national level. Being remote from direct, personal consequences, abstract party loyalties are resorted to. But as the contests move to the state and local levels, additional close-at-hand interests may be brought to the fore; a greater number of dimensions may be involved. In our study, indignation over the unpopular route of the Connecticut Turnpike was a large factor in the decline of Republican Governor Lodge, as were personalities, power factions, and "gold-plated schools" in the local third-party split. As the number of salient dimensions in a contest increases, so do the opportunities for cross-cutting influences that can override and splinter otherwise stable loyalties. Let us look at one such dimension found in our data that illustrates how possibly others may raise the threshold, or vary the scene, of voting flexibility.

Throughout the study, religion (and whatever other background influences are associated with it) was found to be a discriminating factor in partisanship.[19] Table 8 will help summarize some relationships found among religion, parents' usual voting,

[17] Talcott Parsons, "Voting," in Eugene Burdick and A. Brodbeck (eds.), *American Voting Behavior* (Glencoe, Ill.: Free Press, 1959).

[18] See, e.g., the findings of Lazarsfeld *et al.* that little communication crosses party lines, that party members seek out their *own* party's arguments to reinforce prior prejudices, and also that "cross-pressured" voters withhold exposure to either side (*op. cit.*); and the finding by Berelson *et al.* of distortion of issues to accord with one's vested position.

[19] And on occasion, in switch voting. In 1958, for example, 31 out of 32 non-Democratic Jews in our sample voted for Ribicoff, although in a small follow-up of the sample in 1960 no apparent effect was seen among Catholics in voting for Kennedy.

present political registration, and degree of partisanship. The voting categories below contain two terms: how parents usually voted, and how self is presently registered.

As we have seen throughout, the most consistent party voters are those who have followed their parents in the same party (the *R-R*'s and the *D-D*'s). The index of partisanship is the dependent variable.

TABLE 8

INDEX OF POLITICAL PARTISANSHIP, BY PARENTS' USUAL VOTING, OWN REGISTRATION, AND RELIGION

	Parents/Self*			
	R-R	*R̄-R*	*D̄-D*	*D-D*
	Index of Partisanship			
Religion:				
Protestant....	44	34	−14	−58
Catholic......	37	2	4	−38
Jewish, other..	(6)	−40	−63	−77
	N			
Protestant......	97	51	85	29
Catholic........	16	43	20	22
Jewish, other....	6	14	55	96

* Code: *R-R* = parents voted Republican and self is registered Republican; *R̄-R* = parents not Republican while self is one or more steps toward Republican (i.e., parents Democratic and self Unaffiliated, or parents "independent" and self Republican, or parents Democratic and self Republican); *D̄-D* = parents not Democratic while self is one or more steps toward Democratic (the reverse of *R̄-R*); and *D-D* = parents voted Democratic and self is registered Democrat.

The data indicate that, in every category of parents/self voting from two-generation Republicans to two-generation Democrats, Protestants are generally more Republican (or less Democratic) than are Catholics and, in turn, than are Jews. Hence, religion has a cross-cutting effect independent of parents' voting. And equally clear among the religious groups is that parents' voting has a cross-cutting effect on the degree and direction of partisanship. Even when the voter thinks he is changing from the pattern of his group, he does so only a bit. He is restrained from full capitulation by the way he has been taught to see things and by his common associations with others.

Note that the three groups that are the most partisan (and hence most stable) Republicans are either Protestants of any political parentage or two-generation Republican Catholics, each with adjacent social ties of either parents' voting or religion. The most extreme Democratic partisans are persons again who have ties of either parents' voting or of Jewish or other minority religions. Where differences in parents' voting and religion both intervene there is generally only mild partisanship. Among some of the groups on the diagonal, in fact, partisanship is so mild that they seem almost a contradiction of their formal registration. Particularly, would we expect Jewish *R-R*'s and *R̄-R*'s to be unstable Republicans, as they were in the Ribicoff victory of 1958. And we might doubt the Democratic dedication of Catholic and Protestant *D̄-D*'s who were found in considerable numbers in the Eisenhower sweeps of 1952 and 1956.

Religion and family patterns are strong social ties, of course, since they are culturally transmitted almost intact (though look at the variations they produce in system!). But crossing these as warp and woof are the further relatively acquired dimensions of social class, regional, economic, marital, intellectual, and other interests. High partisanship is perhaps a matter of least squares of differences among all possible dimensions, symbolically wrapped. Low partisanship is lesser alignment of environments, the unwitting agent of casual change against entrenched rigidity elsewhere.

Finally, even though our data pertain only to voting, and though we have confined our discussion to political systems, the temptation is great to speculate whether the same model of cross-cutting relationships with differential sensitivity is not just as applicable to the processes of group formation and dissolution almost everywhere.

COLUMBIA UNIVERSITY

WORKING-CLASS CONSCIOUSNESS, RACE, AND POLITICAL CHOICE[1]

JOHN C. LEGGETT

ABSTRACT

A study of 375 Detroit blue-collar workers focused on voting in the 1958 gubernatorial election. Workers were classified according to degree of working-class consciousness, race, and union membership, as well as their voting preferences for either the reform or conservative candidate. Class consciousness failed to differentiate the voting choices of Negro workers to a significant degree, since race proved to be the overriding factor. Almost all Negro voters, regardless of militance or union membership, supported the reform candidate. However, class consciousness and union membership had a predictable impact among white workmen.

The political consequences of working-class consciousness take many forms. Under certain circumstances, class militance contributes to the development of a revolutionary party committed to drastic alterations of the political order. In other situations, however, a high degree of consciousness may constitute the partial basis for a labor party based largely upon working-class support and dedicated to the pursuit of ends attainable within the parliamentary framework. A third historical alternative cannot be overlooked: working-class backing of reform politicians not selected by labor but responsive to its demands. We shall focus on this third political course.

Several decades ago, Goetz Briefs clearly delineated these three alternatives.[2] Other writers have not specified the various consequences as clearly but have generally concentrated on one of them. Toward the end of the nineteenth century, Engels noted the importance of consciousness in preparing the working class for successful participation in parliamentary politics,[3] although both he and Marx had earlier specified the conditions under which working-class consciousness might contribute to revolutionary behavior.[4] During the pre- and post-World War II period, social scientists such as Centers,[5] Glantz,[6] Jones,[7] and Kornhauser[8] studied consciousness in a society lacking both revolutionary and labor party politics and found it related to political choice. Interestingly, social analysts during the present period have not examined the impact of consciousness in a comparable setting. Perhaps this lack of current research effort stems in part from the assumption that status considerations

[1] I am deeply indebted to the Social Science Research Council, the Horace H. Rackam School of Graduate Studies, the Department of Sociology of the University of Michigan, and the Department of Political Science of Wayne State University for their assistance. In addition, I would like to thank Gerhard Lenski, Herbert Blumer, Morris Janowitz, Daniel Katz, Werner Landecker, and Robert Mowitz for their provocative criticisms and suggestions.

[2] Goetz Briefs, *The Proletariat* (New York: McGraw-Hill Book Co., 1937), p. 96.

[3] See Friedrich Engels, "English Fabian Socialism," in Karl Marx and Friedrich Engels, *Basic Writings on Politics and Philosophy*, ed. Lewis S. Feuer (Garden City: Doubleday & Co., 1959), p. 446.

[4] Relevant excerpts from their writings can be found in T. B. Bottomore and Maximilien Rubel, *Selected Writings in Sociology and Social Philosophy* (London: Watts & Co., 1956), pp. 184–88.

[5] Richard Centers, *The Psychology of Social Classes* (Princeton, N.J.: Princeton University Press, 1949).

[6] Oscar Glantz, "Class Consciousness and Political Solidarity," *American Sociological Review*, XXIII (1958), 375–83.

[7] Albert W. Jones, *Life, Liberty and Property* (Philadelphia: J. B. Lippincott Co., 1941), pp. 315–17.

[8] Arthur Kornhauser, Albert J. Mayer, and Harold L. Sheppard, *When Labor Votes* (New York: University Books, 1956), p. 43.

should prove more relevant than class position when predicting the contemporary behavior of many Americans.[9] Although this assumption may lead to valuable theoretical insights and research findings, it does not obviate the importance of class and class consciousness. Our task, then, is to weigh the significance of the latter and to judge whether the class stress emphasized by Glantz, Jones, and Centers is still useful.

Following Briefs, let us hypothesize that *working-class consciousness should lead to support of the reform politician committed to the advancement of working-class interests*. Admittedly, the utility of this position rests largely upon its modification. As one approach to the treatment of political matters, it must be qualified if one chooses to analyze the political preferences of workers located in an advanced industrial town characterized by considerable economic insecurity and racial segregation. First, workers who are highly class conscious will presumably give unitary support to the reform politician when a class issue such as unemployment forms the backdrop of the election, and the reform politician takes a clear, credible, and progressive position on the matter.[10] Under these circumstances, the political prescription common to the most advanced sections of the working class specifies support for the politician who promises to act in terms of labor's interests. Second, variations in working-class consciousness will not distinguish between the political preferences of workers located within a subordinate racial group *when* the reform politician takes a well-defined, plausible, and positive stand on civil rights. Under these circumstances, many of the voluntary associations and mass media located within this subordinate racial group help to specify its political interests along racial lines, work to develop a collective political consciousness based upon racial aspirations, and thereby act to prepare the bases for unitary racial support of the reform politician.[11] When political solidarity of this sort develops, class consciousness although relevant to the political choices of many of the workmen found within the subordinate racial group, fails to differentiate political preferences among them since even the *least* class-conscious workmen support the reform candidate.

PROCEDURE

THE SAMPLE

In order to gauge the political consequences of consciousness, 375 male blue-collar residents of the city of Detroit were interviewed during the spring and early summer of 1960. A list random-sample procedure was used to select male respondents from seven residential districts, three of which were predominantly Negro, three Polish, and one northwest European. Of these workers, three-quarters belonged to a union. Eighty per cent of these were

[9] Many writers treated the American scene during the 1950's as if it had been one of general prosperity, de-emphasized the importance of class, and stressed the relevance of status. Illustrative was a series of essays edited by Daniel Bell, *The New American Right* (New York: Criterion Books, 1955).

[10] Alfred W. Jones took a very similar position on this matter (*op. cit.*, pp. 314–17).

[11] The block voting of Negroes in cities like Detroit constitutes a case in point. An illustration comes to mind. Largely through their own community efforts, Negroes were recently able to elect a white sociologist (and college professor) to the city council, in spite of the fact that white newspapers had dubbed him an "egghead" and "ultraliberal." In an election where candidates were elected on the basis of city-wide and not ward vote, the tactic adopted by the white press apparently worked in many parts of the city. The sociologist ran well behind almost all of his competitors in the all-white precincts. However, he swept most of the Negro neighborhoods. Few believe that he could have won without the support of Negro newspapers, churches, civil-rights organizations, labor-union associations, and block clubs. In the same election, a solid Negro vote was also instrumental in electing a new mayor pledged to revamp the local police department.

unionists members of the CIO, an organization extremely active in Detroit politics.[12]

DEGREE OF WORKING-CLASS CONSCIOUSNESS

Working-class consciousness is defined as a cumulative series of mental states, running from class verbalization through skepticism and militance to egalitarianism. *Class verbalization* denotes the tendency of working-class individuals to discuss topics in class terms, while *skepticism* exists when a worker believes that the wealth is allocated within the community so as to benefit primarily the middle class. *Militance* has reference to a predisposition to engage aggressively in a course of action to advance the interests of one's class. *Egalitarianism* refers to favoring a more equitable distribution of wealth. Since a more detailed specification of these concepts has been presented elsewhere,[13] a very brief statement on how workers were typed in terms of degree of class consciousness should suffice. A distinction was made between militant (militant egalitarians and militant radicals), moderate (skeptics), and non-militant (class verbalizers and class indifferents) workmen. This threefold categorization will be related to political choice.

THE REFORM POLITICIAN

A reform politician is one who normally acts in the interests of the less privileged classes, but does so within the legal framework. G. Mennen Williams—the Democratic governor of Michigan between 1948 and 1960—met these criteria. He supported such measures as a corporation profits tax and workmen's compensation legislation. He also opposed regressive taxation measures and right-to-work proposals, no small task in a state where conservative politics are largely dominated by powerful economic interests. More important, he vigorously worked for federal measures to help the unemployed. His efforts in regard to the latter were specially evident during the 1958 recession. Of course, Williams' declarations on these matters did not make him a "militant egalitarian." Yet they did place him in the same tradition as Franklin Roosevelt and the progressive movements of midwestern politics.

TABLE 1

WORKING-CLASS CONSCIOUSNESS, RACE, UNION MEMBERSHIP, AND POLITICAL CHOICE

Sources of Political Preference	Per Cent for Williams*
Working-class consciousness:	
Militant	76 (123)
Moderate	70 (110)
Non-militant	50 (132)
Race:	
Negro	76 (119)
White	59 (247)
Union membership (whites only):	
Members	66 (185)
Non-members	39 (62)

* Percentages are based on total number of workers and not just those who voted.

EXPECTATIONS AND FINDINGS

The hypothesis specified in the first part of this paper would lead one to believe that there should be a direct relationship between class consciousness and vote for the reform politician. The data presented in Table 1 support this expectation and prove to be statistically significant at the <.05 level. In an election in which Detroit workers gave their overwhelming support to the reform candidate,[14] 76 per cent of the militant workmen supported him. A:

[12] For a detailed statement on sample design see John C. Leggett, "Working-Class Consciousness in an Industrial Community" (unpublished Ph.D. dissertation, University of Michigan, 1962), pp. 54–72.

[13] See my "The Uprooted and Working-Class Consciousness," *American Journal of Sociology*, LXVIII (May, 1963), 685–86.

[14] Of the 375 workmen interviewed, 285 voted, and of these, over 85 per cent (237) cast their votes for Williams. In the city as a whole, he did particularly well in working-class districts, many of which gave him at least a ten to one majority.

the same time, workers who expressed either a moderate or a low degree of consciousness behaved as expected. *Among voters only,* 95 per cent of the militants, 88 per cent of the moderates, and 69 per cent of the non-militants voted for Williams.

Perhaps of more interest than the figures just presented are the workers' justifications for their choices. Apparently, class considerations assumed paramount importance for many of them:

"I like his platform. It was for the working man."

"Because he's more of a man for the poor people. He knows our needs."

"I think he's doing a good job for the poor people. I think he's 100 per cent for the working man."

"I thought he was a good governor. He's a staunch supporter for working-class people."

"He seemed to do fairly well as far as politics go. He's for the working man. That's probably keeping him out of the presidential race."

"As I told you before, I'm strictly Democratic. Williams is number one. He's for labor. He's for the working man."

"He did a pretty good job. He was for the working people."

"He was giving the working man a fair shake, where Bagwell wasn't giving us anything."

"I didn't vote, but I liked Williams. (*Why?*) Oh, I don't know. He seems to be a fair guy. He's honest. If he can do something for the worker, he does it."

"Williams is more for the working class."

"He's a good man. He always helped the working man and was the working man's choice."

As interesting as the table and quotes may be, they do not take into account the independent impact of racial-group membership. This factor has proven to be especially important in the community studied, where, for the past several decades, the vast majority of Negroes (irrespective of gradations of working-class consciousness within the group) have voted in favor of politicians committed to strong civil rights programs. Such was also the case in this election (see Table 1). The voting choices of Negro workers might be treated in yet another way. *Of those Negro workers who voted,* almost 100 per cent of the militants cast their votes for Williams, but what is more interesting, over 90 per cent of the *non-militant* types did so as well. As these percentages indicate, a low degree of class consciousness had little effect on the political choices of these Negro voters. However, if one examines the impact of consciousness within the white category, the information gathered more clearly sustains the paper's hypothesis. Seventy-four per cent of the militants voted for the candidate, while 67 per cent of the moderates and only 47 per cent of the non-militants did the same.

The hard core of Williams support among whites could be found among militants who belonged to the unions. While unionized workers voted disproportionately in favor of the reform candidate (see Table 1), perhaps of more interest was the combined impact of union membership and class consciousness. An analysis of the entire sample of white workers revealed a predictable pattern of voting support for Williams: (1) militant unionists, 81 per cent; (2) moderate unionists, 72 per cent; (3) non-militant unionists, 52 per cent; (4) militant non-unionists, (N = 5); (5) moderate non-unionists, 50 per cent; (6) non-militant non-unionists, 38 per cent. Clearly then, the combination of a high degree of consciousness plus union membership created a block preference, while at the other extreme, non-union types who were less militant voted as expected.[15]

[15] On the other hand, even if this explanation appears feasible, one might still question the data itself, for after all, the items used to measure working-class consciousness do not make any reference to class identification, perhaps the most popular measure of class consciousness. This objection is

INTERPRETIVE REMARKS: CONTEXT AND CLASS CONSCIOUSNESS

Why consciousness should have a consistent impact can be partially explained in terms of (1) the importance of a class issue prior to and during the election campaign, and (2) the class appeal of the reform politician. First, the question of widespread unemployment within the working class formed the backdrop of the 1958 election, since well over 10 per cent of the city's labor force had been unemployed for over six months prior to November.[16] What is more important, the problem assumed a class character. Mass unemployment was not the case, for the percentage of middle-class (that is, white-collar) unemployed was relatively small. Second, the reform politician supported proposals for federal legislation that would have provided jobs for many of the unemployed. Moreover, his position on this matter was made plausible by his long and close working relations with organized labor. On the other hand, the conservative politician was deeply committed to acting in terms of the interests of those who managad the automotive industry, a dominant force in the opposition party. In this case, the obvious ties between the conservative and the industry and his dependency upon these powerful economic interests lent his proposals on unemployment a less than credible character.

These differences between the two politicians plus the salience of the class issue placed the unions (including their political organizations) in an excellent position to propagandize and mobilize their own members, especially workmen who thought in class terms not too different from the appeals used in the campaign.[17] On the other hand, lack of direct contact with most non-union members at the work place lessened the unions' political impact among them, especially those whose attitudes predisposed them to reject appeals that stressed the importance of class interests. Neither organic ties nor mental bonds linked the union to this type of worker.[18]

CONCLUSIONS

This paper has presented material that, first, supports previous findings on the impact of consciousness and, second, indicates its contemporary political importance in a large industrial town. Also demonstrated

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reasonable. However, a close-ended measure of awareness of class position, when substituted for class verbalization and linked to the other measures of consciousness, can be related to political preference. When this is done, the evidence proves to be almost identical with information presented in Table 1. Moveover, when union membership (among whites only) is taken into account, the pattern of findings already noted reappears in almost identical form. The joint impact of union membership and militance remains the same. Clearly, then, the substitution does not change the findings.

[16] The impact of widespread economic deprivation on Detroit Negroes during the late 1950's has been described by David Street and John C. Leggett, "Economic Deprivation and Extremism: A Study of Unemployed Negroes," *American Journal of Sociology*, LXVII (July, 1961), 53–57.

[17] In this regard, several observers noted, "The continued presence of the union's headquarters [UAW] and central staff in Detroit, along with the heavy concentration of its membership here, has stimulated and facilitated active involvement in local and state politics, from elections for the local municipal positions to those for the highest state offices. Along with the state CIO organization, the U.A.W. [has] systematically attempted to influence the selection of Democratic Party candidates, to find and run candidates where Democrats have rarely or never run before, and generally, they have helped to create a vigorous on-going party structure in the state and have participated within the party organization to push toward goals compatible with the union's political objectives" (Kornhauser *et al., op. cit.*, p. 17).

[18] This explanation, then, stresses the importance of the political activities of unions but does not declare that other groupings were unimportant. Political organizations not controlled by labor as well as churches, nationality organizations, neighborhood associations, and other collectivities no doubt contributed to the political mobilization of workers who were both unionized and militant as well.

was the utility of considering the combined impact of consciousness and union membership on the political preference of white workers. On the other hand, consciousness failed to differentiate the political choices of Negro voters, almost all of whom supported the reform candidate. Of course, this paper has not systematically treated a host of relevant considerations, many of which no doubt deserve separate analysis. To cite one example, it has analyzed but briefly the organizational bases for the development of block votes among Negroes and militant whites.

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SOME CORRELATES OF CLASS CONSCIOUSNESS AMONG TEXTILE WORKERS[1]

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ABSTRACT

Data from an earlier study of textile workers are used to construct an index of class consciousness. The index is based on the Marxist assumption that class-conscious workers tend to perceive the class structure in dichotomous terms, the criteria of class placement in economic terms, class relationships as antagonistic, classes as cohesive entities, and the class system as neither desirable nor inevitable. Investigation of the relationships between index scores and certain presumed antecedents and consequences of class consciousness yield both expected and unexpected results. In view of the findings, the relevance of the class-consciousness concept for American workers seems to merit re-examination.

Nearly a decade ago the authors of this paper presented an analysis of the beliefs and feelings of ninety-five textile workers in Paterson, New Jersey, concerning class structure.[2] The assumption of the research was that the range of issues pertaining to class consciousness is not limited to class identification.[3] Our concern was to explore a variety of topics deemed pertinent to the understanding of class consciousness; these aspects were selected following an intensive review of the theoretical literature of class consciousness.[4]

A brief recapitulation of the findings of the earlier analysis will help to set the stage for the present one. Personal interviews, guided by an open-ended schedule, revealed that (1) while the saliency of class was generally limited, class awareness was widespread, (2) two- and three-class divisions of society were most commonly perceived, (3) in defining class membership, economic criteria predominated, (4) class relationships were usually described in terms of partnership rather than antagonism, (5) many workers were convinced of the internal cohesion of classes, and (6) despite common beliefs about the weakening of class lines, a majority viewed classes as both inevitable and desirable.[5]

These findings are in distinct contrast to the social class attitudes described by Warner, Hollingshead, West, and various other researchers.[6] By comparison with

[1] This paper is a slightly revised version of one presented at the meetings of the American Sociological Association in Washington, D.C., on August 31, 1962.

[2] Jerome G. Manis and Bernard N. Meltzer, "Attitudes of Textile Workers to Class Structure," *American Journal of Sociology*, LX (July, 1954), 30-35.

[3] This assumption raises a basic question concerning the adequacy of the approach of Richard Centers in *The Psychology of Social Classes* (Princeton, N.J.: Princeton University Press, 1949). While Centers pays tribute to the Marxist viewpoint, he equates the "naming" of one's class position with class consciousness. Nevertheless, his stress upon economic differentiation and his usage of an "interest-group theory" diverges sharply from most sociological studies of social class membership and relationships.

[4] Among the more pertinent sources of this discussion are: John R. Commons, *Labor and Administration* (New York: Macmillan Co., 1913); Karl Marx, *The Eighteenth Brumaire of Louis Bonapart, The German Ideology,* and *The Poverty of Philosophy;* Werner Sombart, *Socialism and the Social Movement* (New York: E. P. Dutton Co., 1909); Thorstein Veblen, *The Theory of the Leisure Class* (New York: Modern Library, 1934); W. Lloyd Warner, Marcia Meeker, and Kenneth Eells, *Social Class in America* (Chicago: Science Research Associates, 1949); Max Weber, *The Theory of Social and Economic Organization,* trans. A. M. Henderson and Talcott Parsons (New York: Oxford University Press, 1947).

[5] The findings are remarkably similar to those reported by Melvin M. Tumin in *Social Class and Social Change in Puerto Rico* (Princeton, N.J.: Princeton University Press, 1961), esp. pp. 146-53 and 164.

[6] Warner, e.g., has claimed that he found the "economic" approach to social class behavior in-

most other communities in which class attitudes research has been conducted, Paterson is a highly urbanized, industrialized, unionized city with a long history of worker-employer conflict.[7] Under such circumstances, a significant development of class consciousness could be hypothesized. However, in focusing upon the varied dimensions of class conceptions, the previous publication provided neither a summary measure of class consciousness nor any indication of its correlates. The present paper addresses itself to these tasks. This entails both a re-working of some of our originally reported data and an admixture of hitherto unreported data. Our effort is urged upon us by the sparse empirical underpinning of the class consciousness concept and by the current research neglect of that concept.[8]

AN INDEX OF CLASS CONSCIOUSNESS

The findings of our previous analysis suggested that "class consciousness," among our sample of occupationally similar individuals, did not possess a unitary, nor wholly coherent, character. Consequently, the observed patterns concurred only in part with the theoretical formulations proposed by Marx, Sombart, Commons, Veblen, Weber, and others.[9] The occurrence of certain resemblances, however, suggested the need for a re-examination and possible revision of these theories. Although our data were not intended to evaluate the validity of these macroscopic conceptions, the findings indicated the inadequacy of a polar framework—absence or presence of class consciousness—in encompassing the diversity of existing class sentiments.

To obtain a more comprehensive and integrated depiction of the specific aspects discussed in our earlier report, as well as to approximate the measurement of class consciousness more closely, we constructed an "index of class consciousness." By combining responses to certain questions, we felt that a more adequate measure of class consciousness than the usual mere class identification would be available. Any index of a linear form, of course, inevitably oversimplifies the complexity we have indicated. However, to obtain a more compact measure of class consciousness than is currently available, we constructed a scale based upon a simple weighting of responses on five of the class-linked issues analyzed in our previous paper.

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appropriate to the understanding of American class structure. "By class is meant two or more orders of people who are believed to be, and are accordingly ranked by the members of the community in socially superior and inferior positions" (W. Lloyd Warner and Paul S. Lunt, *The Social Life of a Modern Community* [New Haven, Conn.: Yale University Press, 1941], p. 82). This usage more closely parallels Weber's concept of "status situation" (cf. H. H. Gerth and C. Wright Mills, *From Max Weber: Essays in Sociology* [New York: Oxford University Press, 1946], pp. 186–87). The tendency of Warner and others to equate class and status is perhaps responsible for the ongoing conceptual confusion in the field of stratification. From the perspective of the present research, Warner's subject matter might be designated "status consciousness."

[7] Compare this brief description of Paterson with the following criteria for a research site: "We sought above all a well-integrated community where the various parts of the society were functioning with comparative ease . . . where the social organization had become firmly organized. . . . We wanted a group which had not undergone such rapid social change that the disruptive factors would be more important than those which maintained a balanced grouping of the members of the society. . . . We therefore wanted a city under 20,000 and preferably one nearer 10,000" (W. Lloyd Warner and Paul S. Lunt, *The Social Life of a Modern Community* [New Haven, Conn.: Yale University Press, 1941], pp. 38–39). For an even more extreme contrast, of course, see James West, *[illegible]ille, U.S.A.* (New York: Columbia University Press, 1945).

[8] This effort was initially encouraged by the suggestion from Gerhard Lenski (University of Michigan) that such an endeavor was potentially valuable.

[9] Although the present research focuses primarily on the Marxist view of class consciousness, it should be noted that the term does not even appear in the English translations of such an important work as the *Communist Manifesto*. Rather, Marx linked the class concept with such words as antagonism, hostility, struggle, battles, revolution, etc.

Basing our reasoning upon the Marxist conception of class consciousness,[10] we assumed that the more class-conscious working-class individual tends (1) to perceive the class structure in *dichotomous* terms, (2) to perceive the criteria of class placement in *economic* terms, (3) to perceive the relations between classes in terms of *antagonism*, (4) to perceive classes as *cohesive* entities, and (5) to perceive the class system as *neither desirable nor inevitable*.[11] With these ideas as our basis, we assigned weights of 2 to responses we deemed high in class consciousness; weights of 1 to those we deemed intermediate; and weights of 0 to those we deemed lowest.

TABLE 1

WEIGHTS FOR INDEX OF CLASS CONSCIOUSNESS

Item	Weight
1. Number of social classes in community:	
Two	2
Three	1
Other responses	0
2. Criteria of class placement:	
Money, wealth	2
Occupation	1
Other responses	0
3. Nature of class relations:	
Enemies	2
Partnership	1
Other responses	0
4. Existence of class cohesion:	
All classes	2
One class only	1
Other responses	0
5. Class inevitability:	
Undesirable	2
Undesirable but inevitable	1
Other responses	0

Any specific individual could, therefore, obtain a total score from 0 to 10, or low to high class consciousness. Table 1 lists the component items and the weighting system.

The resulting distribution of scores among the subjects in the sample is indicated in Table 2. The small numbers of intensely class-conscious and of wholly class-unconscious subjects, as measured by this linear scale, are partially verified by impressionistic judgments of the behavior of workers in this locale. However, the validity of the scale can, in the nature of the present inquiry, receive only limited examination. To some extent, of course,

TABLE 2

SCORES BASED ON WEIGHTED INDEX OF CLASS CONSCIOUSNESS

Score	No. of Respondents
0	3
1	5
2	4
3	17
4	10
5	18
6	20
7	8
8	7
9	1
10	2
Total	95

its utility will be measured by its capacity to predict accurately a series of responses considered relevant to class consciousness.

CORRELATES OF THE INDEX

The bulk of this paper reports the association between class consciousness, as measured by the index described above, and various situational and behavioral variables. The selected variables are, in our judgment, logically derived antecedents or consequences of class consciousness. We conceive our task as one of assessing the utility of the "index of class consciousness" in coping with research problems in which class consciousness is either a dependent or an independent variable.

We turn first to the putative antecedents, or sources, of class consciousness.

[10] For a comprehensive recent study of the Marxist position, and of some related researches see Ralf Dahrendorf, *Class and Class Conflict in Industrial Society* (Stanford, Calif.: Stanford University Press, 1959), esp. pp. 3–35 and 281–89.

[11] Saliency of class and awareness of class were excluded, owing to the high degree of consensus among the subjects on these matters.

The traditional theory of class maintains that conditions that intensify the economic deprivation of members of the working class help create or sharpen their consciousness of themselves as a class. This view is foreshadowed in Marx's contention that the ever increasing oppression of the proletariat under capitalism would be one factor in promoting the development of class consciousness.

On the other hand, various historical events combine with Marxist ideology to provide a perception of religion ("the opium of the masses") as an ideological force operating to minimize class consciousness and class conflict. Moreover, several recent analyses have indicated the role of religious (and other) affiliations in retarding and blurring class consciousness.[12] Here, then, we have an antecedent condition that is a purported *impediment* to class consciousness.

TABLE 3

INCOME BY INDEX OF CLASS CONSCIOUSNESS*

Weekly Income†	Index of Class Consciousness		
	Low	High	Total
Over $60‡	19	9	28
$50 or less‡	24	12	36
$51–$60	13	17	30
Total	56	38	94

* $\chi^2 = 4.88$; $p < .05$ (1 degree of freedom).

† Not known responses omitted.

‡ Combined for χ^2 analysis.

Drawing upon data collected in our original research, we hypothesized that high levels of class consciousness would be linked significantly[13] with (1) low current income, (2) current unemployment, (3) a relatively large number of unemployment experiences, (4) a relatively large number of strike experiences, (5) low frequency of church attendance, (6) disbelief in future business opportunities for one's children. The relevance of the final item in this list is suggested by Rosenberg, among many others, in his comment that the observation and expectation of social mobility impedes the development of class consciousness.[14]

Of the foregoing hypothesized relationships, only one, that concerning current income, is even partially confirmed by our analysis.[15] Table 3 presents the data on this association.[16]

In interpreting this table, two things should be noted. First, owing to the small number of cases in the sample, the index of class consciousness has been treated as a dichotomy, with scores ranging from 0 to 5 designated as "low" and scores from 6 to 10 designated as "high." Second, in computing χ^2 for this table, the high and low wage brackets were combined, inspection having revealed that those workers earning *intermediate* incomes scored highest on the index of class consciousness. This statistically dubious procedure gains *post factum* justification in certain findings of other researchers. In his study of a socialist movement in western Canada, for example, Lipset concluded:

> Those who are on the bottom of the economic scale will not revolt if they are pushed down further. A person must have a certain amount of security to fight for a larger share. Those who have nothing to lose but their chains are too closely chained, psychologically, to the desperation of their lot to generalize their predicament, face the consequences of a

[12] See, e.g., Morris Rosenberg, "Perceptual Obstacles to Class Consciousness," *Social Forces*, XXXII (October, 1953), 22–27.

[13] At the 5 per cent level of probability or lower.

[14] Rosenberg, *op. cit.*

[15] These negative findings may be compared with the conclusion of a research by Oscar Glantz: "Unionists displayed less out-group antipathy than big businessmen" (see his "Class Consciousness and Political Solidarity," *American Sociological Review*, XXIII [August, 1958], 382).

[16] When the data for only fully employed workers are used, the observed association is even more pronounced ($\chi^2 = 11.28$; $p < .001$).

malcontent position, or otherwise add to their suffering by striving for social change.[17]

This view receives corroboration from the preceding table. Within groups of essentially similar low status, it is likely that individuals of lowest position tend toward resignation, rather than resentment. Such individuals are apt to be constricted in outlook and less inclined toward class cohesiveness. Those workers who are not wholly depressed economically appear prone to experience most intensely a sense of common destiny with other workers. *Relative* deprivation, rather than absolute deprivation, may be more likely to induce class consciousness among members of the working class.

We still face the problem, however, of explaining our negative findings for the other antecedent conditions that we expected to accentuate class consciousness. The concluding section of this paper will briefly consider two alternative explanations: (*a*) the inadequacy of the measure of class consciousness, and (*b*) the possibility that conditions other than those studied operate to impair the linkage of class consciousness with the investigated conditions.

Even more puzzling are the findings on church attendance (Table 4). Among the present sample, a relationship antithetical to the one expected is found. If frequency of church attendance is taken as a measure of religious ties, then the findings are indeed surprising; for subjects of *high* class consciousness attend church *more* frequently than do those of low class consciousness.

Turning now to the presumed *consequences* of class consciousness, we drew upon items investigated in our original study to hypothesize that high levels of class consciousness would be significantly associated with (1) favorable attitude toward government ownership of certain industries, (2) favorable attitude toward the textile union, (3) recent attendance at a union meeting, and (4) political preference for the Democratic party. Here we find ourselves confronted by an apparent tautology. Insofar as "class consciousness" implies a recognition of one's shared economic and political interests, the study of any variable touching upon such interests or upon instrumentalities for realizing such interests smacks of circularity. The rationale for including such variables, however, is twofold: (*a*) they are not explicitly incorporated into our index; and, therefore, (*b*) to the extent that our summary index can predict a variety of such specifics, its utility is supported.

TABLE 4

FREQUENCY OF CHURCH ATTENDANCE BY INDEX OF CLASS CONSCIOUSNESS*

Church Attendance	Index of Class Consciousness		
	Low	High	Total
Monthly or more often	18	22	40
Less than monthly...	39	16	55
Total............	57	38	95

* $\chi^2 = 6.48$; $p < .02$ (1 degree of freedom).

Several interesting results emerge from the analysis of the association of each of these items with the index of class consciousness. The individual's level of class consciousness presumably influences a host of pertinent attitudes. For example, awareness of group inequalities might be followed by more or less consensual views as to desirable means for eliminating or mitigating these "inequities." One such means, in the judgment of many of our subjects, is the government ownership of industry. In response to a question on the government ownership of one or more of four industries (railroads, coal, steel, textiles), a sizable minority of our interviewees (thirty-seven) had favored such ownership. To

[17] S. M. Lipset, *Agrarian Socialism* (Berkeley: University of California Press, 1950), p. 176 (see also Toimi E. Kyllonen, "Social Characteristics of Active Unionists," *American Journal of Sociology*, LVI [May, 1951], 528–29).

our surprise, however, we find no significant relationship between attitudes on this matter and class consciousness.

Persisting in our analysis, we then examined, separately, attitudes toward government ownership of the *textile* industry. Table 5 reveals that, among those subjects giving definite answers to the pertinent question, a significant association *is* present between high class consciousness and advocacy of government ownership in the textile industry.

TABLE 5

GOVERNMENT OWNERSHIP OF TEXTILES BY INDEX OF CLASS CONSCIOUSNESS*

Attitude toward Government Ownership of Textile Industry†	Index of Class Consciousness		
	Low	High	Total
Favorable	8	15	23
Unfavorable	32	20	52
Total	40	35	75

* $\chi^2 = 4.59$; $p < .05$ (1 degree of freedom).
† "Not known" responses omitted.

These data suggest that awareness of group interests is greatest within the specific industry of employment. It may be noted, however, that textile wages are among the lowest of all industries. Hence, the possibility that a greater degree of alienation among the sample is directed specifically against textile ownership must not be overlooked. Nevertheless, the relatively low prevalence of an encompassing ideology of government ownership indicates the limits of class cohesiveness.

The observed association of class consciousness with, at least to a limited degree, the ideology of government ownership does suggest that politics is viewed by some workers as an expression or instrument of class needs. Since governmental action in this society is contingent upon the election of representatives of political parties, the nature of party preferences is conceivably a manifestation of group agreements upon political interests. Among the respondents, Republican affiliations are clearly in the minority—only twelve in all.[18] Fifty-three persons described themselves as Democrats, the remainder as independents or as indecisives. When party preference is compared with class consciousness, it can be seen that Democrats are significantly more likely to have high scores than are other respondents (see Table 6).

It should be pointed out, however, that some of the independent voters expressed the view that neither party represented their interests. As one respondent put it: "Both are on a par. They prefer business all the time. Years ago they were different. Today, it doesn't make much difference to the worker *who* is in. Someday we'll have a better party that will protect the workingman."

TABLE 6

PARTY PREFERENCE BY INDEX OF CLASS CONSCIOUSNESS*

Party Preference†	Index of Class Consciousness		
	Low	High	Total
Democratic party	25	28	53
Republican party or independent	26	9	35
Total	51	37	88

* $\chi^2 = 6.33$; $p < .02$ (1 degree of freedom).
† "Not known" responses omitted.

Our investigation of a *final* hypothesized correlate of class consciousness, degree of commitment to the union (as indicated by favorable attitude toward, and recent participation in, the union), failed to indicate a significant relationship. We encounter difficulty in accounting for this finding, particularly in view of the generally recognized role of unions as instruments of worker interests and, even more particularly, in view of the history of militancy on the part of the union whose members were our sub-

[18] Cf. P. F. Lazarsfeld, B. Berelson, and H. Gaudet, *The People's Choice* (New York: Duell, Sloan & Pearce, 1944), p. 19.

jects. The absence of relationship *here*, as well as the general paucity of positive findings, suggests the need for a re-examination of our procedures and analysis.

CONCLUSION

In summary: Investigation of the relationships between index scores and certain hypothesized antecedents, or sources, and consequences of class consciousness yielded both expected and unexpected results. Statistically significant associations were unexpectedly found between high class consciousness and two antecedent conditions: current income in the *intermediate* range for the sample and relatively *high* frequency of church attendance. Equally unexpectedly, no significant associations were found for such antecedents as current unemployment, a relatively large number of unemployment experiences, a relatively large number of strike experiences, and disbelief in future business opportunities for one's children. While no significant association was observed between class consciousness and the presumably consequent favorable attitudes toward government ownership of certain industries, such association was observed in the case of favorable attitudes toward government ownership of the textile industry. Moreover, class consciousness showed a significant association with political party preference (Democratic) but none with degree of commitment to the textile union.

The generally negative findings of this study may be appraised from two perspectives: empirical and theoretical. Empirically most crucial is the appropriateness of the chosen items for the study of class consciousness. Although selected for their theoretical relevance, the specific items and the weighting scheme for the index are both open to criticism. In addition, by limiting the study to a relatively homogeneous group of unionists, variability of response has undoubtedly been minimized. The resulting clustering of responses around the midpoint of the distribution required a somewhat arbitrary cutting-point.[19] What effect the inclusion of non-unionists in the study might have had can now be only conjectural. Possibly a larger sample, including a more diversified population, might provide a better test of the hypotheses.

In order to assess the internal weaknesses of the index, we made an analysis of the component items. First, we examined the interrelations among the five items, using the χ^2 test of association for each of the ten possible combinations. *Not one* of these combinations yielded an association that was significant at the 5 per cent level.

TABLE 7

VARIABLES ASSOCIATED WITH INDEX ITEMS

Index Item and Variable	Level of Significance
No. of social classes with:	
Current unemployment	.05
Disbelief in children's business opportunities	.02
Government ownership of textile industry	.05
Classes as Cohesive Entities with:	
Recency of union attendance	.02
Favorable attitude toward union	.01
Party preference (Democratic)	.05

This finding in itself does not invalidate the index; for there is no logical or methodological requirement that the various items of an index be closely interrelated. However, it does appear evident that the index includes divergent elements.[20]

Second, we examined the association between each index item and each of the eleven "antecedents" and "consequents" of class consciousness (see Table 7). Three of the five index-items ("criteria of class-placement," "nature of class relations," and "class inevitability") showed *no* significant associations. The negative findings for these items help to explain the limitations of the

[19] However, when the scale is trichotomized (0–3, 4–5, 6–10), *none* of the antecedents or consequents is found to be significantly associated with the criterion.

[20] It should be noted that we have initially assumed that the index referred to a complex, multi-dimensional phenomena. Yet we did expect to find some association between the items.

index. Hence, despite the important role of these items in the Marxist theory of class consciousness, their low predictive power, for the population studied, casts serious doubt upon their utility for index purposes.

Each of the two remaining index items was found to be significantly associated with three variables. However, we observe no overlapping, or duplication, between the two sets of correlates. This finding suggests that the index contains at least two quite divergent dimensions, or patterns of class consciousness. Considering the items associated with a dichotomous view of social classes (current unemployment, disbelief in children's business opportunities, and government ownership of textile industry), we infer that the two-class view of society may involve the traditional "anti-capitalist" dimension of worker class consciousness. On the other hand, the items associated with the belief in the internal cohesion of classes (recency of union attendance, favorable attitude toward union, and Democratic party preference) suggest a less radical orientation to class structure. If these interpretations are correct, our "index of class consciousness" has masked important differences in worker attitudes.

The major theoretical criticism, we believe, concerns the derivation of our class-consciousness index, antecedents, and consequences exclusively from data obtained in our original investigation. While that investigation was guided by ideas from various sources, the research emphasis was placed largely upon certain elements of the Marxist theory of class consciousness. Had it been feasible to reinterview our subjects, we would have supplemented our data for the present analysis with various other items suggested in the recent literature of class consciousness.[21] Our list of antecedents, for example, could have been enriched by the various speculative inventories of the social sources of class consciousness. Thus, we might have investigated such items as the saliency of racial and ethnic memberships among our respondents, their range of direct social experiences with members of other classes, variations in degree of "commodity fetishism" and "market equality" perceptions, and similar information.

Putting aside these questions of theory and method, we are *now* inclined to a somewhat different approach from that which initiated the present study. Although, from the standpoint of the orthodox Marxist theory of class consciousness, the present findings are largely negative, they do tend, in certain respects, to accord with John R. Commons' conception of "job-consciousness." It has been a half-century since Commons wrote that "most of the unions in question are not unions of a class. They are unions of a trade or a strategic occupation."[22] In a later work, he pointed out that:

> As long as the wage-earning class accepts the existing order and merely attempts to secure better wage bargains, its goal must eventually be some form of the "trade agreement," which recognizes the equal bargaining rights of the organized employers. Its union is not "class conscious," in the revolutionary sense of socialism, but "wage-conscious," in the sense of separation from, but partnership with, the employing class.[23]

We are now inclined to agree with this view. And, although our attempt to develop an index of class consciousness has met with only indifferent success, we are hopeful that other students of social stratification will be stimulated—even provoked—to subject class consciousness (or, perhaps, job consciousness) to sorely needed empirical study.

WESTERN MICHIGAN UNIVERSITY
AND
CENTRAL MICHIGAN UNIVERSITY

[21] See, e.g., Bernard Barber, *Social Stratification* (New York: Harcourt, Brace & Co., 1957), pp. 213–18.

[22] John R. Commons, *Labor and Administration* (New York: Macmillan Co., 1913), p. 76.

[23] John R. Commons *et al.*, *History of Labour in the United States*, I (New York: Macmillan Co., 1936), 15.

SOME CORRELATES OF URBAN SIZE: A REPLICATION[1]

LEO F. SCHNORE

ABSTRACT

Urban places in the United States grew substantially in number, in size, and in proportion of total population between 1950 and 1960, but population density declined in all urban size classes over the decade. Nevertheless, the familiar positive association between size and density of population persisted. Other variations with size of place, in both 1950 and 1960, were as follows: *the larger the size,* (*a*) the larger the proportion of "minority" populations, (*b*) the higher the average socioeconomic status, especially as measured by income. The 1960 data reveal only one unexpected finding: the familiar inverse association between size and fertility now appears to be sharply attenuated, for the differences between size classes are no longer as substantial or as regularly patterned as in the past.

The size of the group occupies a prominent place in sociological discussions of "small groups," in analyses of "large-scale organizations," and in treatments of whole communities and societies. In the sociological study of communities, both urban and non-urban, the size of the locality group receives especially close attention. Size enters as a term in most definitions of "urban," as in Wirth's conception of the city as "a relatively large, dense, and permanent settlement of socially heterogeneous individuals."[2] In contrast, the notion of the folk community advanced by Redfield characterized it as "small, isolated, nonliterate, and homogeneous, with a strong sense of group solidarity."[3] We shall be concerned here with community size or, more specifically, with the size of *urban* units. Although relatively little research effort has been devoted to the conditions that make for great size,[4] we can find a number of studies in which community size has been taken as an "independent variable," and in which concomitants or correlates of variations in size as of a particular point in time have been sought.[5]

The most ambitious study of variations according to size of community to date is contained in the census monograph on *Social Characteristics of Urban and Rural Communities, 1950,* by Otis Dudley Duncan and Albert J. Reiss, Jr.[6] Using a "special report" that was based upon a $3\frac{1}{3}$ per cent sample of returns from the 1950 Census of Population,[7] Duncan and Reiss were able to show that a large number of social, economic, and demographic characteristics tend to vary systematically according to

[1] This study was supported by the research phase of the University of Wisconsin Urban Program under the terms of a grant from the Ford Foundation.

[2] Louis Wirth, "Urbanism as a Way of Life," *American Journal of Sociology,* XLIV (1938), 5.

[3] Robert Redfield, "The Folk Society," *American Journal of Sociology,* LII (1947), 293. For an extended discussion, see his essay on *The Little Community* (Chicago: University of Chicago Press, 1955).

[4] This problem has been examined from time to time, as in Donald J. Bogue and Dorothy L. Harris, *Comparative Population and Urban Analysis via Multiple Regression and Covariance Analysis* (Oxford, Ohio: Scripps Foundation for Research in Population Problems, 1954), chap. ii.

[5] Examples of earlier research are the following: William F. Ogburn, *Social Characteristics of Cities* (Chicago: International City Managers' Association, 1937); Otis Dudley Duncan, "Optimum Size of Cities," in Paul K. Hatt and Albert J. Reiss, Jr. (eds.), *Reader in Urban Sociology* (Glencoe, Ill.: Free Press, 1951), pp. 632–45; Leo F. Schnore and David W. Varley, "Some Concomitants of Metropolitan Size," *American Sociological Review,* XX (1955), 408–14; and Fenton Keyes, "The Correlation of Social Phenomena with Community Size," *Social Forces,* XXXVI (1958), 311–15.

[6] New York: John Wiley & Sons, 1956, Part I, "Size of Community."

[7] U.S. Bureau of the Census, *U.S. Census of Population: 1950, Vol. IV, Special Reports,* Part 5, chap. A, "Characteristics by Size of Place" (Washington, D.C.: Government Printing Office, 1953).

size of place. Chapters were devoted to age and sex composition, race and nativity composition, marital status and family characteristics, mobility, education, labor force and occupation, and income. Duncan subsequently employed some of these materials, together with some tabulations not to be found in the monograph itself, in a separate essay on variations according to community size.[8]

Although they worked with the full range of sizes—a noteworthy feature of the study by Duncan and Reiss is the use of statistics for "urbanized areas"—special tabulation units developed for the first time in conjunction with the 1950 census. This unit was deliberately designed to provide information for the "greater city" or "conurbation"—the area of continuous urban settlement extending beyond the political limits of the city as a corporate entity. With certain modifications in definition, similar units were delineated in conjunction with the 1960 Census. It is the purpose of this paper to draw as many comparisons as possible between the Duncan-Reiss materials relating to characteristics of urban areas by size in 1950 and the same characteristics in 1960.

URBAN AREAS IN THE UNITED STATES

Table 1 shows the number of urbanized areas officially recognized in the 1950 and 1960 Censuses, and the number of urban places lying outside urbanized areas, together with the populations contained within each of seven broad size classes. The size classes are those that were employed in the 1950 special report on "Characteristics by Size of Place" and in the subsequent analyses by Duncan and Reiss. It will be seen that the urban places of all sizes are more numerous, and that the urbanized areas (the larger urban units considered here) contain a substantially larger proportion of the total population of the United States. While the 157 urbanized areas contained 46.0 per cent of the 1950 population, the 213 areas currently recognized contain 53.5 per cent of the 1960 population; in fact, almost 30 per cent of the 1960 population was found in the sixteen areas containing a million or more inhabitants. The urban

TABLE 1

URBAN AREAS OF THE UNITED STATES, 1950 AND 1960

Size Class	No. of Urban Areas		Population (000's)		Per Cent of Total Population	
	1950	1960	1950	1960	1950	1960
Urbanized areas	157	213	69,249	95,848	46.0	53.4
3,000,000 or more	3	5	21,214	33,736	14.1	18.8
1,000,000–3,000,000	9	11	16,603	18,050	11.0	10.1
250,000–1,000,000	37	52	17,428	25,990	11.6	14.5
50,000–250,000	108	145	14,005	18,073	9.3	10.1
Other urban places	3,253	3,633	27,219	29,420	18.0	16.5
25,000 or more	193	198	7,131	6,935	4.7	3.9
10,000–25,000	547	605	8,237	9,238	5.5	5.2
2,500–10,000	2,513	2,830	11,851	13,247	7.8	7.4
"Rural" remainder	....	....	54,230	54,054	36.0	30.1
Total United States	3,410	3,846	150,697	179,323	100.0	100.0

Source: 1950 data adapted from Donald J. Bogue, *The Population of the United States* (Glencoe, Ill.: Free Press, 1959), Tables 2-6, p. 38; 1960 data from Bureau of the Census, *U.S. Census of Population: 1960*, Vol. I, *Characteristics of the Population*, Part A, "Number of Inhabitants" (Washington, D.C.: Government Printing Office, 1961), p. xiii, Table E.

[8] Otis Dudley Duncan, "Community Size and the Rural-Urban Continuum," in Paul K. Hatt and Albert J. Reiss, Jr. (eds.), *Cities and Society: The Revised Reader in Urban Sociology* (Glencoe, Ill.: Free Press, 1957), pp. 35–45.

population as a whole rose from 64 to 70 per cent of the total.

Before proceeding to the analysis itself, it would be well to specify the nature of the urban units under investigation. This can be best accomplished by quoting at length from the official description of "Urbanized Areas."

The major objective of the Bureau of the Census in delineating urbanized areas was to provide a better separation of urban and rural population in the vicinity of the larger cities, but individual urbanized areas have proved to be useful statistical areas. They correspond to what are called "conurbations" in some other countries. An urbanized area contains at least one city of 50,000 inhabitants or more in 1960, as well as the surrounding closely settled incorporated places and unincorporated areas that meet the criteria listed below. An urbanized area may be thought of as divided into the central city, or cities, and the remainder of the area, or the urban fringe. All persons residing in an urbanized area are included in the urban population. (There are a few urbanized areas where there are "twin central cities" that have a combined population of at least 50,000.)

It appeared desirable to delineate the urbanized areas in terms of the 1960 Census results rather than on the basis of information available prior to the census as was done in 1950. For this purpose, a peripheral zone around each 1950 urbanized area and around cities that were presumably approaching a population of 50,000 was recognized. Within the unincorporated parts of this zone small enumeration districts were established, usually including no more than one square mile of land area and no more than 75 housing units. (An enumeration district is a small area assigned to an enumerator which must be canvassed and reported separately.)

Arrangements were made to include within the urbanized area those enumeration districts meeting specified criteria of population density as well as adjacent incorporated places. Since the urbanized area outside of incorporated places was defined in terms of enumeration districts, the boundaries of the urbanized area for the most part follow such features as roads, streets, railroads, streams, and other clearly defined lines which may be easily identified by census enumerators in the field and often do not conform to the boundaries of political units.

In addition to its central city or cities, an urbanized area also contains the following types of contiguous areas, which together constitute its urban fringe:

1. Incorporated places with 2,500 inhabitants or more.

2. Incorporated places with less than 2,500 inhabitants, provided each has a closely settled area of 100 housing units or more.

3. Towns in the New England States, townships in New Jersey and Pennsylvania, and counties elsewhere which are classified as urban.

4. Enumeration districts in unincorporated territory with a population density of 1,000 inhabitants or more per square mile. (The areas of large non-residential tracts devoted to such urban land uses as railroad yards, factories, and cemeteries, were excluded in computing the population density of an enumeration district.)

5. Other enumeration districts in unincorporated territory with lower population density provided that they served one of the following purposes:

a. To eliminate enclaves.
b. To close indentations in the urbanized area of one mile or less across the open end.
c. To link outlying enumeration districts of qualifying density that were no more than 1½ miles from the main body of the urbanized area. . . .

The boundaries of the urbanized areas for 1960 will not conform to those for 1950, partly because of actual changes in land use and density of settlement, and partly because of relatively minor changes in the rules used to define the boundaries. . . . In general, however, the urbanized areas of 1950 and 1960 are based on essentially the same concept, and the figures for a given urbanized area may be used to measure the population growth of that area.[9]

[9] U.S. Bureau of the Census, *U.S. Census of the Population: 1960*, Vol. I, *Characteristics of the Population*, Part A, "Number of Inhabitants" (Washington, D.C.: Government Printing Office, 1961), pp. xviii–xix. For a discussion of the 1950 unit see Robert L. Wrigley, Jr., "Urbanized Areas and the 1950 Decennial Census," *Journal of the American Institute of Planners*, XVI (1950), 66–70; reprinted in Harold M. Mayer and Clyde F. Kohn,

This unit clearly conforms to the notion of a "city" as a *physical* rather than as a *political* entity. Many investigators have commented on the limitations that surround the use of statistics for, say, legally incorporated municipalities, and others have argued against omnibus definitions of the city. As Davis has recently observed: "Our preference is to define the city in demographic and spatial terms. In this way a consistent conception can be stated which

TABLE 2

POPULATION DENSITY, BY SIZE OF URBAN AREA, 1950 AND 1960

Size Class	Population Per Square Mile	
	1950	1960
Urbanized areas:		
3,000,000 or more	7,679	6,079
1,000,000–3,000,000	6,776	3,822
250,000–1,000,000	4,468	3,034
50,000–250,000	3,869	2,699
Other urban places:		
25,000–50,000	3,329	2,545
10,000–25,000	2,721	2,272
2,500–10,000	1,992	1,676

Sources: 1950, Weighted averages for size classes from Duncan, *op. cit.*, Table 2, p. 40. 1960, Weighted averages for size classes with populations of individual areas serving as weights, from Bureau of the Census, *U.S. Census of Population: 1960: Number of Inhabitants, United States Summary* (Final Report PC (1)-1A [Washington, D.C.: Government Printing Office, 1961]), p. xiv, Table E, and Vol. I, *op. cit.*, Table 22, pp. 1–40 ff.

is not only close to usage but also leaves open the question of the socio-economic causes and consequences of urbanization." He goes on to point out that "the conception of a city in terms of people and space has the advantage of leaving open the question of the causes of concentration and deconcentration. It allows us to ask how social and economic conditions such as the occupational structure are in fact related to the spatial patterning of human settlement."[10] It is this general type of inquiry for which "urbanized areas" seem best suited.

Outside urbanized areas, we are obliged to work with "urban places." For the most part, these are incorporated places of 2,500 or more inhabitants, but they also include some distinct unincorporated places and some towns or townships treated as "urban" under special rules established by the Bureau of the Census. We shall now proceed to an examination of selected characteristics of these two types of units—"urbanized areas" and "other urban places"—with a view toward identifying systematic variations by size.

SIZE AND DENSITY OF URBANIZED AREAS IN 1960

As previously noted, the Bureau of the Census believes that the 1950 and 1960 "figures for a given urbanized area may be used to measure the population growth of that area." Actually, 149 of 155 comparable areas recognized in 1950 (or 96.1 per cent) increased in population size. At the same time, 151 of these same 155 areas (or 97.4 per cent) had larger physical areas; these changes in territory probably resulted from a combination of two factors: (*a*) actual changes in the extent of the built-up area and (*b*) changes in definitional rules. With respect to density, 89 per cent, or 138 of 155 areas, had lower over-all densities in 1960 than the comparable areas had in 1950. Since physical area was not held constant, of course, the lower levels of density may result in large part from the additions of large tracts of lightly settled territory at the peripheries, tracts that might have been excluded if 1950 procedures had been used again in 1960. Outside urbanized areas, numerous annexations by incorporated places probably exerted a similar influence, generally lowering densities.

Variations in density by broad size classes are shown in Table 2 for both 1950 and 1960. It is noteworthy that lower densities were registered in all size classes, in-

(eds.), *Readings in Urban Geography* (Chicago: University of Chicago Press, 1959), pp. 42–45.

[10] Kingsley Davis, "Foreword" to Jack P. Gibbs (ed.), *Urban Research Methods* (Princeton, N.J.: D. Van Nostrand Co., 1961), pp. xvi and xviii.

cluding the very largest. If one computes the percentage change in average population density between 1950 and 1960, the following results are obtained:

	Per Cent Change
3,000,000 or more	−20.8
1,000,000–3,000,000	−43.6
250,000–1,000,000	−32.1
50,000–250,000	−30.2
25,000–50,000	−23.8
10,000–25,000	−16.5
2,500–10,000	−15.9

The spread of urban areas at lower average densities—or "urban sprawl"—has been one of the most frequently remarked population trends of the fifties, and it is perhaps no surprise to find it reflected in these data. Equally important for our present purposes, however, is the persistence of the traditional positive association between size and density.

RACE AND NATIVITY COMPOSITION

The Duncan-Reiss study of 1950 data revealed rather clear-cut variations with size in the proportions non-white and foreign-born white in all but one region (the South). Both proportions were positively associated with size over the full range of classes recognized in the census tabulations. The larger the size, in other words, the higher the proportion of "minorities." Our results, using 1960 materials, are generally similar, but a few specific features warrant comment.[11]

Table 3 shows the percentage non-white (Negro and "other races" combined) in the seven urban size classes for 1950 and 1960. In addition, data are reported separately for the South and for the remainder of the country, here labeled as the "North and West." Outside the South, the 1960 proportions are generally higher than those for 1950, and the averages for size classes reveal a more clear-cut association with size than was evident in the 1950 materials. In contrast, the South shows a clearly higher proportion non-white in only the largest size class—urbanized areas with between one and three million inhabitants. (There are no urbanized areas in the South larger than three million.) In the four smaller size classes within the South there were smaller proportions non-white in 1960 than in 1950.[12]

TABLE 3

PER CENT NON-WHITE BY REGION AND SIZE OF URBAN AREA, 1950 AND 1960

Size Class	Total United States		North and West		South	
	1950	1960	1950	1960	1950	1960
Urbanized areas:						
3,000,000 or more	8.9	14.1	8.9	14.1	....	....
1,000,000–3,000,000	12.6	13.1	10.7	9.1	22.4	29.8
250,000–1,000,000	11.3	13.4	5.7	9.0	21.5	19.8
50,000–250,000	9.9	10.4	3.7	4.5	22.7	19.9
Other urban places:						
25,000–50,000	9.2	10.5	3.0	4.4	21.7	21.4
10,000–25,000	8.8	9.5	2.6	3.3	21.0	20.3
2,500–10,000	8.3	8.7	1.6	N.a.	19.8	N.a.

Sources: Weighted averages for size classes in 1950 from Duncan and Reiss, *op. cit.*, p. 59, Table 9, and Duncan, *op. cit.*, p. 40, Table 2. For 1960 sources, see n. 11 in the text.

[11] Unless otherwise indicated, all of the 1960 data were taken from the series of "Advance Reports," PC (A3), issued in advance of the Final Report on "General Social and Economic Characteristics," Tables 32 and 33.

[12] Similar regional patterns for Standard Metropolitan Statistical Areas are shown in Leo F. Schnore and Harry Sharp, "Racial Changes in Met-

Table 4 shows comparable data for the foreign-born population in urbanized areas for the two dates. In the column devoted to 1950, the proportions refer to foreign-born *white* persons, while the 1960 data cover the entire foreign-born population without respect to color; since very few non-whites are of foreign birth, however, this discrepancy is a minor one. It is clear that foreign-born persons now represent smaller proportions of the total population in urban areas of all sizes. In only one size class represented in the table, urbanized areas between 250,000 and 1,000,000 in the South, does the 1960 proportion foreign-born exceed that for 1950, and the difference is small. Moreover, there is a rather clear gradient according to size in the rest of the country's urbanized areas. The unweighted averages for the North and West show progressively higher proportions foreign-born moving up the size range.

TABLE 4

PER CENT FOREIGN-BORN, BY REGION AND SIZE OF URBAN AREA, 1950 AND 1960

Size Class	Total United States		North and West		South	
	1950	1960	1950	1960	1950	1960
Urbanized areas:						
3,000,000 and over	16.4	10.6	16.4	10.6	....	....
1,000,000–3,000,000	10.7	6.9	11.7	8.2	4.8	3.5
250,000–1,000,000	6.3	5.3	8.2	6.5	2.7	3.6
50,000–250,000	6.6	4.2	8.6	5.7	2.5	2.0
Other urban places:						
25,000–50,000	5.1	3.8	6.6	5.0	2.0	1.7
10,000–25,000	4.5	3.4	5.9	4.0	1.6	1.5
2,500–10,000	3.9	3.0	5.6	N.a.	1.3	N.a.

Source: Same as in Table 3. Note that 1950 referent was "foreign-born white," while 1960 data refer to the total foreign-born population.

TABLE 5

PER CENT HIGH-SCHOOL GRADUATES BY SIZE OF URBAN AREA 1950 AND 1960

Size Class	Per Cent High-School Graduates in Population 25 Years Old and Over	
	1950*	1960†
Urbanized areas:		
3,000,000 or more	39.4	43.0
1,000,000–3,000,000	39.9	45.3
250,000–1,000,000	39.9	45.7
50,000–250,000	38.3	44.6
Other urban places:		
25,000–50,000	38.9	44.7
10,000–25,000	37.9	43.2
2,500–10,000	35.4	41.2

* Source: Weighted averages for 1950 from Duncan, *op. cit.*, p. 41, Table 3.

† Unweighted averages; see n. 11 in text for sources.

In summary, while the size-of-place patterns observable in 1950 tended to persist in 1960, the proportions non-white increased in urban areas outside the South, and the proportions foreign-born tended to drop off in all regions. In part, these trends reflect changes in the race and nativity composition of the country as a whole, where the proportion foreign-born has been falling for decades and where the proportion non-white has only recently reversed a long-term decline.

SOCIOECONOMIC CHARACTERISTICS

We turn now to three measures of socioeconomic status—education, occupation, and income, three of the characteristics

ropolitan Areas, 1950–1960," *Social Forces*, XLI (March, 1963), 247–53.

that were examined in some detail by Duncan and Reiss.

Education.—Turning to Table 5, concerned with the proportions completing high school, we find that the limited variation by size observable in the 1950 data is repeated in 1960. Only four and one-half percentage points separate the highest and lowest values for 1950, and the same is true for 1960. In short, lack of perfectly systematic variation by size is evident for both years. It is only as one examines smaller urban places (between 10,000 and 50,000) outside urbanized areas that one begins to see the emergence of a size-of-place gradient in the proportions completing high school. Urbanized areas are notably homogeneous with respect to this variable.

TABLE 6

PER CENT IN WHITE-COLLAR OCCUPATIONS BY SIZE OF URBAN AREA 1950 AND 1960

Size Class	Per Cent in White-Collar Occupations	
	1950*	1960†
Urbanized areas:		
3,000,000 or more	47.8	46.3
1,000,000–3,000,000	45.9	47.7
250,000–1,000,000	45.3	46.9
50,000–250,000	42.7	44.5
Other urban places:		
25,000–50,000	42.8	44.9
10,000–25,000	41.0	43.0
2,500–10,000	38.6	41.4

* 1950 data adapted from Duncan and Reiss, *op. cit.*, p. 96, Table 33. The percentage reported for each size class (weighted average) covers those in the following occupational groups: professional, technical, and kindred workers; managers, officials, and proprietors, except farm; clerical and kindred workers, sales workers. It is not to be confused with "white-collar workers as per cent of all non-farm workers," reported in Duncan, *op. cit.*, p. 41, Table 3.

† Unweighted averages for same combination of occupational groups; see n. 11 in the text for sources.

Occupation.—Table 6 shows rather similar results, at least for 1960. The unweighted averages shown in the last column do not show a perfect pattern of systematic increase or decrease with size in the proportions employed in white-collar occupations. Compared to the 1950 data, where there is a much more regular pattern, the 1960 data reveal a lower than expected proportion of white-collar workers in the very largest size class (3,000,000 or more).[18] There is some evidence of convergence with respect to size, for the total range of variation is reduced.

Income.—In contrast with the lack of pattern in education and occupation for 1960, Table 7 exhibits a sharp and clear series of variations by size in median family income. Although we are obliged to use family income rather than individual income (as in the Duncan-Reiss study), the gradation by size is clearly evident in the 1960 data; over $1,600 separates the median values for the largest and smallest size classes represented in the table. Commenting on the 1950 materials, Duncan and Reiss observed that "of all the differences among communities of different size

TABLE 7

MEDIAN INCOME, BY SIZE OF URBAN AREA 1950 AND 1960

Size Class	Median Income, Persons with Income, 1949*	Median Family Income, 1959†
Urbanized areas:		
3,000,000 or more	$2,492	$6,863
1,000,000–3,000,000	2,443	6,658
250,000–1,000,000	2,160	6,096
50,000–250,000	2,057	5,785
Other urban places:		
25,000–50,000	1,899	5,566
10,000–25,000	1,822	5,350
2,500–10,000	1,700	5,223

* Source: Weighted averages for 1950 size classes from Duncan, *op. cit.*, Table 3, p. 41.

† Unweighted averages; see n. 11 in text for sources.

[18] Sharper differences by size in 1950 were observed by Duncan and Reiss when they employed more detailed occupational groups; among white-collar workers, size-of-place variations were most evident among "professional, technical, and kindred workers" and among "clerical and kindred workers" (see Duncan and Reiss, *op. cit.*, Table 33, p. 96).

revealed in this study, perhaps the most striking is the pronounced direct relationship between size of place and income."[14] They showed that this association held for both males and females in 1950, and that it persisted with control of age, color, and region. It is thus no surprise to find family income exhibiting the same general pattern in 1960.

TABLE 8

FERTILITY RATIOS, BY SIZE OF URBAN AREA, 1950 AND 1960

Size Class	Married Fertility Ratio, 1950*	Fertility Ratio, 1950†	Cumulative Fertility Ratio, 1960‡
Urbanized areas:			
3,000,000 or more	56	433	1,585
1,000,000–3,000,000	61	478	1,607
250,000–1,000,000	62	503	1,690
50,000–250,000	63	510	1,685
Other urban places:			
25,000–50,000	64	522	1,667
10,000–25,000	63	525	1,722
2,500–10,000	66	570	N.a.

* Children under 5 years old per 100 married women 14 to 44 years old (Duncan, *op. cit.*, p. 43, Table 4).

† Children under 5 years old per 1,000 women aged 20 to 44 of all marital classes (Duncan and Reiss, *op. cit.*, p. 50, Table 6).

‡ Children ever born per 1,000 women 15 to 44 years old of all marital classes ("Advance Reports," *op. cit.*, Table 32).

URBAN FERTILITY

We turn now to the matter of size-of-place variations in fertility. This is a question that has intrigued American demographers over the years.[15] With respect to variations among urban areas by size, Duncan and Reiss were able to show systematic patterns using two different measures—the fertility ratio and the married fertility ratio. The first two columns of Table 8 show their results. In both cases, an inverse association is evident, that is, the larger the size, the lower the fertility. While the differences between some of the adjacent size classes are not substantial, the general pattern is clear. The 1960 results, however, do not reveal such a perfectly regular pattern, for there are unexpected reversals in the measure (the cumulative fertility ratio) that were not evident in the 1950 materials. As Grabill has observed, "the variation [in fertility] by size of place is only a general tendency. Frequent exceptions are noticeable when individual cities are arranged in order by size."[16] Our data indicate that

TABLE 9

PER CENT FOREIGN STOCK AND PER CENT USING PUBLIC TRANSPORTATION BY SIZE OF URBAN AREA, 1960

Size Class	Per Cent Foreign Stock, 1960*	Per Cent Using Public Transportation, 1960*
Urbanized areas:		
3,000,000 or more	32.4	34.1
1,000,000–3,000,000	25.2	28.7
250,000–1,000,000	18.7	15.6
50,000–250,000	16.5	9.3
Other urban places:		
25,000–50,000	15.2	4.9
10,000–25,000	13.9	1.9
2,500–10,000	12.9	1.5

* Unweighted averages; see n. 11 in text for sources.

[14] *Ibid.*, p. 103.

[15] See Warren S. Thompson and P. K. Whelpton, *Population Trends in the United States* (New York: McGraw-Hill Book Co., 1933), chap. viii.

[16] Wilson H. Grabill, "The Fertility of the United States Population," in Donald J. Bogue, *The Population of the United States* (Glencoe, Ill.: Free Press, 1959), p. 320.

this is no longer even a "general tendency," for they suggest that some convergence has occurred in the past decade.

OTHER CHARACTERISTICS BY SIZE OF URBAN AREA

We have previously shown (in Table 4) that the proportion foreign-born tends to increase with increasing size. The fact that 1960 statistics are also shown for the proportion native of foreign or mixed parentage permits the examination of variations in another aspect of ethnic composition, for we can look at persons of "foreign stock," that is, the foreign-born *plus* those with at least one foreign-born parent. Table 9 shows that the per cent of the population classified as being of "foreign stock" mounts regularly with size of urban area. In the very largest urbanized areas (three million and over), such persons actually make up almost one-third of the total population. (Statistics for persons of foreign stock were not included in the 1950 special tabulations by size of place, so we can say nothing about trends.)

Table 9 also shows another item of information that was not previously available. The 1960 Census, for the first time, collected sample statistics on work place and method of transportation to work. The last column shows the proportion of the employed population that used public transportation in getting to work, according to size of urban area. While one of every three employed persons commuted by mass transit in the largest urbanized areas, less than one of ten did so in the smallest urbanized areas, and smaller urban places registered progressively lower proportions using public transportation in traveling to work.[17]

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[17] This subject is treated in greater detail in Leo F. Schnore, "The Use of Public Transportation in Urban Areas," *Traffic Quarterly*, XVI (October, 1962), 488–98.

BOOK REVIEWS

The Legislative System: Explorations in Legislative Behavior. By JOHN C. WAHLKE, HEINZ EULAU, WILLIAM BUCHANAN, and LEROY C. FERGUSON. New York: John Wiley & Sons, 1962. Pp. x+517. $8.95.

Is a sociology of legislative bodies possible? Careful reading of this book will help to suggest some of the directions it might take, as well as some of its difficulties. This is an important book and justifies its ambitious title, even though it is based on a study of only four American state legislatures—those of New Jersey, Ohio, Tennessee, and California—in 1957. It reports a carefully executed interview study of the approximately five hundred legislators in these four states, organized about an eclectic conceptual scheme devised for the purpose. The authors (political scientists) draw extensively on the literature dealing with legislative behavior, as well as that of the behavioral sciences.

The major contribution of this volume is to define a series of questions about legislative bodies in general, using legislator interviews to answer them for the four states (eight chambers) studied. Individual legislators are classified by their role orientations toward party, district, interest groups, and the differentiated functions performed in the legislature itself. Among these functions are those of leadership, subject-matter expertise, and representation. The four states, the eight chambers, and the parties within them are then characterized by their distributions of role orientations, and these in turn are used to support generalizations about the differences between the states, the parties, majority and minority. For example, New Jersey was the most partisan; Ohio was next except that the minority Democrats were relatively disorganized; California was atomized and individualistic with party counting for less; and in Tennessee the handful of Republicans attained influence only when they held the balance of power bewteen Democratic factions. These differences are in turn associated with a variety of attitudes that legislators displayed toward their own roles and the functioning of their chambers.

Another significant contribution is the delineation of two major clusters of roles observed in the four states. A tendency was observed for the members of the majority party to be more oriented toward the state as a whole rather than the district; to be more concerned with the process of forming legislation or reconciling group interests ("inventor" or "broker" role); and to rely on their own judgment of their responsibilities ("trustee" role) rather than some popular mandate. Minority members tended to assume the opposite roles: district orientation, emphasis on reflection of popular needs ("tribune"), subservience to instructions ("delegates"). This general dichotomous clustering does not, of course, preclude some independent variation in the roles assumed.

The theory and concepts used have a welcome generality for the political scientist, brushing aside the barriers between the study of state, national, and comparative government. Yet for the sociologist, still more generality might have been sought. He must ask what is the general field within which a theory of legislative bodies might fall. Should the concepts of small-group organization be relevant? To what extent is organizational theory relevant to an organization in which all are formally equal in major decisions? To what sorts of collective-decision processes do the participants bring with them major determinants of choice from roles earlier in life and outside the decision-making group? What other sorts of collective decision are so institutionalized or constrained that the rules of the process itself, or orientations developed after entry into the process, are mainly determinant of these choices? These questions, and others like them, will ultimately set the limits and define the content of the broader theoretical field in which the findings of this study will take their place.

A general field of this sort will also of course make use of kinds of data that were not used even in this intensive study: longer time-series, records of behavior other than interviews, information about and from the others outside

the legislature with whom legislators deal. But the data and problems of this book are central to that field, and it cannot develop without them.

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The Polity. By NORTON E. LONG. Edited by CHARLES PRESS. Chicago: Rand McNally & Co., 1962. Pp. xiv+247. $5.50.

Professor Press has assembled seventeen articles and papers by Norton E. Long, a political scientist who, although he never has chosen to express himself at book length, seems to have acquired a widespread and appreciative audience within his discipline and elsewhere in the social sciences. The articles, covering a span of 1937–1961, are presented topically under the headings "Rationality and Responsibility in Policy-making," "Politics and the Economy," "The Politics of the Metropolis," and "The Study of Local Government." One of the articles, a Goffmanesque essay entitled "The Local Community as an Ecology of Games," originally appeared in this *Journal*.

Long's writings should be of considerable interest to sociologists. He is a hardheaded analyst of contemporary American politics, placing his subject matter squarely in its social context. At the same time, many of his essays propose specific political and governmental reforms—often rather unconventional reforms, as in his proposal for the representation of competing political viewpoints within administrative organizations and his advocacy of non-hierarchical administrative decision-making procedures patterned after the organization of inquiry in the sciences. The reform proposals, in spite of their frequently unorthodox character, are tempered by an appreciation of the restraints on institutional change. Thus, although Long sympathizes with the "party reform" thesis that decentralized political parties in the United States impede the government's capacity to respond to domestic and foreign challenges, his deep understanding of the roots of party decentralization prevents him from accepting the easy conclusion that party reform is feasible. Instead, he turns to analysis of the bureaucracy and the presidency as vehicles of co-ordination and direction. (A seeming departure from realism, however, is his advocacy of British-style augmentation of Mr. Nixon's present role, the "titular leadership" of the "out" party.)

It is doubtful whether many readers will want to follow *The Polity* from beginning to end. First, there is the inevitable repetitiousness, as well as discontinuity, in articles written on various occasions for various purposes. Second, Long's style is exceedingly epigrammatic, so much so that an essay sometimes seems to be a string of topic sentences, each of them facile, polished, and insightful, but the major themes difficult to disentangle from subordinate points. Given Long's further tendency to make assertions by historical allusion or reference to classical political philosophy, after reading several essays one feels that one has consumed a surfeit of chocolate éclairs.

The éclair image does not do justice to Long's rigorous and imaginative thought. Many of the essays warrant careful and repeated reading. I would especially recommend "Power and Administration" (particularly for sociologists who feel that Weber's conceptualization of bureaucracy is satisfactory for analyzing American public administration), "Party Government in the United States," "Public Policy and Administration," and the aforementioned "The Local Community as an Ecology of Games." Other readers will have different preferences.

In view of the increasing number of collections of this sort, it is worthwhile to comment on the editorial decision to arrange Long's writings topically and to elide the more dated references to events at the time the essays were originally published. I found too little unity within the sections of the book and too much overlap between them to be helped by the device of topical organization. A more conventional chronological presentation, complete with contemporary references, would probably have displayed Long's thought more effectively by locating it in the context of its development. A fuller bibliography and a biographical note also would have been of interest.

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Foreign Policy Decision Making. Edited by RICHARD C. SNYDER, H. W. BRUCK, and BURTON SAPIN. New York: Free Press of Glencoe, 1962. Pp. vii+274. $5.00.

In 1954, Snyder, Bruck, and Sapin published, although only for limited circulation, *Decision-making as an Approach to the Study of International Politics.* This essay is now republished with three papers: (1) a review article by McClosky (published in *World Politics,* 1956), (2) Snyder and Paige's application of the approach to a study of "The United States Decision To Resist Aggression in Korea" (published in the *Administrative Science Quarterly,* 1958), and Brody's comparison of the conceptual schemes of Quincy Wright, Charles A. McClelland and Morton A. Kaplan, and Snyder, Bruck, and Sapin (published in the *Journal of Student Research,* 1960).

In the Introduction, the editors review developments since the original publication of their essay. They admit certain deficiencies and cite new work that could contribute to and specify their scheme. Although they are not rejecting their past work (indeed, they are republishing it), I feel constrained to direct attention in this review to what they now consider central and enduring in their approach.

In the original essay the editors had set out "to present a tentative formulation of an analytical scheme . . . [to] serve as the core of a frame of reference for the study of international politics." They included a vast range of variables ordered around decision-making. They now believe that the variables outlined were too numerous; they suggest that the variables that will turn out to be most fruitful should "'represent' simultaneously the decision-makers' variables and the mode of relating these in the choice process." This seems to be related to their emphasis upon the decision-makers' perceptions and definition of the situation. In application, the results are strange.

For example, in Snyder and Paige's study of the United States decision regarding Korea made in June, 1950, they ask, "Why this positive response rather than some other?" Among their four answers (the other three have the same character), is "Direct military intervention, having the specific objective of restoring the *status quo,* was commensurate with the basic values threatened." In short, the decision-makers did what they did because they thought it was worth doing. Such answers indicate the risks of emphasizing the definition of the situation. In general, McClosky's telling criticisms of the decision-making approach are not refuted by the later writings included in this book.

Snyder, Bruck, and Sapin are trying to bring sociology and the other social sciences—with the emphasis upon science—to the study of international politics. Sociologists should be cheered by such efforts of political scientists, but in this case disappointment is a more likely response. Although some factors may be added to conventional political science analyses, too often words and concepts are taken over in ways that do not add much.

LOUIS KRIESBERG

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Young Germany: A History of the German Youth Movement. By WALTER Z. LAQUEUR. New York: Basic Books, Inc., 1962. Pp. xxii+253. $6.00.

This book is of considerable interest from the point of view of those chiefly concerned with the course of recent German history and from the point of view of those whose attention is focused on the analysis of social movements. For the author, the first concern must have been predominant; for some sociologists the second may carry greater weight. Actually, both concerns are inseparable because the study of social movements, by its very character, can only be historical. Certainly, the record of the German Youth Movement reveals the anatomy of any social movement: it is indicative of uneasiness and unrest as the Wilhelmian period in Germany nears its end, and of the renewed and profounder disquietude that characterizes the years of transition and confusion during the Weimar Republic; and it shows the phenomenon of milling very concretely in its compulsive inclination toward aimless "rambling." It further shows the element of discussion, but breaks off before decisions were reached. However, these general characteristics have about them the unique flavor of the time and place where they occurred, and the majority of sociologists who will read the book—not to speak of other categories of readers—undoubtedly will seek in the record of the youth movement an answer to the question how recent German history was at all possible.

The author's treatment of the topic has great merits but also leaves something to be

desired. The author, a German Jew who left Germany when he was very young, is both a participant and an observer, an insider and an outsider; he combines some amount of familiarity with remarkable detachment. His sources are solid and diligently assembled; his style has clarity and readability. He avoids "blaming" the youth movement for the naziism that followed, but he also shows that, for all the unreconcilable differences between the two, the youth movement participated in the general drift toward racism and the charismatic conception of leadership that prevailed at the time; he conveys a vivid picture of the dissensions and uncertainties of the period. But, I believe, the outsider more than the insider in the author is revealed in his failure, except for isolated passages, to appreciate the more intimate positive sides of the German Youth Movement. Among these I would reckon the high evaluation of individual experience along with the recognition that such experience is gained in collectivities; the emphasis on sincerity in expression, coupled with informality in human relations; and the conviction that young people must work out their own destiny. These maxims, that only a short while ago sounded revolutionary, are now a near-consensus in education.

Generally, while reference is made to Hermann Schmalenbach's *Die soziologische Kategorie des Bundes* (p. 135), there is no attempt to analyze the concrete ways in which this "category," which is neither *Gemeinschaft* nor *Gesellschaft*, manifests itself in the German Youth Movement. Specifically and amazingly the author fails to comment in depth on the fact that the only youth movement that can be compared to the German, as far as its *Bund* quality is concerned, is the Jewish youth movement that grew alongside the German movement and was profoundly influenced by it. The Jewish youth movement was compelled, as it were, to avoid the mistakes of its German counterpart, turning political; hence, comparison at this point might have thrown light on the reasons for the German Youth Movement's "splendid failure."

One thing is exceedingly clear from the account given, although the author deals with it more *en passant* than in systematic fashion, and that is the quality of paralyzing indecision that is so characteristic of German affairs. The German Youth Movement could not make up its mind collectively, and its members individually, between personality and collectivity, between left wing and right wing, between the freedom of the West and the *mystique* of the East. This fatal and, as it would seem, congenital weakness left it exposed to the ruthless grip of dictatorship.

WERNER J. CAHNMAN

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The Pyramid Climbers. By VANCE PACKARD. New York: McGraw-Hill Book Co., 1962. Pp. x+339. $5.00.

"We are about to enter a veiled and curious world." This is the first sentence in Mr. Packard's book. Unfortunately the veil is never removed but only occasionally flicked aside in the manner of a barker at a sideshow. The world referred to is that of higher management in the modern corporation, and the aim of the book is to examine what it "takes to get ahead" in this world. The author attempts to accomplish his aim neither by direct observation nor by utilizing the views of the subjects themselves. His informants are those who sell their services to industry: management consultants, executive recruiters, psychological testers, and members of the academic community engaged in "sensitivity training." This latter group, identified by the author as members of a movement, are actively working as consultants attempting to transform interpersonal relations between superiors and subordinates.

The views of the assorted informants are accorded equal validity. This, combined with a lack of critical analysis on the part of the author, has resulted in a number of basic contradictions. Assertion follows assertion, interrupted only by the retelling of secondhand anecdotes. Each assertion remains unrelated to the others. For example, we find that, on the one hand, it is necessary to ingratiate oneself with the boss to get ahead. On the other hand, we discover that one of the three characteristic features of life within a large bureaucracy is the impersonality of treatment.

The author not only tells us that things are terrible within corporations but also asserts that they are getting worse. For example, "private bureaucracies have recently become more intrusive in their personal demand on the individual at the managerial level and more manipulative toward that individual." The author's discussion of the present is one in which we can hardly place any confidence; there is no discus-

sion of the past. His assertions of change are without merit.

Packard's main concern is in what he considers to be the destruction of individual initiative and an increasing encroachment by the corporation on the life of the individual manager. The manager's personality is tested, his wife is carefully examined, and his clothes are prescribed. His evaluation depends on his physical appearance. He is a lousy lover, and he is crippled by the dependence upon a superior for favor. Packard sees a way out, however. It lies in overhauling the superior-subordinate relationship and creating an "environment that will encourage a *deep commitment from managers to the company's objectives*" (emphasis mine).

Categorizing this book is not easy. It can best be described as belonging to the same genre as after-dinner conversation. There are some truths and even some insights presented, but, as with gossip, there are no guides to help the reader discriminate between what is or is not representative nor between trivia and what are matters of moment. Also, as with gossip, it emphasizes whatever is sensational.

Near the end of the book the author expresses a concern for the values of those who are involved in the promotion of goods and services that are frivolous and of doubtful social value. He asks "what this kind of huckstering does to the young huckster." If he desires an answer rather than merely asking a rhetorical question he need only go as far as examining his own present condition.

Regardless of the merit of this book it should provide one service to sociologists. It should make clear the neglect of serious research in an area that is so vital to industrialized society.

FRED H. GOLDNER

International Business Machines Corporation

The Community of Scholars. By PAUL GOODMAN. New York: Random House, 1962. Pp. xi+175. $3.95.

The chief virtue of this book is Goodman's attention to the power and status structure of higher education, for this is an area where research is badly needed. By no means does the book fill the gap, but it does remind us of our ignorance. In particular, Goodman bewails the encroachment of an "administrative mentality" into organizations of higher learning. Accordingly, the title refers to an educational setting that the author devoutly and sentimentally wishes to be restored, a setting devoid of administrators and their numerous controls. The facilitative role of administration is dismissed as producing nothing but dysfunctions.

If the reader can tolerate the author's urbane vituperation, his pot shots at social science, his sarcasms, and his patches of opaque writing, he may be rewarded by some critical leads concerning the social structure of higher education. Several issues arise that deserve much more serious thought than Goodman has been willing to give them. For example, what have been the special causes of bureaucratization in education? (Goodman mentions only conflicts with church and state.) What are the mechanisms that shield teachers from administrators, despite the current ideology that identifies the top administrator as an educational leader? (This is a question that Goodman does not bother to ask because of his assumption that every facet of the enterprise is distorted by "coordinative management.") How much role consensus exists between teachers and the larger community, and how are conflicting expectations accommodated or resolved?

Goodman claims to have the answers to almost every question he raises; hence, the final chapters contain recommendations for reform. These include recruiting professional practitioners to teach, giving more self-government to students, eliminating credits and compulsory attendance, and encouraging scholars to secede from the universities in order to set up outside. The last reform is modeled after the medieval universities, to which Goodman devotes a good deal of nostalgic reverie. He fails, however, to note the particular circumstances that promoted the success of these early communities of scholars. The bases for his other reforms are chiefly anecdotes. Thus, if the book is read with an eye to discovering hypotheses and to the kinds of evidence that are needed for proposing reforms, then it might prove to be somewhat valuable. But if it is read as a recipe, full of the slogans and epithets that are so tasty to literary intellectuals who engage in social criticism, then I fear it will be more purgative than nutritious.

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The Effects of Federal Programs on Higher Education. By HAROLD ORLANS. Washington, D.C.: Brookings Institution, 1962. Pp. 361. $5.00 (cloth); $2.50 (paper).

The Effects of Federal Programs on Higher Education is the report of a study by the Brookings Institution (under the direction of the author) made at the request of the United States Office of Education. The study resulted from congressional interest in the impact of federal programs on higher education, expressed in Title X of the 1958 National Defense Education Act. This title requires the Secretary of Health, Education, and Welfare to obtain comprehensive information on federal programs at institutions of higher education and to develop policies and procedures "which will strengthen the educational programs and objectives of the institutions of higher education."

The study as designed examined the effects of federally supported *research* programs upon selected departments of natural science, social science, and humanities at thirty-six universities and colleges. (Effects of other important although less extensive support programs—housing, urban renewal, veterans' education, tax legislation, etc.—were not included.) Specifically, the inquiry centered upon three questions: (1) the effects of federal programs upon the quality of higher education, particularly at the undergraduate level, (2) the extent to which fuller use can or should be made of institutions not heavily involved in current federal programs, and (3) the experience of institutions with the administration of federal programs. An extensive questionnaire and interviews were utilized at thirty-six institutions: twelve universities receiving "relatively large sums" from the federal government ($4 million or more each during 1957–58); twelve others receiving "decidedly smaller" sums ($0.5 to $1.9 million each during 1957–58); and twelve liberal arts colleges (which received very much smaller sums). Thus, the study represents "a comparative investigation of three types of institutions and three types of liberal arts disciplines, chosen for their importance and the range of educational situations exemplified—not for their statistical representativeness."

Mountains of statistical data are presented in textual form, in tables (seventy, if our count is correct), and two appendixes, relieved throughout by an engaging writing style plus a forthright willingness to state views on the pertinent issues (a rarity in official, quasi-official, and educational literature). Summarized with reference to the three basic questions, the author believes that the findings show that federal support has had profound positive effects upon the quality of research and graduate training in the natural sciences, fewer but still beneficial effects upon the social sciences, and perhaps negligible effects upon the humanities. While not affecting the movement of quality students and faculty into science fields, the data reflect a concentration of such individuals in a few leading institutions. The colleges and undergraduate science education have suffered. The most unfortunate consequence, to Orlans, is the cleavage engendered by federal research support between the status and rewards of science faculties as compared to humanities faculties.

Regarding the difficult question of concentration of funds in a few leading institutions, this outcome of federal support is viewed as almost inevitable since it coincides with the concentration of high-quality faculty and students at these schools. There is ground for concern over the latter, and primary responsibility from government for broadening the distribution is felt to rest with the NSF, NIH, and the Office of Education. The most promising antidote appears to the author to lie in strengthening the major state universities without, at the same time, weakening the great private institutions.

The experiences of universities and colleges with the administration of federal programs, with few and minor exceptions, appears to be reasonably satisfactory. Strength is to be found in the project system, although various forms of broadened aid that are emerging are thought to be desirable as a way of alleviating the dangers inherent in faculty support through project grants. In the general realm of administration, Orlans sees potential danger in the failure of some educators to properly assess the interplay between academic values and goals and those of business components of educational institutions and political components of the federal government (a view that we share).

To the reviewer, the upshot of the Orlans study is succinctly reflected in the comment of one manuscript reader who observed that "it seems odd that you do not seem to find federal funds to be decisive at any point in instituting or terminating social trends in Academe. Do they always only either complement or complicate what is already going on there anyway?" Contrary to those who feel that the purity of higher education is somehow being compromised

by federal support, the reviewer agrees with this observation and with what seems to be the import of the findings—that no shortcomings or evils have been introduced into the higher learning by federally sponsored research programs that were not already there, either in incipient or virulent form, prior to the initiation of large-scale federal research support.

JOHN T. WILSON

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Behavioral Science and Civil Defense. Edited by GEORGE W. BAKER and LEONARD S. COTTRELL, JR. ("Disaster Study," No. 16; National Research Council Publication, No. 977.) Washington, D.C.: National Academy of Science, 1962. Pp. 169. $2.00.

The ten papers published in this volume were presented at a conference held in May, 1961. In Part I, three federal civil defense officers develop the case for a comprehensive civil defense program. Needless to say, their *published* remarks avoid treatment of the very political and social questions that social scientists might assemble to consider. The last sentence in this section illustrates the level of treatment: "We face many unanswered questions and many unsolved problems which provide challenges to a broad range of research skills."

Part II contains papers on "Policy and Decision Making" by Herman Kahn and Kenneth Boulding. Kahn's paper, essentially his testimony before a Congressional Committee, has appeared elsewhere in other versions. Kahn argues logically that "Civil defense does not increase our deterrence very much while it increases markedly our ability to survive a war if a war occurs and is fought by what might be called rational methods." Boulding argues imaginatively: "Concentration of effort on defensiveness . . . either is inimical to survival or, if it succeeds, succeeds only at a fantastically high cost in terms of the nature of the organism which is defended."

Parts III and IV contain a miscellany. Wilbert Moore, Arnold Feldman, and Charles Loomis offer fragments of social-system theories they have reported on more completely elsewhere. Psychologist Jum Nunally prescribes better ways of selling the public on fallout shelters, such as "Supply fewer gory details of what would happen in an atomic assault . . . present a more consistent front among experts," and the like.

If it were not for a running bibliography with comments by Ralph Garrett that reviews the relevant literature, one would finish this collection with the impression that behavioral scientists had nothing to say about civil defense. Happily, Garrett restores the belief that we have already contributed something of at least mild pertinence to the debate.

ROBERT A. DENTLER

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Intergroup Relations and Leadership: Approaches and Research in Industrial, Ethnic, Cultural and Political Areas. Edited by MUZAFER SHERIF. New York: John Wiley & Sons, 1962. Pp. 284.

In our present international circumstances, a volume containing contributions by fifteen social scientists on the problems of intergroup relations ought not to be just another collection of essays. In many ways, however, that is exactly what this book is. Typically, numerous key areas are omitted and the output is highly uneven.

The book is divided into three sections: theory, intersystem and intrasystem relations, and the effects of social change. The strengths of the volume are largely in the second section. Among the worthwhile fare here are: a penetrating analysis of union-management relations by Robert Dubin, an examination of the desegregation crises and their impact on Negro leadership by Lewis M. Killian, a study of the intimate relationship between intranational and international problems by Otto Klineberg, and Robert C. North's approach to the study of international conflict.

Conspicuous by their absence from this volume are studies of the military establishment and the role of the military in intergroup relations. This is hard to stomach when we realize that the two major international powers are pouring yearly over one-tenth of their total national income directly into defense. Likewise, we find no systematic comparative studies of political leaders, their background, values, beliefs, interests. It is significant that

the major theoretical contributions of the volume can be found not in the early chapters that self-consciously focused on abstract theory, but instead where there was a substantive problem clearly in mind, for example, in Robert Dubin's chapter cited above.

Still, this book could be used profitably as a source book for courses in the sociology of conflict, intergroup, or international relations. This may mean a small sale because there seem to be few such courses. We need more. There is enough significant material available now for intensive and profitable treatment of these areas. Such courses might well stimulate research that obviously is desirable, not *essential*. If this book can contribute to this end, it will not be just another collection.

OSCAR GRUSKY

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Traité de sociologie du travail. Edited by GEORGES FRIEDMANN and PIERRE NAVILLE, with the assistance of JEAN-RENÉ TRÉANTON. 2 vols. Paris: Armand Colin, 1962. Vol. I, pp. 472, New Fr. 37; Vol. II, pp. 448, New Fr. 36.

These two volumes, consisting of twenty-five rich chapters written by twenty-four men and women already established or emerging in the growing field of industrial and occupational sociology, are not only the first attempt in France at a synthesis in this field; they are probably the first such attempt among all the countries now embarrassed by the riches of social science.

The several chapters are organized in six parts, comprising "Definitions and Methods," "Industry," "Population and Employment," "Technology" (not so entitled but implied by the chapters therein), "The Enterprise," "Values and Attitudes," and "Work and Industrial Civilization." This sort of classification alone is a contribution in the light of the jungle of researches and essays one encounters when trying to keep up with the literature in this "specialization." But such has always been the genius of Georges Friedmann and his colleagues—including Pierre Naville, the chief co-editor of these two treatises, and Jean-René Tréanton, who has labored long in the vineyards of France's academic industry.

Obviously, a work of this nature is not conducive to a brief review that would hope to capture a single central theme running through twenty-five chapters. One can only hope that selected chapters will soon be translated for general use in the English-speaking world. Naville's section on methods deserves careful consideration: this reviewer is not aware of any American text in industrial sociology that deals specifically with this problem. Alain Girard's chapter on training bemoans the fact that this subject has been relegated to pedagogy and typically neglected by industrial sociology and by economics—a neglect certainly revealed by America's sudden but late recognition of the disequilibrium between its occupational needs and its educational programs. The chapters on technology show Naville at his best—which is only natural since this subject is his forte—empirically as well as theoretically. It is refreshing to see some recognition of the role of the social factor as an independent variable, as well as a dependent one, in the evolution of technology.

The chapter on morale and job satisfaction by Jacqueline Frisch-Gauthier is but one example of wide acquaintance with studies conducted outside France, a trait that enhances the analytical abilities of the typical French social scientist. Her interpretive statement on morale is a specific manifestation of this reliance on a wide reading. Another example is to be found in Michel Crozier's chapter on the sociology of unionism, in which he distinguishes five complementary viewpoints on this topic: genetic, structural, functional, ideological, and dynamic (*le point de vue de changement*). A third example is in Tréanton's essay on strikes.

It is unlikely that a chapter on labor law and social security would be found in an American handbook on industrial sociology, but Yves Delamotte (Institut des Sciences Sociales du Travail) devotes in this one nearly thirty pages, pointing out the ways in which sociologists might find valuable sources of research and theoretical data in the statistics and behavior relating to these subjects.

A great deal of original research and thinking is presented in the contributions by Alain Touraine and Bernard Mottez on mobility and worker attitudes. These two authors are careful to distinguish between individual and group mobility, as well as the differences in working-class identification according to worker origins

at various stages of industrialization and also the nature of the "société globale."

In the final chapter, Friedmann attempts to draw from all that precedes in the two volumes the major trends and possible future developments concerning the significant transformations in the nature of work, the emergence of new structures in the labor force, the status of wage earners, and the shift from an emphasis on production to an emphasis on consumption in modern industrial societies. His creative contribution to industrial sociology lies in his taking an empirical fact or trend and seeing in it a signpost or manifestation of something more meaningful than the original datum itself.

His many observations relate to such as the following: the clear-cut diminution of manual tasks, with a corresponding increase in the symbolic content of work activities (the reading of dials and signals, for example); the disappearance of spontaneity in work; the possible obsolescence of the division of labor as we conceive of that term today, thus affecting the very notion of skills. Friedmann notes the declining proportion of manual workers that lies at the base of the "crisis" of labor unions and working-class parties in the great industrial nations of the West and which raises the question of alliances with the middle classes as evidenced, for example, by the transformations in the British Labor Party—and even, for the first time, among French unions. "What is important, in the view of these militants oriented toward new realities, is no longer the collective ownership of the means of production . . . but the power of decision, that is, the creation and the control of relationships between the employees and the managers of an enterprise." The change is from collectivization to decision-making.

But a different kind of conflict might possibly develop if one reads correctly Friedmann's discussion of alienation and the rise of a civilization of welfare (*bien-être*) or consumption: this could be the chronic conflict between the power group composed of all those who think, prepare, and administer the system —and the group who obey, who carry out the tasks in the shops, the offices, and the fields.

Finally, while classical and feudal societies adapted their educational systems to the image of man *exempt from work,* and while early and even contemporary industrial society cultivated a system devoted to man *at work,* the new emerging one must begin to add to the latter those educational institutions' methods and programs that are congruent with the needs of *man after work.* The secular trend toward shorter workdays and workweeks, longer vacations, and longer retirement years calls for a great mobilization of public and private resources to humanize our working and our leisure lives.

HAROLD L. SHEPPARD

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Transactions of the Fifth World Congress of Sociology. By INTERNATIONAL SOCIOLOGICAL ASSOCIATION. 2 vols. Louvain: International Sociological Association, 1962. Pp. x+254; ix+208.

These two volumes contain most but not all of the papers presented at plenary sessions of the International Sociological Association in Washington, D.C., last September. The many sociologists who were in attendance will recall that they dealt with three themes: (1) "The Sociologists, the Policy-Makers, and the Public," (2) "The Nature and Problems of Sociological Theory," and (3) "The Sociology of Development." Papers on the first two of these themes appear in Volume I of the *Transactions;* and those on the third theme comprise Volume II.

The ten authors on the first of these themes came to different conclusions about the possibility of utilizing sociology in the formation of policy, and one detects the old dilemmas of neutrality versus engagement, of pure versus applied science, and of scientific versus public definition of social problems. Professor Anton Vratusa, of Belgrade, struggles valiantly with the first of these, and on the second and third Everett Hughes contributes one of his most perceptive and penetrating pieces.

The section on sociological theory covers a fairly wide spectrum, dealing as it does with sociological concepts, Marxism, positivism, and the historical method, in papers written, respectively, by Ernest Gellner, Henri Lefebvre, A. K. Saran, and P. A. Sorokin. René Koenig has a thoughtful introductory essay.

The papers in Volume II, on economic development, are devoted both to the early stages of growth and to the maintenance of growth.

They treat another wide variety of relevant subjects and point, in one way or another, to the problem of constructing a general theory of development. There are eleven contributors and ten papers.

The *Transactions* are incomplete in that they lack not only the full complement of plenary papers but also the papers delivered at other than plenary sessions and the commentaries in both cases. Taken all together they fail to reflect a single or coherent view of sociology in the world today, but as a record of a successful congress they are indispensable.

ROBERT BIERSTEDT

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Regionalization and Rural Health Care. By WALTER J. MCNERNEY and DONALD C. RIEDEL with the assistance of DARWIN O. FINKBEINER and EDWARD M. DOLINSKY. Ann Arbor: Graduate School of Business Administration, Bureau of Hospital Administration, University of Michigan, 1962. Pp. xi+209. $5.00.

For those interested in health organization, this is a book of considerable importance. In it the new head of the National Blue Cross, a health administrator and educator by background (McNerney), and the new director of research of the organization, a sociologist (Riedel), lay out the social and economic conditions related to the relative failure of three small Michigan towns to implement regionalization between their health centers and two base hospitals even though aid was provided by the Kellogg Foundation. As the authors point out, this problem has implications for the efficient and effective operation of many systems--prepayment health care plans, consolidation of schools, regionalization of libraries, farm co-operatives, etc.

The authors avoid a formal definition of regionalization and may thereby have missed greater generality that would have interested all sociologists. How does regionalization differ from co-operation? From co-ordination? There is, in fact, a disappointing lack of attention to sociological theory both in the formulation of the problem and in the interpretation of the results. While this is the case, the reviewer finds the study of theoretical as well as practical interest, since it presents a clear picture based on empirical study that the reader can use to ask theoretical questions.

For example, is differentiation and co-ordination of elements to achieve a common purpose in a geographic area (our notion of regionalization) different from such differentiation and co-ordination within a complex organization or some unit other than one based on geographic area? Even more important theoretically, perhaps, what is the effect of introducing new identity centers such as "our home town" into systems of co-ordination? Is the establishment of regionalization different in its mechanisms and processes from the insertion of any new organization into "the stream of history"? In an excellent closing chapter, the authors raise a number of questions that have theoretical and practical import. For example, is regionalization more effective under voluntary or governmental auspices? Is it more effective under one or more systems of authority? Clearly this experience has suggested to the authors that even if regionalization is agreed upon as valuable, it will occur with difficulty under the present voluntary system. They recognize the need for careful further study of the factors fostering and retarding the implementation of regionalization plans.

As for methods and analysis, the authors seem to have made the best out of a not-too-perfect situation over which they may have had only limited control. Instead of having clear successes to compare with clear failures, all three cases must be considered failures in that they have not continued, though one, the Kalkaska–Traverse City affiliation, was more successful during the period of formal agreement than either St. Ignace–Petoskey or Onaway-Petoskey. Many peculiar aspects of the relatively successful case might have received attention instead of placing the whole weight of the difference on (1) the lack of an alternative base hospital for Kalkaska and (2) the Kalkaska physician's greater acceptance by the base hospital staff. From other comments in the book (e.g., the Kalkaska Health Center's co-ordination with public health programs in the town, etc.) one can speculate that a major factor may have been that this town had more of what Kaufman calls "communityness."

There are other minor problems with the methods and analysis. The samples of health organization respondents and community leaders were not carefully drawn, though the general community samples seem to have been well chosen. Few within-community comparisons were made, although the data allowed of them.

One interesting finding of this sort, however, was that although people in these small towns generally were reluctant to go outside for medical care, professional, managerial, and clerical families were more likely to go to outside hospitals and other care facilities both before and after the health centers were constructed.

In general, the book is important and well done for what the authors had to work with. It examines formal arrangement as well as informal relations and attitudes. The book includes a lengthy bibliography.

RAY H. ELLING

Cornell University

The Southern Appalachian Region: A Survey. Edited by THOMAS R. FORD. Lexington: University of Kentucky Press, 1962. Pp. xv+308. $10.00.

The Southern Appalachian Region has persistently lagged behind the economic and social developments of the rest of the nation. Using an earlier study of the region (1935) as a bench mark, this volume examines the area's current status, problems, outlook, and offers some practical solutions. The articles were commissioned for this volume, and there is considerably more continuity than one normally encounters in a book in which only two chapters were written by the same author. The volume is virtually an encyclopedia of the region—running the gamut from topography to singing games. In addition to an introductory section and conclusions, there are at least three chapters on each of the following: population, economy, institutions, and folk arts.

In one of the more noteworthy chapters, Ford uses survey data to provide an analysis of the decline in "provincial" attitudes within the region that drives home the point that the old stereotypes about the area no longer hold. Belcher reviews the early settlement of the region and more recent population developments. Brown and Hillery, handicapped by the limited data available to them, survey migration from the region during recent decades. The unfortunate position of agriculture is analyzed by Proctor and White, who also find a limited future for agriculture in the region. Gibbard describes the restricted employment outlook in the coal industry as well as the problems facing the forestry industry in his study of resource-based industries. Morris' assessment of the tourist industry recognizes the severe limitations of what tourism can do for the area, although he shows some possibilities for improvement.

Several chapters on the region's institutions are very useful for understanding the problems faced. Wager provides an excellent examination of the malfunctions and handicaps of local government. The chapters on planning agencies (Gray), education (Graff), health (Hamilton), and welfare services (Cole) all survey the current situation and the steps necessary to raise the region's level. Vance's concluding chapter provides a realistic appraisal of the outlook for the Southern Appalachians as well as a bold program for the future that would involve both training people for migration and concentrating the development of industries within the urban areas of the region. It would encompass considerable assistance from the national government.

Given the widespread acceptance of outmigration as one of the major solutions to the region's problems, I was disappointed to find relatively little analysis of difficulties faced by migrants from the area in their new settlements. Griffin's discussion of migrants in Cincinnati is primarily concerned with their participation rates. Although the chapter on manufacturing by Quittmeyer and Thompson reports results from a survey of executives' attitudes toward location of plants in the region as well as examines manufacturing trends in the region, the discussion is altogether too brief when one considers the significance of manufacturing for the region's future. The authors, incidentally, are rather too glib in their rejection of such concepts as labor- and market-oriented manufactures as well as weight-gaining and -losing industries.

There is hardly any discussion of the financial credit system of the region and the ability to provide capital for local entrepreneurs and small industry. Similarly, as a number of articles point to the significance of state and federal funds for many activities in the region and because the outlook for future developments outlined by Vance seems to hinge on projects supported by federal funds, one would think the ability of the region and its congressional delegation to gain support for such projects would be no trivial consideration.

STANLEY LIEBERSON

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Labor in Developing Economies. Edited by WALTER GALENSON. Berkeley: University of California Press, 1962. Pp. x+299. $6.00.

This book provides useful material on labor organization in Pakistan (Weatherford); Indonesia (Hawkins); Brazil, Argentina, and Chile (Alexander); Israel (Sobel); and Turkey (Rosen). These countries are representative of the underdeveloped world, except Africa; they include a wide range of cultures. The writers are all labor economists who have apparently spent some time in the countries of which they write, and who seem in each case to have mastered the language spoken there. Their descriptions of the organization of labor are serious and thorough.

In three countries employers—private or public—have been much stronger than unions. In the face of employer opposition and government unconcern unions flagged in Pakistan after partition. The Novo Estado of Vargas was able to assimilate the unions into the state apparatus of Brazil so that they became social welfare bodies for their members without maintaining independent bargaining functions. The étatism of the Turkey that emerged after World War I under the auspices of a single modernizing party that was itself the chief employer of industrial labor circumscribed its unions to insure that they would not check the pace of economic development.

On the other hand, Indonesian trade unions got a good start with the revolution in which they took a considerable part and added to their power by assimilating to political parties. Foreign employers were no match for them, and after suffering many exactions the great majority have left the country. It remains to be seen, as Galenson observes, whether the unions will have the same governmental encouragement in the future when their chief opponents will be Indonesian employers, government and private, and the armed forces. The Histadrut of Israel resembles the Indonesian unions in having had a part in the struggle for independence, but differs in being solidly united and in having an active industrial wing that has engaged in building construction, bus transport, and several branches of manufacturing. At one time the Histadrut seemed likely to become coterminous with the state, but more recently its activity has diminished. Perhaps only in Chile have unions faced employers in the bargaining process familiar in developed countries, with a minimum of government interference, the two opposed parties being of approximately equal strength. But even here the main union is Communist, and the unions have become strong enough to generate a rapid inflation, so that the government is increasingly having to step in to provide needed protection to employers and the public.

Economists used to be hard-headed. When they discussed social security and minimum wages for industrial workers, they were interested in who was to pay for these, whether employers, foreigners, or peasants. Their preoccupation with trade-union pressure on wages in circumstances where inflation was caused by such pressure made them ask whether real wages over the long period were higher as a result of trade-union activity than they would have been if there were no unions. The writers of this book either do not ask such questions at all or leave them unanswered. Does their silence imply an opinion that the formation of unions at all costs is a condition for economic development? But in that case something has changed drastically: During the time of initial growth of the presently developed countries unions were unlawful and subject to the same sort of prosecution as any other conspiracy against management. In the U.S.S.R. unions were and are part of the *encadrement* of the country by the Communist party, and their function is to see to the energetic delivery of labor, not its withholding.

Where the effect of the unions has been destructive of immediate production, they are sometimes defended as doing good in other ways—educating the raw labor force and stabilizing it. This argument appears in Galenson's Introduction, where the Argentine unions are conceded to have "probably raised labor costs above what they otherwise would have been, and thus hindered development," but they have also "tended to assure the worker that he will not be considered merely a 'factor of production,' but will be treated as an individual, with certain prescribed rights and duties." Galenson adds that "the resultant gain in social stability may more than offset the possible loss in investment" (p. 5). What is this "gain in social stability" which is said to characterize the Argentine? The Argentinians themselves are the first to declare that there has not been a single month during the past ten years without its armed revolt attempted or seriously contemplated, and that the demagogy of Perón, still working through the unions he captured and

indoctrinated before 1955, has been a major factor in these uprisings.

But the reader who turns to this book for a first-hand factual record of how labor is organized in seven countries will not be disappointed.

NATHAN KEYFITZ

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Blossoms in the Dust: The Human Factor in Indian Development. By KUSUM NAIR. New York: Frederick A. Praeger, Inc., 1962. Pp. 201. $4.00.

There is an exhortative poetry whose enthusiasms envelop and romanticize the efforts of underdeveloped countries to achieve a rapid rate of economic growth. Its ubiquity attests to its many services. To the cynic this poetry may serve only the purpose of legitimizing the exercise of governmental power by a small elite who claim to serve as caretakers acting on behalf of the interests of the masses. From a less jaundiced viewpoint, the poetry seems a necessary means of mobilizing consensus and the will to change throughout the society. One of its chief weaknesses, however, is that it tends to equate a statement of intentions with accomplishment, and thus forestalls the creation of the societal infrastructure necessary to implement schemes. It diverts attention from the tasks of technological and procedural innovation that must accompany cultural borrowing and directed social change. And the mechanistic, almost technocratic, folkways of centralized economic planning do little to overcome this myopia.

If the poetry is persuasive enough, one can ignore for long periods of time the kind of incongruity between plans for rural change drawn up by benevolent, urban-based, Westernized elites and the consequences of the resulting governmental programs among the unheeding, largely subsistence-oriented rural populace. Kusum Nair's book is an eloquent account of how great that incongruity can become, of how misleading the poetry can be. This gifted journalist traveled the length of India gathering the peasant's-eye view of the various programs for improvement, continually testing schemes against the payoff variable of agricultural productivity. The answers she got, told in the form of a series of village vignettes, comprise a useful casebook on how difficult and perverse externally planned and initiated rural change can be.

The structure of the book is deceptively simple—it is a collection of short case studies drawn from different villages in thirteen Indian states and two territories. Background data are sequestered in footnotes. In the course of the narrative, however, the Community Development Program, land reforms, co-operative societies, irrigation schemes, seed and fertilizer distribution, cottage industries, abolition of untouchability, new village democratic institutions, subsidized housing, legislation for the protection of the peasants, minimum wage laws, and many other programs are made to appear at best quixotic and at worst added burdens on the already encumbered peasantry.

A few examples will suffice. In a Madras village the governmental edict aimed at raising the proportion of the produce going to the sharecropping tenant was viewed by the tenants as a windfall and lessened their desire to increase their productivity. In Kerala well-constructed government-subsidized houses lay idle in an area of housing shortage because they did not provide the privacy of a house on one's own site in a rural setting. Again in Kerala a landlord reporting on the effects of the minimum-wage law for agricultural labor related: "My workers are still working for me as before on the same terms. They did ask for more. So I gave them the minimum wage but withdrew the other facilities—clothes, loans and gifts on births, deaths and festivals. Then they came back to me of their own accord and told me and the local revenue official that they wanted to continue the old system." In Andhra the effect of tenancy legislation was the dispossession of the tenants and their conversion into landless agricultural labor. Ceilings on the size of individual holdings were circumvented by subdividing the property among collateral family members. In the area of the famous Etawah pilot project of Uttar Pradesh, peasants reported that the benefits of development went only to the wealthy, that the co-operatives took but did not give and distributed favors according to status, not need. In eastern Uttar Pradesh, Gandhi's spinning wheel, the government-sponsored Ambar Charka, was found to be too heavy and burdensome to operate and paid too small a return on the labor. The construction of roads and school buildings through semivoluntary contribution of labor, *sramdan*, promoted initial construction but not maintenance. And so on.

Not all the accounts are so gloomy. There

are sharp regional variations, and within the same area or even the same village some groups had high agricultural productivity and willingness to adopt new crops and practices while others remained immersed in ritualized inefficiency. Contrasting the few successful with the unsuccessful examples, Kusum Nair tried to correlate these differences with various objective factors such as size of holding, fertility of the soil, amount of water, landholding system, and intensity of governmental program inputs, but none of them consistently explained the variations. Almost by default, she found the key variables to be subjective, especially the aspiration level, and this she found to be distributed not geographically, but by caste.

From the technical viewpoint, this book is methodologically naked and without a sociological concept in its pages. On the crucial question of the representativeness of the sample we have only the authoress' assurances that she came to know the countryside well enough so that her selection from her voluminous field notes conveys the essence of each local situation. This approach, of course, prevents her from answering the crucial questions of aggregation: What proportion of all areas are successful, and if she likes, high aspiration areas? Has the proportion improved over the time period of governmental planning? And leaving governmental programs aside, what is the direction and the extent of change in the rural standard of living? Nor does she in this book face the question of what caused the differences in aspiration levels historically, and are they at all manipulable by any concerted governmental effort?

While the author does not allow herself the pleasures of the advisory role in this book (she intends to undertake it in a sequel volume), elsewhere she has indicated that she believes that the government must shed its habit of dealing in programs that are geographically oriented and abandon its programs that, under an egalitarian ethic, spread the available resources evenly among the efficient and inefficient alike. Rather, they should squeeze out the low-productivity farmers and concentrate investment on the currently more productive, even if this calls for the recognition and tacit support of caste differences. Whether this is politically feasible and what will happen to the millions of potential displacees is not clear. Above all she pleads for a system whereby the administering bureaucracy will make its feedback a realistic assessment and not a reporting of what underlings believe the top echelon wants to hear. Meanwhile she recommends that all operating personnel be trained in sociology and psychology. This last suggestion seems admirable on general principles but, with due deference to our applied branches, I am not at all sure that this would help matters.

RICHARD D. LAMBERT

University of Pennsylvania

Divorce and Fertility: An African Study. By EDWIN ARDENER. ("Nigerian Social and Economic Studies," No. 3.) New York: Oxford University Press, 1962. Pp. xvi+171. $3.20 (paper).

The Bakweri, a people of some 16,000 total population in West Cameroon, were reputed to have a high degree of marital instability, concubinage, and prostitution, and a low fertility rate. This monograph reports on an interview survey of 1,062 Bakweri women, representing virtually all adult females in three villages. Complete cohabitation and pregnancy histories were obtained for each woman, although accurate dating of events was not possible. The data showed that divorce and separation were far more frequent than death of partner as a source of dissolution of conjugal unions. Women had participated in an average of 2.1 unions, including concubinages and connected periods of casual prostitution. There was no evidence that marital instability rates had changed significantly during this century, or that concubinages were less stable than formalized marriages. Estimated fertility rates (presented in an Appendix by N. H. Carrier) were as low as in any African study to date.

Of the variables studied, rank of the wife in a polygynous union was most strongly related to conjugal instability, with junior wives having a higher likelihood of divorce or separation than senior wives. Most of the women were adherents to Christian missions, and those unions involving a pagan partner showed slightly higher instability, as did those with partners in unskilled employment. Many of the slight associations may be subsumed under "lack of maintenance," often cited by the women themselves as a cause of divorce.

Within their territory, the Bakweri are outnumbered by immigrants, mainly males working

on plantations. In the introductory and concluding sections of this monograph, it is argued that the high sex ratio (about 200 males per 100 females), together with other effects of immigration, is the basic cause of the marital instability among the Bakweri. "There are some situations in which certain crude demographic facts (for example, an overwhelmingly high male sex ratio due to immigration) become such important influences on social patterns that the study of their effects becomes the most fruitful single means of comprehension of the sociology of those situations. . . . It may be suspected perhaps that in some societies interpreted through institutional and value systems alone, without detailed attention to demographic factors, anthropologists may have sometimes attributed to the fly the momentum of the chariot wheel" (pp. vi–vii).

This important thesis is argued in an earlier monograph on the Bakweri and does not receive sufficient attention here. Rather the interview data are presented in a series of forty-four simple cross-tabulations, and each table is discussed in turn, albeit briefly and carefully. The tabulations were apparently designed in advance of data collection, and there was no attempt to pursue any of the associations. The data from the three villages are combined, and there is no systematic effort to relate any of the findings to the abnormal sex ratio.

The care and sophistication in the design and conduct of the demographic interviews and in the routine analysis of the data is a heartening sign in the rapidly developing field of African demography. The distance between the anthropologist and his survey data and his inability to effect an analytical merger between statistical data and "anthropological" data reflect a gap that has yet to be filled.

KARL E. TAEUBER

Population Research and Training Center University of Chicago

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Kinship and Community in Carriacou. By M. G. SMITH. New Haven, Conn.: Yale University Press, 1962. Pp. xiv+347. $6.00.

Carriacou is a tiny island of roughly 7,000 people, located about one hundred miles northeast of Trinidad and belonging to the Creole culture province of the Caribbean. Despite its small size, Carriacou is of special interest because it differs from other West Indian populations in several crucial respects. It is remarkably homogeneous in culture and social structure, has been free from direct elite influence for the past one hundred years, and hence is in a position of isolation in which to develop and systematize its own culture. What makes it of special theoretical interest is the fact that while Carriacou folk are descendants of slaves as other West Indian populations are, they have a well-regulated kinship system with clearly defined rules of mating, parenthood, and family organization, indicating the error of attributing the weakly organized family and kinship systems of other British West Indian populations to the continuing influence of slavery. What makes the island society of even greater interest is the fact that Carriacou has had a marked pattern of out-migration of males from the mid-nineteenth century. Although its demographic conditions represent the severest strain on a family system of any in the British West Indies, it is precisely under these conditions that Smith has found the most clearly organized family structure in this culture area, suggesting that the causes of familial disorganization in other West Indian societies are linked to the domination of the culturally dissimilar political, economic, and social elite.

Of the book's many merits, two call for special mention. The first is the inductive approach Smith uses in his analysis of kinship structure. This is no "ideal" portrait of Carriacou social and normative structure so frequently found in social anthropological literature. Smith starts with an essentially quantitative description of Carriacou mating, showing how the domestic organization is linked to the mating system. From this analysis, he extracts the principles that govern domestic organization. It is possible, therefore, for a reader to trace each step in Smith's emerging profile of the relations between mating, family organization, and lineage patterns.

A second contribution of the volume is the well-founded basis it gives to Smith's overly cautious suggestion that West Indian family organization should not be too facilely labeled "matrifocal." Carriacou households "seem" matrifocal (e.g., three of five homes have female household heads [p. 249]), but in marriages the husband-father dominates them, and in extra-residential mating, the offspring belong *to* the father's blood, not the mother's. Smith's analysis demonstrates the importance of focusing on

the mating forms and regulations rather than the household organization in an analysis of West Indian family systems: "in short, the differing forms of domestic group and the differing positions of women within them are both effects of alternative mating patterns; because this is so, many households under female heads have never contained elementary families and cannot therefore be 'explained' by hypotheses about the developmental cycle of such units" (p. 312).

Although the quantitative underpinning to the analysis of Carriacou is one of the book's merits, it is also in one sense a shortcoming. At several points the text and the tables are needlessly cumbersome and overly detailed because the author chose to present the material largely in raw figures rather than percentages or ratios. The text would read far more smoothly if the latter were adopted more consistently, particularly in chapters viii and x and the Appendixes.

A second shortcoming is perhaps more likely to be found by sociologists than by social anthropologists. After the introductory chapters outlining the abnormal demographic characteristics of Carriacou and its strong family and lineage system, this reader at least looked for an account of how the society remains so stable in the face of the long history of out-migration of males. With such an emigration pattern, Carriacou cannot be said to be "isolated," for conceivably such returning migrants could be the carriers of new ideas or techniques. Smith gives us a good account of why men leave the island in the first place: social and economic independence and marriage require considerable outlays of money and material, and the island provides very little local employment. Hence emigration is the normal way in which men achieve economic and social independence (pp. 104, 123–24). One gathers, furthermore, that such returning migrants have not been the carriers of change in Carriacou. If we raise the question "Why haven't they been?" we are given only scattered hints in Smith's book. It is suggested that the strength of the patrilineages to both reward and punish returning men has effectively curtailed change in the society, and patrilineal values motivate the proper discharge of paternal roles, which, were they absent, could lead rapidly to a breakdown in the presently stable kinship system (p. 307). But how does this operate? What happens to the migrants during their years away from home? What differentiates the men who return from those who do not? Is there any "brainwashing" process whereby the returnees are reabsorbed into the culture? That some change takes place in these men is hinted: thus, to illustrate the cultural approval of productive investment, Smith mentions the local disapproval of squandering of money in gambling and "show" by young migrants returning on holiday (p. 57), and later he mentions that the returnees are "quietly made to observe local customs and conventions if they wish to remain on the island" (p. 71). But are such men ever "expelled" from the society? And what is it about Carriacou that brings the men back? Is it the security of the stable kin relationships? The absence of domination by any elite? The prestige and honor awaiting a man when he becomes a household head or lineage head, an honor he could not hope to experience in Trinidad or Venezuela? Questions of this order remain unanswered by the book. Precisely because of the unique features of stability in Carriacou kinship structure, I hope someone will return to the island to study these emigrants and returnees.

ALICE S. ROSSI

University of Chicago

Aspects of Religion in Indian Society. Edited by L. P. VIDYARTHI. Meerut, India: Kedar Nath Ram Nath, 1961. Pp. 410. Rs. 18.

The first thing to impress the reader about this volume of twenty-one essays is its range. It includes tribals, peasant village communities, castes in urban setting, Hinduism in general, Sikhism, and Islam. There is also a brief but highly suggestive essay by Marriott on "Changing Channels of Cultural Transmission in Indian Civilization," in which he proposes that the civilization of India has been changing in recent years "from a rich old cultural mansion of many levels and varied styles into a more uniform structure comprising a few levels of standard designs." In another general essay Aiyappan recommends his own transformation from a rationalist skeptic to an admirer of spiritual attainments of some notable Hindu Yogis, as a means for enlarging and deepening anthropological understanding of religious behavior through "sympathy" and "empathy."

The main merit of the book is its wealth of concrete data, often based on competent first-hand field experience. Many of the writers, taking off from the earlier leads of Srinivas

(Sanskritization, spread), Redfield (orthogenetic and heterogenetic civilization), and Marriott (parochialization and universalization), explore further the nature of religious tradition on the basis of locality, ethnic affiliation, and caste stratification. Sects and subsects become the focus of attention in a number of essays. One would welcome Sharma's finding that vegetarianism in North India is mainly based on sectarian affiliation rather than on Sanskritic Hinduism as such. Singh brings out into relief how the earlier strata of Hindu and Muslim beliefs persist in Sikhism over the centuries through the process of retention and rationalization. Space does not permit mentioning the highlights of many other papers.

The editor Vidyarthi certainly deserves credit for organizing this feast of fresh, concrete material on religion in India that he appropriately dedicates to his mentor, the late D. N. Majumdar, one of the pioneers of Indian anthropology. One would only wish that he had exerted greater editorial control over the first-draftish style in which many of the articles, including that of the reviewer, have been presented in this volume. Most of the essays are obviously amenable to more orderly compression as well as more meaningful elaboration.

SURAJIT SINHA

Duke University

Sects and Society: A Sociological Study of the Elim Tabernacle, Christian Science, and Christadelphians. By BRYAN R. WILSON. Berkeley: University of California Press, 1961. Pp. 397. $5.75.

This book is an important contribution to the extensive literature on sectarian religious groups. It reports in clear and precise fashion the results of prolonged work as a participant observer of three quite different modern sects: Elim (Pentecostal), Christian Science (semignostic), and the Christadelphians (premillenarian). Though these three groups have much in common, as in their reliance on the Bible, their hatred of the Roman church, and the certainty of each that it has "the Truth," there is still sufficient difference among them to allow illuminating contrasts, in contradistinction to the usual tendency to generalize from a single group or to lump all such groups together as an undifferentiated phenomenon.

Mr. Wilson analyzes each group in turn in terms of its religious teachings, history, organization, social teachings, and social composition. Along the way he also clarifies such important questions as schism, ecumenism, relations with "the world," recruitment, leadership and charismatic power, and education. This multiple approach to each group naturally causes some overlapping in the report.

The author comes to the conclusion that he has been dealing with *established* sects, in contrast to the usual emphasis on the dynamic trend of sectarian groups toward denominational status. The groups under study appear to have been influenced by the comparative homogeneity and stability of British society, as over against the greater mobility of American religious and social groups. Two of the groups (Elim and Christadelphians) are located for the most part in Great Britain, and Christian Science is well represented there.

Accordingly, Wilson has wrested from the Pentecostals in Britain more theological substance than is displayed by their counterparts in America. He also has gained acute and subtle understanding of the fundamental teachings of Christian Science and the Christadelphians. He rules out a rigorous economic or social determinism and is impressed by the fact of "totalitarianism at the ideological level." He leaves still debatable the question whether religious teachings and the composition of sectarian groups *reflect* economic factors, or whether the teachings themselves exert a highly selective influence in relation to such factors. He demonstrates that both processes are at work simultaneously.

Wilson also commands an extensive knowledge of the far-ranging literature on contemporary sects, and he appends an excellent Bibliography. His own studies have led him to modify the classical theories of Troeltsch and Richard Niebuhr in very important respects.

The book is perhaps weakest in its study of the social composition of the various religious groups. Indirect evidence is admittedly used on occasion, but the conclusions are probably correct. Statistical evidence is used rather extensively, but the author is uncritical of the notoriously unreliable *United States Religious Census Reports* of 1906, 1916, and 1926.

The volume in itself is an admirable example of bookmaking. This reader did not find a single typographical error in nearly 400 pages of text, and only one insignificant error in citation.

Wilson's book should take its place immediately among the classics in the sociology of religion.

LISTON POPE

Yale University

The Oedipus Complex: Cross-Cultural Evidence. By WILLIAM N. STEPHENS. New York: Free Press of Glencoe, 1962. Pp. x+273. $6.00.

Stephens advances the thesis that many sexual fears in primitive societies, as well as the cultural adjustments made to them, can be traced to family arrangements that dilute marital ties between husband and wife. The *post partum* sexual taboo is for him a prime example of this kind of arrangement. Primitive societies, he notes, can easily be dichotomized by the length of time these prohibitions are in force. Either they are brief, or else they continue for a year and a half or more—usually not coming to an end until the child is weaned. When enforced for a protracted period, the taboo has the effect of intensifying those erotic elements psychoanalytic theory posits as inherent in the mother-son relationship. According to Stephens, the mother gains an outlet for her frustrated sexual energies by displacing them to the infant male, which in turn has the effect of significantly intensifying his sexual interests in her and, thus, of heightening the level of Oedipal strivings in the society. This, he further hypothesizes, has the consequence of generating sex anxieties in the male that persist into adulthood and become institutionalized as taboo and avoidance practices or take on more indirect ceremonial form.

Stephens does not endeavor to test the entire deductive chain of this Oedipal complex hypothesis but concentrates instead on only the beginning and end. Using ethnographic material from the Human Relations Area Files, he assesses the relationship between duration of the *post partum* sexual taboo and a wide assortment of customs that presumably reflect adult preoccupation with Oedipal problems. Included among these are "menstrual taboos, kin-avoidances, initiation ceremonies, totemism, sorcery, as well as a variety of fearful superstitions and restrictions pertaining to sex." The contingency coefficients yielded by this analysis are all positive, ranging from .24 to .37, and are with but one or two exceptions statistically significant. This Stephens interprets as evidence of the proximate validity of his hypothesis and the psychoanalytic assumptions in which it is couched.

Despite its simplicity of theme, this turns out in many ways to be a perplexing book—one that engenders confusion rather than resolves it. Part of the difficulty lies in the elaboration of the Oedipal complex hypothesis, itself. Stephens departs from theoretical precedent by discounting the Oedipal rivalry between father and son as a relevant consideration. By so doing he creates a logical hiatus; for then there is no basis for postulating that Oedipal strivings generate lasting sex fears. Stephens evades the issue by postulating the existence of these fears on the basis of psychoanalytic literature on castration anxiety. This is no solution at all, since the literature cited invariably attributes a prominent and critical role to Oedipal rivalry.

Matters are made more confusing by the fact that the same *post partum* sexual taboo Stephens uses for an independent variable was used four years previously by Whiting and his colleagues as an empirical indicator of cultural arrangements that give rise to Oedipal rivalry. This being the case, it would seem necessary that some effort be made to reconcile these differences in conceptual viewpoints before Stephens can feel justified in claiming even the proximate validity of his own hypothesis. This step has not been taken. Instead, the mediating factors are treated as "unmeasured intervening variables." To add further to the reader's confusion, data on Oedipal rivalry are introduced at various parts of the report. Sometimes this is done with the implication that the assumption of Oedipal rivalry is untenable; other times, however, the implication is that the assumption of Oedipal rivalry adds meaning to the hypothesis being elaborated. It is, as a result, impossible for the reader to discern just what Oedipal complex hypothesis is being offered for test and equally difficult to ascertain what implications may be drawn from the correlation data presented.

ROBERT A. ELLIS

University of Oregon

Living with Mental Illness. By ENID MILLS. New York: Humanities Press, 1962. Pp. xii+184. $5.50.

British studies in social psychology typically have been narrow in scope but precisely and carefully executed. Mills's broad-spectrum in-

vestigation of patients from East London, however, reflects the methodological tradition of her associates in the Institute of Community Studies rather than the research approach of her colleagues in social psychiatry. In a short book she trys to describe and analyze the hospital, familial, and community experiences of patients, the attitudes of patients and their families toward mental illness and mental hospitals, and the use of community health and welfare services.

Mills's study, or at least the report of it, is superficial at many points. For the most part, she treats her variables one at a time on a descriptive level, with little attempt to interrelate measures. Much of her description is consistent with the prevailing stereotypes and impressions of the behavior, treatment, and fate of the mentally ill. Previous British and American studies have reported similar findings, usually in more detail and depth. Her study documents the trauma associated with hospital admission, the impersonal character of the large mental hospital, and the fear of and hostility toward the hospital of patients and their relatives.

The second part of her book, on community services for mental patients, will be of interest to social scientists in this country who are engaged in mental-health research. It describes in some detail the health and welfare services in the community and their use by mental patients. The study was undertaken just prior to marked modifications in the legislative code and the administrative policies that regulate the care of mental patients in England. Her description of the health and welfare services consequently has historical value in understanding reports of past and current treatment programs in Britain. In the final chapter, Mills pleads for increased community services and greater continuity of care for the mental patient. The programmatic suggestions in this chapter—unification of services, recognition of local ways of life, organization of patient groups, sheltered workshops, and community-located hospitals—have been suggested before. In brief, from cover to cover, persons with a reasonable familiarity with current mental-health research will find themselves wondering, quite correctly, whether or not they have read this book before.

HOWARD E. FREEMAN

Brandeis University

The Health of Older People: A Social Survey. By ETHEL SHANAS. Cambridge, Mass.: Harvard University Press, 1962. Pp. xii+250. $6.00.

This study reports on a nationwide probability sample of persons 65 years of age and older outside of institutions (plus a subgroup of persons 60–64 who seem to disappear from the analysis). Responses to questions on illness, medical care, employment, and income are presented and often compared to the results of other nationwide surveys that asked similar questions. A small amount of new information in these general areas is added; the major source of new information is in a chapter on the living arrangements of older people and their relations to their kin. In addition to interviews with the older persons in the initial sample, also interviewed were a group of persons named by the aged as persons who would help them in an emergency. Finally, some opinions and attitudes of a nationwide adult sample on questions pertaining to the aged and medical care are presented.

In spite of a fine methodological base, this study will attract the attention of only those persons who have a very keen desire to learn the simple frequency distributions of responses to a large number of questions concerning old people and some of their views on the world. There is no multivariate analysis of the data, with the result that the text consists of verbal descriptions of the frequency distribution of question 1, 2, 3, etc. In the absence of multivariate analysis the author occasionally applies her imagination, with distressing results. For example, old people were asked if they thought their health was good, fair, or poor and 52 per cent chose "good." Some 63 per cent agreed that "a person understands his own health better than most doctors do." These two items lead the author to conclude: "Since their health is 'good,' most older people use medical care and medical services only when they experience an acute illness" (p. 52). Why one would reach this conclusion without introducing some cross-tabulation using the income data, insurance coverage available, or the health condition of the respondents is unclear. Failure to exploit the data adequately is evident throughout the book. The absence of comparative age groups does not aid this type of study.

The generally elegant research design that allowed for interviews with both husbands and wives is not utilized, nor does the research focus

on exploring hypothetical relations among variables. The closest we get to a hypothesis is this sort of comment: "Most people think that old people are living alone but the data show they are not." The odd thing is that "most people" as revealed in the separate adult sample reported late in the book do not believe any such thing, leading one to wonder if the book was written chapter by chapter, with little regard for over-all integration of the findings into the research process.

PHILLIPS CUTRIGHT

Social Security Administration

Social and Psychological Aspects of Aging. Edited by CLARK TIBBITTS and WILMA DONAHUE. New York: Columbia University Press, 1962. Pp. xviii+952. $20.00.

This thick volume includes more than one hundred papers presented at the Social Research Section of the Fifth International Congress of Gerontology in 1960. It gives a good cross-section of the present stage of social gerontology. As a self-conscious field of interest, social gerontology is quite new. It still lacks a unifying theory, and it is mainly treated by people who apply their main interest to the aged, not those who apply a body of knowledge called "gerontology" to a certain problem. This condition is shown by the lack of general treatises on gerontology written by one author, in contrast to a wealth of symposia by many authors in different fields, such as this one.

In subject matter it consists of seven sections: "Population and Social Organization"; "Economics of Aging"; "Housing, Family, and Social Relationships"; "Mental Health and Rehabilitation"; "Personality Theory, Attitudes, Roles, and Adjustment"; "Experimental Studies in the Psychology of Aging"; and a "Symposium on the Meaningful Use of Free Time." Within each of these sections the papers are quite heterogeneous. Some are hortatory pieces asking for consideration of certain problems of the aged; some are descriptions of conditions of the aged or governmental programs in different corners of the world; some are comparative studies of aging in different countries; some are progress reports of the principal studies in gerontology conducted at this time; and some are efforts at integrating our knowledge in some field of gerontology. Most of the pieces are valuable as a survey of different problems, studies, and approaches used in various countries and settings and give the interested reader further references to the more important studies done at this time. Most of the large-scale studies reported are conducted within the United States. The volume thus contains papers, in addition to those previously reported elsewhere, on the Kansas City study, the NORC medical expenditures studies, the work at the Langley Porter Clinic, the Duke Center for Aging, and the Cornell Study of Retirement Practices.

A few of the last classification of papers, the comparative studies and integrative pieces on general theory deserve special mention. Outstanding among the former is Brian Abel-Smith's piece on the relation of the philosophy of retirement pay and the amount of pension for dependent wives in different countries; among the integrative papers Clyde Kiser's discussion on the demographic meaning of aging of human populations, Irving Rosow's paper on the conditions and effects of age segregation, Peter Townsend's discussion of the purpose of institutionalization, and Harold Wilensky's general discussion of life-cycle, work situation, and participation in formal associations. These papers show an approach to a common ground of interpreting the aging process. The general direction seems to be an acceptance of some voluntary disengagement by the aged in their relation to a society that either neglects the aged person for his failure to conform to the standards of the younger population or helps him meet these standards. The volume thus reflects faithfully the present fragmentary state of the field and possible directions for unified treatments.

The excessively high price of the book deserves mention. In spite of the high cost of production, the editors included abstracts of papers given in the seminar before the congress that will be collected in another volume edited by the same authors. Avoidance of proliferation of this kind would have been helpful.

KURT W. BACK

Duke University

The Sociology of Punishment and Correction. By NORMAN JOHNSTON, LEONARD SAVITZ, and MARVIN E. WOLFGANG. New York: John Wiley & Sons, 1962. Pp. xi+349. $6.50 (cloth); $4.25 (paper).

This volume, a companion piece to *The Sociology of Crime and Delinquency* by the same authors, concentrates on problems of causation. It provides a selection of previously published articles and excerpts from longer works all concerned with problems of justice and corrections. The fifty selections are organized into five sections.

The first section, "The Administration of Justice," contains twelve selections dealing with such subjects as police violence and detention practices, the bail system, jury deliberations, legal sanity, and sentencing practices. Fourteen articles make up the second section dealing with the prison community. Most of these are from the recent and sophisticated studies of the prison as a social organization. The third section deals with treatment. Of the nine articles in this section, four outline problems of treatment strategy, two report on specific treatment projects, and two report evaluations of an institutional and probation program. The fourth section presents ten selections dealing with prediction. The fifth and final section contains five articles describing prevention projects.

The criteria employed by the editors in selecting the articles to be included were that they be sociological, contemporary, and empirical. The selections are largely contemporary and empirical; most are directly sociological, and all are of sociological import. Although I would have preferred certain articles to some included, the editors have chosen a sample of articles from the better portions of the literature.

The editors have written a brief introductory statement for each section, intended to introduce the articles in the section and to tie the sections together. They provide only the briefest framework or introduction for the reader to the selections and only the thinnest thread between the sections. Unless the reader has benefit of well-structured lectures or a text to supply him with a statement of the problems, issues, and theoretical positions in each of these areas, he will probably have no sound framework to use in attempting to relate the selections to one another and to the problems with which they deal.

For those who would like to use a sample of reasonably good articles conveniently bound together in a single volume, this book is recommended. As is the case with most books of readings, the instructor must provide the framework for the theoretical integration of the material.

DONALD L. GARRITY

San Francisco State College

The Psychology of Strange Killers. By JAMES MELVIN REINHARDT. Springfield, Ill.: Charles C Thomas, Publisher, 1962. Pp. xiii+196. $7.00.

The author makes abundantly clear in his preface that "this book does not propose a theory of murder." And indeed it does not; neither grand, middle range, nor minuscule. Nine individual cases comprise the entire book, with the exception of chapter i, which is a brief summary of some bizarre killings and a few rambling notes by the author, who raises questions that remain unanswered. All but two of the cases are "multicides," that is, each slayer had killed more than once. Most of the offenders were neglected children, who "spent their early years in wretched states of social and psychological deprivation," and never experienced "normal communication with a dependable, understanding part of the social world about them." Lest the reader feel that these very general remarks might lead to some meaningful hypotheses, the author hastens to add that "the same could be said of many people who never killed anyone."

For psychiatrists and psychologists interested in these unusual types of murders, the book provides disappointingly sketchy case histories. For the sociologically oriented criminologist there is inadequate empirical material for data analysis and an absence of meaningful interpretation. We are told that these strange murderers kill "ingeniously for the strangely fevered excitement of the killing" and are "impelled to commit the act by some indescribably cruel psychic need." They are oppressively lonely and "thoroughly sick of themselves." This is the highest level of abstraction the book offers, as the author makes only a tantalizingly passing reference to Dostoevsky. Although the material may have taken a long time to collect, the book appears to have been put together hastily and ends abruptly. There is no index, but none is needed.

MARVIN E. WOLFGANG

University of Pennsylvania

Psycho-analysis in Groups. By ALEXANDER WOLF and EMANUEL K. SCHWARTZ. New York: Grune & Stratton, 1962. Pp. 326. $8.00.

Small-group theorists and students of social interaction may take exception to Lewis R. Wolberg's note that "no clear picture exists of the psychological and sociological elements that enter into group interactions" (Foreword, p. v). Nor will the sociologically oriented reader find a systematic discussion of what is presently known of the social psychology of social interaction and of what still remains problematic or unknown. Such theory and research are beyond the authors' purview. As Wolberg appropriately notes, the authors' "emphasis is depth analysis rather than on the impact of group support and interaction" (p. viii). Although a sociological approach is summarily dismissed, there is much to stimulate thought.

By "depth analysis" is meant, essentially, the individual's communication and working out of his intrapsychic and interpersonal problems in social interaction. While these intrapsychic and interpersonal problems are characteristically communicated and worked out in the dyadic analyst-patient relationship, the senior author has been developing psychoanalysis in groups for twenty-four years. The initial impetus for group psychoanalysis was in response to the treatment needs of low-income patients.

The difficulties in objectively studying intrapsychic processes qualitatively, let alone quantitatively, are well known, and this book will provide little that is of value to quantitative research processes. One cannot completely suppress the feeling that acceptance of the concepts developed here, acknowledging problems of scientific "proof" (however *this* may be defined), depends upon the extent to which one feels comfortable with a psychoanalytic frame of reference. Although highly readable, this contribution is not likely to convince those who are unconvinced of the value of a psychoanalytic approach to group process.

Bypassing the therapeutic value of psychoanalysis in groups (this reviewer is not competent to evaluate it), there is much to stimulate social psychological thought about group social interaction. What characterizes patients in group therapy may be an accentuation of latent personality intrusions upon everyday social interactions in the home, at work, and in the community. For example, a young patient may elicit transferences from other patients who overprotect and immobilize him. Or the presence of both sexes may sharpen and incite projection of maternal, paternal, and sibling relationships. One wonders to what extent atherapeutic mixed-age groups may provide an arena for some younger members to enact dependency roles aided and abetted by solicitous and overprotective older persons.

Furthermore, there may be similarities in attitudes toward the analytic group and other social groups. The minority who are very enthusiastic about the analytic group have motivations that range from needs for social contact to such unconscious motivations as exhibitionism, voyeurism, and a search for foils for neurotic destructiveness. But many social groups have members who facilitate social interaction. While many have a "constructive" approach, there are others who are working out their intrapsychic problems in ways that are helpful to the group but that reinforce their own inner personality structure: compulsive sociables, compulsive organizers, some hypomanics, and some exhibitionists. In the analytic group at least, even the schizoid facility with unconscious symbols may enhance the functioning of the group. Psychoanalytically speaking, it may be that social facilitation roles may provide an outlet for the working out of character structures that are not diagnosed as neurotic. In short, a focus upon organization may represent not only a constructive social talent; it may reflect the way in which a person maintains control over himself and his relation to his environment.

Although subject to the same problems of researchability, discussion of the six stages of group therapy, how the group functions, and how the analyst manages himself stimulates one to further thought. In no uncertain terms the authors advocate a heterogeneity in group psychoanalysis with the exception of psychopaths, alcoholics, morons, stutterers, hallucinating psychotics, and hypermanic patients. Homogeneous groups may run the risk of becoming the "pathological subculture" that the neurotic seeks (p. 66). Homogeneity may be narcissistic in that it may comply with a person's neurotic drive to make others like himself. A group without differences may create a false sense of belonging, and it may be isolational (from personal and cultural differences), authoritarian, and inbred.

There are numerous references to American

culture and value systems. In espousing heterogeneity, the authors imply that the analytic group must counter a prevailing trend. "In order to belong, the American tends to sacrifice his private views—for there is no greater sin in America than to stand apart" (p. 79). Chapter xi contains a rather rambling, overlapping statement of twenty-seven values to which the authors adhere. One wonders how frequently students of small-group interaction do more than simply acknowledge that "the therapist [researcher] has values, the patient [subject] has values, and there are prevailing values in the community" (p. 239, bracketed comments added).

This review has concentrated upon some implications of group psychoanalysis for some aspects of social psychological analysis. It may help the reader to list the chapter headings: "Basic Design" (of the book), "Structure," "Size," "Other Variables" (other than structure and size), the "Alternate Session," "Acting Out," "Dreams," "Working Through," "Routine Individual Sessions," "Misuse of the Groups" (by patients), "Values," "An Application" (to the formal educational process), "Synthesis," and "The Future."

For those who are comfortable with a psychoanalytic framework and who have a basic familiarity with its concepts and terminology, this book is not only interesting reading but also challenges the social scientist to think of group interaction in terms of questions that cannot yet be answered with currently available quantitative research methods.

John Mabry

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Casework in Child Care. By Jean Kastell. New York: Humanities Press; London: Routledge & Kegan Paul, 1962. Pp. xi+306. $7.00.

This is a clear review of the varied responsibilities carried by the child care officer in the Children's Department in England. These responsibilities are centered primarily on children in foster homes. The author is concerned with the potential for casework service in the child care officer's contacts with the natural parents, the foster parents, and the children. Recognition is given to the conflict in roles for the worker when attempting casework service while representing the Department in implementing decisions to remove or place a child.

The volume can serve as a good general introduction to casework with children within a public agency. Unfortunately, the bureaucratic structure of child care in England is not developed, and thus little context in terms of the agency setting is provided. While the author's argument for more and better casework service is supported and case material is provided, the volume contributes little to a deeper understanding of either the problems of children in care or of the dynamics of casework service.

Jane C. Kronick

Bryn Mawr College

Social Welfare Policy, First Collection: Contributions to Theory. By J. A. Ponsioen, Pierre de Schlippe, D. G. Karve, and Eugen Pusić. The Hague: Mouton & Co., 1962. Pp. 287.

The theory and practice of community development become more lucid, meaningful, and significant by the provocative discussions of four experienced practitioners. This most recent area of knowledge in the field of social welfare, because of its very newness, has kept many social scientists and social work practitioners wondering about its nature, purpose, and development. The existing literature has been largely developed by American authors, ranging from the theoretical sociologist to the governmental practitioner of community development. This new text presents the theory and practice of community development from the point of view of the European expert who has had first-hand experience in this area.

Divided into four parts, the text covers the following areas: a general theory of social welfare policy; a theory of community development; planning and evaluation of community development as witnessed in India; and a case history of the Yugoslavia experience.

Dr. Ponsioen, the editor of this volume, launches the discussion with a comprehensive review of social services as experienced by some European countries. The exploration of the theoretical conceptualization of social welfare policy, covering the entire gamut of problems encountered in meeting human needs, is elaborately revealed. The evolution of the social work method is underscored as an effective process for solving such needs, and its adequacy is further lauded when it is utilized for the development of workable policies for social change.

The second section is devoted to a theoretical framework for community development. Sketching a brief history of the movement, Schlippe's analysis attributes the dramatic growth of this social phenomenon to the natural process of cultural development brought about by rapid industrialization, coupled with an accelerated cultural change, and culminating in a serious cultural lag. Community development is the directed action to counterbalance these upheavals brought on by either an industrial or a scientific revolution, or by both.

The third section, the planning and evaluation of community development, is written by D. G. Karve. Based upon his experiences of working with volunteers in India, Karve cites cases and examples of developing a comprehensive program of village improvements. The significance of such progress lies in the fact that it has been achieved entirely with volunteers. The comprehensiveness of the involved programs, where some ten million people were involved, adds a new dimension to community development.

The last section of the book, and undoubtedly the most fascinating part, deals with Yugoslavia as a case study in community development. Written by Eugen Pusić, of Zagreb University, it is a dramatic example of a successful operation.

In conclusion, this is an interesting and valuable textbook dealing with a brand-new topic, community development. The volume has some limitations because of its format. The authors saw fit to present their material in the form of lecture notes. This creates some rigidity, with the language frequently becoming highly condensed. For the reader who is accustomed to the narrative and expositional styles, this may create some discomfort. On the other hand, the format does not detract from the wealth of material included by the four authors.

A further criticism is the concept of social work as a profession. On the positive side, the authors identify social work as the most effective process adopted by the community developers. There is adequate recognition of the philosophy and methodology of social work. The only criticism leveled is against the projected role of the social worker. The role suggested by the authors is rather rigid and restricted and may be seriously questioned by American social workers.

On the other hand, when the writers indicate the distinction between community development as the overseas modus vivendi, and that of community organization in the American conceptualization, this will be quite acceptable to the social work profession.

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Essentials in Interviewing. By ANN F. FENLASON. Revised by GRACE BEALS FERGUSON and ARTHUR C. ABRAHAMSON. Rev. ed. New York: Harper & Row, 1962. Pp. xvi+372. $5.00.

This revision of Fenlason's text on interviewing "for the interviewer offering professional services" is not extensive. Ferguson and Abrahamson, who are responsible for it, have attempted as little change as possible, restricting their contribution to less than fifty pages. They are probably correct in their feeling that the original text represents a sound introduction to interviewing for the person who must use it as a tool in professional practice.

The guiding conception of the original work was that of a presentation of broad ideas from the social sciences and their bearing on the interviewing of clients. This idea was implemented by a discussion in outline form of major concepts such as culture and personality, and deductions from them as related to the interviewing process. The outline was then reinforced and interpreted for the student by the introduction of copious case materials from a variety of sources.

The major addition is a chapter on role theory which, while updating the text, serves to bring the perspectives of sociology and social psychology more sharply to bear than was the case with the original. The body of this new chapter is generally sound and clear, and the illustrations appropriate. The initial presentation of status and role, however, stays too close to sources where these concepts are elaborated at much greater length, and might possibly induce some confusion in the beginning student. The balance of the chapter goes far toward redressing this situation.

Although the original has not become dated to a marked degree, it would seem advisable to extend the suggested readings following the chapters incorporating new material. This has been done only sketchily. The student, thus, is not directed to some of the most incisive recent material bearing on the points covered.

There are some errors in collation in this edition. The pages numbered 98 and 99, for example, are transposed.

As so much caution has been shown in revising, the new edition retains all the strengths of the preceding one. The material that has been added represents an extension of the format and spirit of the original and is generally successful. As before, the book represents a practical and constructive introduction to client interviewing for persons entering professional training or for those who find themselves in practice without such training.

W. W. VOSBURGH

Bryn Mawr College

Sources of Religious Sentiment. By MAURICE HALBWACHS. Translated by JOHN A. SPAULDING. New York: Free Press of Glencoe, 1962. Pp. 109. $4.00.

Though the dust jacket of this slender volume describes it as "a concise analysis of Durkheim's studies of primitive religious practices," it is, in fact, merely an abridgment of *The Elementary Forms of the Religious Life*. There is, therefore, little to be said about Halbwachs' contribution except that it is faithful to the original and quite readable. This volume could prove of considerable value in undergraduate courses in which the instructor would like to treat briefly Durkheim's work in this area. However, the price of the book (in view of its size) will probably dictate the purchase of copies for the library rather than direct student purchase.

Durkheim's original thesis, which this volume summarizes, has been reviewed and criticized so often that most of its strengths and weaknesses are by now quite familiar. One could not hope even to list them all in a brief review such as this. Hence, I shall limit my remarks to brief comments about three features of Durkheim's work that in my opinion have not received the attention they deserve.

To begin with, it seems strange that more of Durkheim's reviewers have not seen fit to explore the relevance of the Weber-Troeltsch distinction between churches and sects for Durkheim's central thesis that society is the reality that lies behind all religious symbols. Sectarian religions would seem to pose a major problem, since, according to all the literature, such groups are either hostile or indifferent to society. It is rather difficult to reconcile this traditional conception of sects with Durkheim's thesis. This suggests that his thesis may be more relevant to some types of religions than to others and hence may not have the universal applicability its author attributed to it.

Second, sociologists in general and functionalists in particular seem to have been far too uncritical about Durkheim's thesis that the worship of society is a safe or desirable expression of piety. One need only recall the Nazi movement, which would seem to qualify as a modern, non-theistic faith, to realize the demonic powers inherent in this type of religion. In fact, it is one of the ironies of history that Maurice Halbwachs, a dedicated disciple of Durkheim, should have died in a concentration camp operated by adherents of this new faith whose object of worship was German society.

Finally, on a more positive note, it seems strange that none have yet commented upon the interesting relationship between Durkheim's thesis and Will Herberg's thesis that a new faith is emerging on the American scene—that which he calls "the common religion of Americans," in which the object of worship is the American Way of Life. According to Herberg, this new faith threatens to subvert the older faiths and transform them into mere vehicles for the expression of our reverence for American society.

In short, Durkheim's thesis is a mixed package involving on the one hand a shrewd and penetrating insight and an important new perspective on the problem of the sources of religious faith, and on the other hand a view of society and religion that is grossly oversimplified at a number of points and thus very misleading. The task of assessing his work hardly seems to have begun.

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CATHOLIC EDUCATION, ECONOMIC VALUES, AND ACHIEVEMENT[1]

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ABSTRACT

This study tests the hypothesis that at each of three educational levels Catholic males who are educated solely in religious schools will exceed those whose formal education was exclusively secular in the proportion of those who fail to (1) internalize the values of worldly success and (2) actually achieve superior socioeconomic status. The sample comprising 265 to 309 men is part of a larger probability sample of young married couples residing in the nation's seven largest Standard Metropolitan Areas. The data derived from questionnaire and interview materials include thirty items measuring commitment to work and mobility values, and five measures of monetary achievement and occupational status and mobility. In the value sphere only six of the ninety comparisons were significant with a reasonable probability that even these reflect nothing more than sampling variability; there were no statistically significant differences between the two groups in economic behavior. Accordingly, the hypothesis that among Catholics a Catholic education is negatively related to achievement values or to subsequent economic success is completely rejected.

The continuing debate on the eligibility of religious schools for tax-supported financial assistance has provoked considerable speculation on the social consequences of a Catholic education. The legal, philosophical, and even metaphorical dialectics guiding the discussion have by now assumed a certain ritualistic predictability. Justice Black's strict interpretation of the establishment clause[2] is confronted by Justice Douglas' declaration that "we are a religious people whose institutions presuppose a Supreme Being."[3] Social cohesion is invoked against cultural pluralism, and the melting pot is once again offered as a counterimage to the symphony orchestra. But whatever their differences, both partisans and opponents of religious schools share the common conviction that a Roman Catholic education serves as a mechanism of indoctrination that reinforces the influence of home and church. In experimental terms this assumption may be expressed as the expectation that Catholic products of Catholic schools will differ in important respects from their coreligionists who have been exposed to secular educational institutions. We shall bring evidence to bear on the validity of this assumption for one critical sector of American life by exploring the influence of type of education on the values and achievement of worldly success.

The two most prominent studies in the

[1] The data used in this study were collected as part of a study of fertility conducted by the Office of Population Research, Princeton University, under the administrative direction of the Milbank Memorial Fund. It was supported by grants from the Carnegie Corporation and the Population Council, Inc. We wish to acknowledge our appreciation to the School of Education of New York University for defraying the costs of additional data processing in connection with this analysis, and to the Social Science Research Council for its award of a grant-in aid to Marvin Bressler.

[2] See *Everson* v. *Board of Education of the Township of Ewing, et al.*, 330 U.S. 1, 67 Sup. Ct. 504 (1947).

[3] See *Zorach* v. *Clauson*, 343 U.S. 306, 72 Sup. Ct. 679 (1952).

meager research literature devoted to this issue are not comparable in design, in use of research techniques, or in their samples, and perhaps for these reasons arrive at opposite conclusions. In his *The Religious Factor*, Gerhard Lenski writes: "Evidently Catholic schools do not generally develop in boys those attitudes, values, beliefs, and intellectual orientations which make it possible for a man to enjoy the more demanding jobs in the modern metropolis."[4] This observation was most pertinent for middle-class men. By contrast, on the basis of their analysis of Strodtbeck's unpublished New Haven data, Rossi and Rossi conclude: "Although the differences between parochial and public high school students are not very large they do tell a consistent story: the parochial student, whether from a 'white collar' or 'blue collar' background, is less attached to his family group and more oriented toward the prevalent middle-class norms of occupational success."[5] However, since small numbers prohibited Lenski from holding educational level constant and Strodtbeck's data refer only to high-school students, it is altogether possible that these apparently contradictory findings are an artifact of the research process rather than genuinely irreconcilable depictions of empirical reality.

In the absence of unambiguous directives from concrete research findings, hypothesis formation must necessarily rely on plausible inferences derived from the main stream of sociological scholarship. Orthodox theory supports the expectation that Catholic education should reinforce historical tradition that has been antagonistic to the full expression of the individualistic ethic and the impulse for material acquisition. Thomist ambivalence on the morality of private property, the principle of the "just price," medieval injunctions against usury, the emphasis on social justice, and the ethical imperative to share surplus wealth are all still a durable part of the mood and substance of Catholic teaching.[6]

These specific doctrinal restrictions on accomplishment in the economic sphere are presumably augmented in the Catholic educational establishment by an "atmosphere in which the spiritual and the supernatural hold the primacy in the hierarchy of temporal and eternal values."[7] In the words of Fichter: "Religion permeates the curriculum and the child is deliberately given to understand that his relationship with God is the most important single aspect of his life on earth."[8] It would appear a priori that so much preoccupation with other-worldly matters would impose severe restraints on an achievement ethos and prove dysfunctional for the realities of economic struggle.

By contrast, instruction in secular schools is reputed to be relatively free of religious symbolism, theological elucidation, spiritual exhortation, and other pervasive religious influences. According to one critic: "Our public schools and colleges are rarely antireligious. They simply ignore religion. Obviously, if a child is taught in school about a vast number of things . . . for 25 hours a week, eight or nine months a year . . . and if for all this time matters of religion are never seriously treated, the child can only come to view religion as, at best, an innocu-

[4] Garden City, N.Y.: Doubleday & Co., 1961, p. 248.

[5] Peter H. and Alice S. Rossi, "Some Effects of Parochial School Education in America," *Daedalus*, Spring, 1961, p. 313.

[6] See, e.g., Matthew F. Brady, "Why American Catholics Conduct Schools," *The Role of the Independent School in American Democracy* (Papers Delivered at a Conference on Education [Milwaukee, Wis.: Marquette University Press, 1956]): "To educate solely for 'success' in life when we mean by success, comfort, ease, luxury, esteem, power, is laudable to a degree but only a partial objective of complete education. Again material consideration alone cannot fulfill the longings, the ideals of man's soul, for his spiritual nature cries out for fulfillment in a realm that is above and beyond the omnipresent and encroaching world about him."

[7] Neil G. McCluskey, S.J., "The Catholic School in Theory and Practice," quoted in Philip Jacobson, "A Jewish Viewpoint on Church-State-School Relations," in William Brickman and Stanley Lehrer (eds.), *Religion, Government, and Education* (New York: Society for the Advancement of Education, 1961).

[8] Joseph S. Fichter, *Parochial School* (Notre Dame, Ind.: University of Notre Dame Press, 1958).

ous pastime preferred by a few to golf or canasta."[9] It seems plausible that a philosophy of instruction that transmits the ethical standards of the Judeo-Christian tradition without supernatural sanctions might considerably reduce inhibitions impeding the rational pursuit of financial gain.

These considerations strongly suggest the hypotheses that, at each educational level, religiously educated Catholics should exceed secular-educated Catholics in the proportion who (1) fail to internalize values that are presumably conducive to worldly success, and (2) actually fail to achieve superior socioeconomic status. Moreover, one might reasonably expect such differences to increase in magnitude as additional increments of years of education multiply the opportunities for the inculcation of Catholic doctrine.

DATA AND PROCEDURES

The data used to test the hypotheses that Catholic men[10] educated in Catholic schools are less committed to an achievement ethos and subsequently achieve less than those educated in non-sectarian schools and colleges come from a longitudinal study of factors affecting fertility in metropolitan areas of the United States.[11] The sample design has several distinct advantages for testing such an hypothesis. In 1957 a probability sample of 1,165 native-white, once-married couples, all of whom had had their second child born six months earlier, and who were residing in one of the seven largest Standard Metropolitan Areas of the country, was interviewed by teams of professional interviewers. The main feature of such a sampl for the present analysis is that it permit comparisons of economic behavior at roughly similar stage in the life-cycle whil eliminating other sources of variation extra neous to our interest.[12]

The data utilized in the present analysi are drawn from two sources. The informa tion on education, socioeconomic status an mobility of the husband come from inte views with the wife. The items measurin attitudes toward achievement derive fro a self-administered questionnaire that th husband was requested by his wife to com plete and return by mail.[13]

Since the sample was drawn from th largest metropolitan areas in the country the proportion of Catholics (nearly half) wa larger than would have been found in sample of the national population as whole. For purposes of our analysis, how ever, we have reduced the sample to a tota ranging between 265 and 309 men.[14] Th major loss of cases resulted from our desir to purify the comparisons by eliminatin Catholics who were educated partly i Catholic and partly in public schools an colleges. Although this purification resulte in the loss of a quarter of the Catholic me (105), most of the combinations were in sufficiently frequent to be usable.[15]

The Catholic sample, as a whole, at th time of the first interview (1957) exhibite the following characteristics. All had tw children, with the youngest child six month old, and lived in one of the largest cities o

[9] Bernard Iddings Bell, "Our Schools—Their Four Grievous Faults," in C. Winfield Scott and Clyde M. Hill (eds.), *Public Education under Criticism* (New York: Prentice-Hall, Inc., 1954), p. 63.

[10] The religious preference of the man was determined by a question asked of his wife.

[11] The report summarizing the first phase of the study is in Charles F. Westoff, Robert G. Potter, Jr., Philip C. Sagi, and Elliot G. Mishler, *Family Growth in Metropolitan America* (Princeton, N.J.: Princeton University Press, 1961). A second volume will appear in 1963 that summarizes the prediction of fertility over a three-year period.

[12] The details of the sampling plan are discusse in Westoff *et al.*, *op. cit.*, pp. 16–28.

[13] A total of 941, or 81 per cent, of the husband returned usable questionnaires. Although this re sponse rate is unusually high for mail questionnaire there is some bias in the typical direction of favorin respondents from slightly higher socioeconomi levels.

[14] The range arises because in the analysis o economic behavior we were able to use data collect ed from the wife, but the attitudinal material cam from the husband's questionnaire with the lowe response rate.

[15] The most common pattern (thirty-six cases was attendance at a Catholic elementary school an graduation from a public high school.

its suburbs. The average Catholic couple had been married four to five years, the man was twenty-eight years old and his wife twenty-six. The average earnings of the men in 1956 was $4,400 and about 43 per cent had white-collar jobs. Some 26 per cent had attended college, while 65 per cent had not gone beyond high school. Only 9 per cent failed to receive education beyond the elementary-school level.

ANALYSIS

The strategy of our approach is to compare economic achievement attitudes and behavior of Catholic men educated solely in Catholic schools with the attitudes and behavior of those whose formal education was exclusively secular. Quite clearly, a wide variety of circumstances that might have affected their attitudes as well as their fortunes could have and probably did intervene in the five to ten years that these men have been out of school. But the fact remains that we are interested in ascertaining whether a Catholic education has any lasting effects in the economic sphere, in many respects a more important consideration than any immediate differences that might exist while the individual is in school.

The items selected to measure attitudes toward achievement are a series of thirty statements that the man was asked simply to indorse or reject. The statements fall roughly into two classes: those measuring a commitment to the work life and those reflecting a drive for success.[16] The respondent was asked to contrast the value of work with leisure-time activities and other aspects of home life. Attitudes toward success were measured by two groups of items, one of which probed the importance the individual attached to getting ahead in work and another set that attempted to assess the strength of his mobility drive by inquiring whether he would be willing to sacrifice a number of different values (such as friends, leisure time, family, security, political and religious views) in order to get ahead.

These items are not, of course, statistically independent. On the contrary, they were designed originally to form a number of different scales that were also utilized in the present analysis in order to capitalize on greater reliability;[17] the scales failed to disclose any patterns differing from the individual items. The items were analyzed individually for this study with full knowledge that, although many do correlate with each other, there is a considerable amount of independence; this led to the hope that variations in items with different contents might provide some insights into the nature of any association with Catholic education.

Five measures of economic achievement were employed: the respondent's earnings for the year prior to the interview, the change in his earnings since his marriage, the social prestige[18] of his current occupation, and two measures of occupational mobility,[19] one from his occupation at the time of his marriage and the other a comparison of his present with his father's main occupation.

Because of the limited number of cases involved we are only able to differentiate three levels of education: elementary only (up to eight years), attended high school, and attended college. The majority of men in the first two categories are graduates, but most of those classified at the highest educa-

[16] For reasons of economy the detailed wording of the items is not reproduced here. The reader interested in any such detail is directed to Westoff *et al.*, *op. cit.*, pp. 383–400, where the full wording, the distribution of responses of the total sample, and the intercorrelations of items are presented.

[17] The scales constructed were commitment to work values, importance attached to getting ahead, general drive to get ahead, willingness to sacrifice social interests to get ahead, and willingness to sacrifice ideological values to get ahead.

[18] Scored on the North-Hatt occupational prestige scale based on the 1947 National Opinion Research Center poll. For a recent appraisal and revision of this instrument see Albert J. Reiss, Jr., *Occupations and Social Status* (New York: Free Press of Glencoe, Inc., 1962).

[19] Intragenerational and intergenerational occupational mobility scores were calculated by differencing the social prestige ratings of the occupations involved.

ional level spent less than four years in college.

RESULTS

In Table 1 the responses to thirty statements reflecting the achievement attitudes of Catholic men whose education was exclusively secular or Catholic are compared at each of three educational levels, for a total of ninety comparisons. These data fail to confirm the expectation that a Catholic education is associated with the devaluation of an achievement ethic. Statistical tests reveal that only six of the ninety comparisons are significant, with the reasonable probability

TABLE 1

PROPORTION OF ACHIEVEMENT-ORIENTED REPLIES* TO THIRTY QUESTIONS ON ATTITUDES TOWARD WORK AND SUCCESS OF CATHOLIC MEN EDUCATED IN SECULAR SCHOOLS COMPARED WITH THOSE EDUCATED IN CATHOLIC SCHOOLS, BY HIGHEST LEVEL OF EDUCATIONAL ATTAINMENT

Attitudinal Item	Elementary School		High School		College	
	Secular	Catholic	Secular	Catholic	Secular	Catholic
Relax at home or work	81	92	91	89	84	85
Work more satisfying than time spent at home	31	33	28	27	30	35
Would work regardless of inheritance	37	75†	58	52	45	50
Work makes life worthwhile	50	50	33	32	18	25
Main interests in life connected with work	31	50	42	62†	70	70
Regret going in present line of work	63	50	52	53	48	50
Work most satisfying part of life	50	50	38	49	61	45
Enjoy spare time activities or work	13	50†	32	38	50	75
Work just a way of making money	31	33	52	47	77	80
Opportunity most important aspect of work	81	92	68	64	50	50
Spent time thinking about improving chances	75	92	74	87	61	35
Achieving money and possessions very important	37	75†	58	52	50	40
Satisfied with present opportunities	62	83	53	53	68	75
Important to own things as good as friends do	56	58	47	44	41	40
Anxious to get much further ahead	69	83	81	89	67	90
Getting ahead one of most important things in life	62	58	64	64	50	65
Determination and ambition important qualities	75	92	66	64	34	25
To get ahead, would:						
Give up vacations for several years	75	75	77	84	59	89†
Leave friends	75	75	71	71	68	84
Live in undesirable neighborhood temporarily	81	50	44	47	41	63
Give up leisure time	75	92	70	84	68	89
Postpone having another child	56	42	48	38	27	21
Move family to strange part of country	87	75	60	67	73	74
Do less interesting or less enjoyable work	37	50	48	44	41	53
Not see family as much as would like	37	33	18	35*	24	41
Risk health	12	17	11	18	18	37
Keep quiet about political views	50	75	40	37	37	53
Take a less secure job	53	42	46	56	48	68
Send children to poorer schools	44	25	17	24	18	21
Keep quiet about religious views	19	33	17	11	19	5

* In two or three instances it is not unambiguously clear whether a positive or negative response reflects an orientation toward achievement. These instances were decided in terms of the direction of their correlation with other items.

† Difference between the two proportions significant at the .05 level as determined by χ^2 with 1 degree of freedom. The number of men who attended secular or Catholic schools is 16 and 12 for elementary school, 128 and 45 who attended high school, and 44 and 20, who attended college.

that even these reflect nothing more than sampling variability.[20] Moreover, a straightforward analysis of the items that elicited any sort of differential response between the two groups shows that the proportion of achievement-oriented replies was higher for the Catholic-educated in fifty-one of seventy-five instances. It would be overstating the case to argue the obverse of our original supposition; there is insufficient evidence to demonstrate that a Catholic education actually *encourages* an allegiance to work and mobility values. But we can quite clearly reject the hypothesis that, among Catholics, religious education is negatively related to these values of wordly success.

One might reasonably maintain that such value statements are of doubtful reliability and even assuming high reliability, there may be reason to question the predictive validity of such values for economic behavior. Most social scientists would argue that actual economic achievement is of greater importance than attitudes toward achievement. With such considerations in mind we compared the socioeconomic accomplishments of Catholics educated in the two educational systems (Table 2). The five measures of status and mobility described above produce fifteen comparisons for the three educational levels. In none of these fifteen comparisons is there any statistically significant difference in the proportions. Whether the variable involved is income, occupational status, or mobility, the proportion of Catholic men scored as "high" on achievement is the same for those educated in Catholic schools and colleges as it is for Catholics educated in non-sectarian schools. Thus our negative conclusion can be extended to important measures of economic behavior as well as economic values and attitudes.

TABLE 2

PROPORTION OF CATHOLIC MEN ABOVE AVERAGE* ON SELECTED MEASURES OF SOCIO-ECONOMIC STATUS AND SOCIAL MOBILITY, BY TYPE AND LEVEL OF EDUCATION

	Elementary School		High School		College	
	Secular	Catholic	Secular	Catholic	Secular	Catholic
Earnings in 1956	30	47	56	58	78	75
Changes in earnings since marriage	25	33	45	34	47	33
Social prestige of occupation	25	20	38	44	81	100
Intragenerational occupational mobility	30	33	28	32	29	25
Intergenerational occupational mobility	50	43	33	28	58	59

* Above the median of the distribution of all men on each scale. For sample sizes see n. †, Table 1.

IMPLICATIONS

The lack of association between a Catholic education and distinctive patterns of socioeconomic values and achievements invites speculation both as to causes and consequences. These results are especially striking since the initial selection into religiously affiliated schools and colleges is from the most religious homes. Thus both formal and informal educational processes within the school should be mutually reinforcing.[21] As-

[20] Depending on the severity of the test applied, a plausible argument can be advanced that these data (1) are, (2) are not, consistent with the findings reported by the Rossis (Rossi and Rossi, *op. cit.*). They are supported by the over-all direction of the associations, but not by the criterion of statistical significance.

[21] The comparative effects of home and school training on religious knowledge and values are the subject of a projected study, the plans for which are described in Joseph G. Keegan, "A Study of the Relationship between Religious Atmosphere in the Home and Amount of Catholic Education, on the One Hand, and the Religious Knowledge and Religious Attitudes of Male Catholics Attending College, on the Other," in Stuart Cook (ed.), *Research*

uming that our inquiry is free of serious 'aws in design or measurement, the sources f this seeming anamoly may lie in a faulty et of implicit assumptions that frequently uide sociological expectations. These are 1) Catholic schools teach a distinctively Catholic or at least "religious" stance on conomic matters, (2) schools are an effective vehicle for the transmission of values, nd (3) religion markedly influences attiudes and achievement in the economic phere. Each of these assertions is open to onsiderable question.

We suspect, for instance, that orientaions to worldly success proferred by the wo school systems are more congruent than s ordinarily supposed. Sociologists of reliion, following Max Weber, probably have veremphasized the monolithic unity of Church teaching on economic matters and neglected opposing tendencies within the Catholic tradition. Economic historians such s Amitore Fanfani have demonstrated that he ideals of hard work and thrift, a welleveloped rationale for private property, nd an intellectual climate favoring the exansion of commerce and industry, existed n Catholic Italy during the fourteenth and fteenth centuries in the period of the rise f modern capitalism.[22] Similarly, H. M. Robertson has assembled impressive evilence from a variety of sixteenth- and eventeenth-century sources that reveal fa'orable Catholic responses to the idea of vork as calling, worldly asceticism, bour;eois virtues, economic rationality and the onsecration of labor.[23] Pending further inestigation, it would be premature to disniss the possibility that this other side of Church tradition has become a viable part of the economic message of the contempoary Catholic school.

Even assuming that Catholic instruction s free of doctrinal ambivalence, formal education may be an indifferent instrument for the transmission of a durable and coherent value system. The least generous estimate is that the absence of pronounced differentials in this study may simply reflect a crisis of competence that is endemic throughout the entire American educational structure. The inculcation of a self-conscious and articulate philosophy of worldly success to average students possessing ordinary perception, intelligence, and motivation may well confound the talents of the classroom teacher in either Catholic or secular schools. A more persuasive explanation is that as only one of many socializing agencies, the school's sovereignty in the domain of values may be highly circumscribed. For example, in summarizing the available evidence on collegiate influences on values Brim concludes that the college curriculum influences one's knowledge more than one's values.[24]

It is possible, of course, that significant differentials by type of education did exist but were nullified by the experiences of the respondents in the years subsequent to their departure from school. Catholic and secular educational institutions differ markedly in their formal aims, curriculums, and pedagogical practices and possibly in their immediate impact on students. They are, however, both engaged in converting latent talent into salable professional, vocational, and personality skills. As Catholic males who are products of either system are exposed to

lans in the Fields of Religion, Values and Morality nd Their Bearing on Religious and Character Formaion (New York: Religious Education Association, 962), pp. 47–55.

[22] *Catholicism, Protestantism, and Capitalism* New York: Sheed & Ward, 1955).

[23] For some representative quotations from Catholic spokesmen see Robertson, "A Criticism of Max Weber and His School," in Robert W. Green (ed.), *Protestantism and Capitalism* (Boston: D. C. Heath & Co., 1959), pp. 74–75: "One must rise to obey God, who only allows us sleep for the body's needs and commands us, when these needs are satisfied, to busy ourselves with the work which He prescribes for us according to our state." Or, "One serves God by faithfully serving one's Prince; one serves God by employing one's capital . . . according to all the rules of probity and justice. There are duties to be performed in all conditions of life, and it is in acquitting oneself of these duties that one is sanctified." Or, "one can work fruitfully for God, for men and for oneself."

[24] Orville G. Brim, Jr., *Sociology and the Field of Education* (New York: Russell Sage Foundation, 1958), pp. 69–70.

given quantities of education, their real worth in the market place and prospects for success in the struggle for scarce economic rewards are correspondingly fixed. Thus confronted by the actualities of the real world, most men may respond by adopting realistic levels of aspiration and a set of values that prove to be functional for their particular potentiality for economic achievement. A rational man occupied in solving his most fundamental problems of economic and personality survival may, in the long run, be anesthetized to the philosophical niceties of Thomist, Calvinist, or Deweyan dogma to which he had been originally exposed.

These reflections purporting to explain why Catholic men appear to be immune to differential educational influences also imply that religion may have dubious utility as an independent variate capable of discriminating achievement orientation and economic success. This conjecture is partially supported by an analysis of Catholic-Protestant differences employing the same procedures that were applied to the intra-Catholic comparisons. Thus, for example, the fact that only fourteen of ninety value items and two of fifteen measures of achievement are significantly different statistically between Protestants and Catholics, and not all of these even in the same direction, strongly suggests a potentially profitable area of inquiry.[25] Any final conclusions about the influence of religion in the economic sphere must be postponed until such time as we are able to augment education with the numerous additional controls that would be required to construct a satisfactory index of religiosity.

All the *post factum* speculations advanced thus far, taken separately or in combination, suggest that the Catholic breadwinner, like other Americans, derives his orientations to worldly success from the norms of his several subcultures and the broader society. Both our data and our interpretations are thus compatible with the conclusions reached by Rossi and Rossi in their survey article on the effects of a parochial school education. The authors note: "We have been unable to find strong evidence that parochial-school Catholics are very different from other Catholics. The influence of the school is shown most dramatically in areas where the Church has traditionally taken a strong stand, for example, on support of religious

[25] After surveying the relevant literature and the data in his own study, Lenski (*op. cit.*) concludes that "it is reasonably safe to say that in the modern American metropolis socially significant differences exist in the rates of mobility among the four major socio-religious groups. The Jewish group seems clearly to be the most successful, with white Protestants second, Catholics third, and Negro Protestants fourth" (p. 80). He concludes also that "as a general rule, commitment to the *spirit of capitalism* is especially frequent among white Protestants and Jews; is much less frequent among Catholics and Negro Protestants even when position in the class system is held constant" (p. 115). Therefore he finds that "it seems safe to conclude that religion makes a difference in the behavior of men in the realm of economic activity. We should not exaggerate the magnitude of this influence, but neither should it be minimized" (p. 114). Donald J. Bogue on the other hand finds that "Roman Catholic household heads tend to have higher incomes, for a given amount of schooling completed" but cautions that "these differences may be due to differences in the age of head, number and type of secondary earners, family structure, and occupation—as well as to cultural factors associated with religious affiliation" (*The Population of the United States* [Glencoe, Ill.: Free Press, 1959], pp. 707–8). Bernard Lazerwitz' extensive data can be interpreted as demonstrating that Catholicism is no bar to economic achievement. His tables show that in the late 1950's identical proportions of Catholics and Protestants (18 per cent) had family incomes of $7,500 or above and also that the distribution of Protestant and Catholic heads of families in white-collar occupations was virtually identical – 21 per cent and 19 per cent, respectively ("A Comparison of Major United States Religious Groups," *Journal of the American Statistical Association*, LVI, No. 295, 574). David C. McClelland's review of social-psychological studies on the achievement motive leads him to conclude "that while the Protestant and Catholic religious labels may have been significantly associated with certain value attitudes, they are not associated in any over-all way with *n* achievement. More careful discriminations must be made within each of the religious sub-groups before one can arrive at any consistent differences in *n* achievement scores" (*The Achieving Society* [Princeton, N.J.: D. Van Nostrand Co., 1961], p. 364). There is no consensus on the relationship between religion and the economic sphere, but all investigators agree that further refinements and controls are needed. There obviously remains much to be done.

education, or on the performance of religious duties. In other areas of life the parochial-school Catholic is only marginally differentiated from other Catholics."[26] Their findings should, of course, be interpreted with considerable caution. The generalizability of the few existing studies[27] in this area is seriously restricted by sampling limitations including geographic specificity, small numbers, and inadequate representation at all grade levels. The Rossis were obliged to rely mainly on four studies that seldom differentiate among various levels of educational attainment and whose data have primary reference to Florida and the New England area.

Obviously, research on the influences of Catholic education must be further refined and extended in range if it is to be genuinely useful in clarifying the alternatives of public policy. Meanwhile, the available evidence fails to sustain those who either hope or fear that a Catholic education magnifies religious differentials in the economic sector or in other secular areas in American life.

PRINCETON UNIVERSITY

[26] Rossi and Rossi, *op. cit.*, p. 311.

[27] Our general conclusions are quite consistent with the findings from comparisons of Protestants and Catholics reported by Greeley in an article that appeared too late to be integrated more fully in this paper. See Andrew M. Greeley, "Influence of the 'Religious Factor' on Career Plans and Occupational Values of College Graduates," *American Journal of Sociology*, LXVII (May, 1963) 658–71.

CLERGYMAN AS COUNSELOR

ELAINE CUMMING AND CHARLES HARRINGTON

ABSTRACT

The clergyman's role as counselor is examined with reference to its articulation with the whole deviance-controlling system. His activities appear to vary with the characteristics of his congregation and his own educational level. It is proposed that the effect is produced by certain strains, some of which inhibit the development of a system of divided labor between clergymen and other deviance-controlling agents, particularly social workers.

This is the second report from a group of studies designed to discover how the task of controlling deviance in society is divided among the various integrative agents and agencies.[1] In this paper we report a study of the clergyman's description of his counseling role and an analysis of this role with emphasis upon its articulation with the rest of the controlling system.

Helping is a traditional activity of the clergy[2] and, as such, has received considerable attention from social scientists. Recent studies have shown that a large number of people turn to clergymen when they are in trouble.[3] Studies of our own have shown that the clergyman ranks with the doctor as the first contact made outside the kinship and friendship circle during the onset of mental illness.[4] Most people appear to be satisfied with the help that they get from the clergyman.[5] As Eaton and his co-workers point out, "Clergy are close to many people during crucial periods of the life cycle. . . . For those who know they need help the clergy is accessible without a waiting list or an intake worker to screen applicants. And going to a clergyman does not require a self-admission of helplessness on the part of the client."[6] In spite of his popularity, the clergyman's counseling role appears to some who have studied it to be "poorly defined," "diffuse," and "conflicted."[7]

[1] A revision of a paper read to the annual meetings of the American Sociological Association, Washington, D.C., 1962. The research reported was supported in part by NIMH Grant 4735, principal investigator, Elaine Cumming. Acknowledgment is made of the assistance of the staff of the Mental Health Research Unit, Syracuse, N.Y., and its director, John Cumming, under whose auspices the studies are taking place.

[2] See Benton Johnson, "The Development of Pastoral Counseling Programs in Protestantism: A Sociological Perspective," *Pacific Sociological Review*, I (Fall, 1958), 59–63, and Robert S. Michaelsen, "The Protestant Ministry in America: 1850 to the Present," in H. Richard Niebuhr and Daniel D. Williams (eds.), *The Ministry in Historical Perspective* (New York, 1956).

[3] *Action for Mental Health: The Final Report of the Joint Commission on Mental Illness and Health* [illegible] 1961).

[4] See Elaine Cumming, "Phase Movement in the Support and Control of the Psychiatric Patient," *Journal of Health and Human Behavior*, III (Winter, 1962), 235–41 (also see Bruce D. Dohrenwend, "Some Aspects of the Appraisal of Abnormal Behavior by Leaders in an Urban Area," *American Psychologist*, XVII, No. 4 [1962], 190–98).

[5] Gerald Gurin, Joseph Veroff, and Shiela Feld, *Americans View Their Mental Health*, Vol. IV of a series of monographs published by the Joint Commission on Mental Illness and Health (New York, 1960).

[6] Joseph W. Eaton *et al.*, *Pastoral Counseling in a Metropolitan Suburb* (Pittsburgh: Southeastern Community Guidance Association [4232 Brownsville Rd.], 1961).

[7] See Samuel Blizzard, "Role Conflicts of the Urban Parish Minister," *City Church*, VII (September, 1956), 13–15, and Allan W. Eister, "Religious Institutions in Complex Societies: Difficulties in the Theoretic Specifications of Functions," *American Sociological Review*, XXII (August, 1957), 387–91 (see also Warren O. Hagstrom, "The Protestant Clergy as a Profession: Status and Pros-

Other observers have suggested that the clergyman has a peculiarly difficult role because of his sensitivity to the values and norms of his congregation.[8] Indeed, the clergyman is probably alone among integrative agents in being a member of the group he controls.[9]

In some respects the clergyman resembles the other helping and controlling agents in the community. All have policies and norms governing who may call upon them for what kinds of help and who should be referred for more specialized attention. These policies and norms set the agent apart as a recognizable system of action and define the "boundary conditions" that both separate him from, and relate him to, other systems. Concretely, an agent's boundary conditions determine the accessibility of his services to clients and other agencies, and help to regulate the patterns of mutual dependency—that is, the division of labor—among agents.

The clergyman's relationships with the remainder of the controlling system, the permeability of the boundary around his role as counselor, and his own helping practices can all be expected to resemble those of other agencies in some ways, but the major hypothesis tested in this study is that, *because the clergyman differs from other agents of social control in being normatively involved with his congregation, his behavior, including his articulation with these other agents, should be influenced by the characteristics of that congregation.*

I. THE STUDY

In Syracuse, New York, a sample of sixty-one churches was selected. They included ten Roman Catholic, six Episcopal, three Lutheran, three Eastern Orthodox, eight Methodist, seven Presbyterian, five Baptist, ten fundamentalist, three Jewish, and six churches of other Protestant denominations. The sample was selected from the total universe of churches in Syracuse after they had been stratified by size, neighborhood status, and denomination, the latter grouped into five categories—Roman Catholic, liturgical Protestant, other Protestant, fundamentalist Protestant, and Jewish. The stratification yielded eighty cells; the sixty-one that contained churches were randomly sampled.[10] One clergyman from each of fifty-nine churches was interviewed as a representative of that

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pects," *Berkeley Pub. Soc. Instit.*, III, No. 1 (Spring, 1957), 1–12; Waldo Burchard, "Role Conflicts of Military Chaplains," *American Sociological Review*, XIX, No. 5 (1954), 528–35; and Blizzard, *op. cit.*

[8] See Joseph H. Fichter, S.J., *Social Relations in the Urban Parish* (Chicago: University of Chicago Press, 1954), chap. x; Burchard, *op. cit.*; Ernest Campbell and Thomas F. Pettigrew, "Racial and Moral Crisis: The Role of Little Rock Ministers," *American Journal of Sociology*, LXIV (March, 1959), 509–16; Michaelsen, *op. cit.*, Lee Braude, "Professional Autonomy and the Role of the Layman," *Social Forces*, May, 1961.

[9] For a general discussion of this point see Talcott Parsons, *The Social System* (Glencoe, Ill.: Free Press, 1950), chap. x; Charles Kadushin, "Social Distance between Client and Professional," *American Journal of Sociology*, LXVII (March, 1962), 517; and Campbell and Pettigrew, *op. cit.*

[10] Church names and addresses were taken from the City Directory; local church officials contributed further details. Size was classified as follows: small, up to 150 total members; medium, 150–500; large, 500–1,000; and very large, over 1,000. Neighborhood status was taken from Charles V. Willie, "Socioeconomic and Ethnic Areas" (unpublished Ph.D. dissertation, Syracuse University, 1957). The neighborhods were classified as upper, middle, lower, and mixed metropolitan. The empty cells in the stratification plan are accounted for chiefly by the absence of small Roman Catholic churches and large fundamentalist churches. The latter group included the so-called store-front churches, seven of which no longer existed at the time of the research. These were replaced by further random choices from the cells. The sample included 10 of the 22 Roman Catholic churches, 6 of the 12 Episcopal, 3 of the 7 Lutheran, 3 of the 11 Eastern, 8 of the 17 Methodist, 7 of the 8 Presbyterian, 5 of the 15 Baptist, 10 of the 21 fundamentalist, 3 of the 8 Jewish, and 6 of the remaining 15 churches of other Protestant denominations. This represents, in terms of the grouping used, 45 per cent of the Roman Catholic, 40 per cent of the liturgical, 47 per cent of the other Protestant, 48 per cent of the fundamentalist, and 37 per cent of the Jewish churches.

church.[11] In churches with more than one clergyman, a pastor was allocated for interview by the clergyman in authority, usually on the basis of his interest in counseling. The results, therefore, should be generalized to the counseling activities of churches in Syracuse, not to the city's clergymen.

Three types of variables were examined: the characteristics of the clergyman and his congregation, the nature of the problems counseled, and the articulation of the clergyman's counseling role with the remainder of the system. Each clergyman was asked to describe his congregation in terms of his age, income, residence, and occupation. With this information, the socioeconomic statuses that had been assigned to the churches on the basis of neighborhood reputation alone were modified. Finally, thirty-one of the fifty-nine churches were classified as upper or middle class and twenty-eight as working class.[12] The size of the congregation and the age and education of the clergyman himself were noted.

Each clergyman was asked to describe the problems brought to him for counseling and the extent of his counseling activities. Forty-one different types were distinguished from these descriptions. In Table 1 they have been divided into problems that more than half of the reporting clergymen counsel, and those that more than half of those reporting refer for service elsewhere. The problems are also grouped, in terms of their social characteristics, into three categories: (1) transition states, (2) deviant behavior, and (3) exigencies. The categories are rough, and decisions to include types in one category rather than another are sometimes based on the interviewer's interpretations, but they are an attempt to develop theoretical classifications of problems that can be expected to be differently handled by the clergy.

Transition states are the adjustment problems inherent in the life-cycle that everyone can expect to encounter. They include adolescent sex problems, need for vocational guidance, loneliness in old age or widowhood, bereavement, and transient maladjustments. Clergymen would be expected to deal with these problems frequently.

Deviant behavior includes infidelity, criminal or quasi-criminal behavior, unwed motherhood, severe marital conflict, mental, emotional, and sexual disturbances, psychoses and neuroses, and chronic inability to get along with others. It would be reasonable to expect clergymen to refer many of these "pathological" problems.

Exigencies are essentially environmental; they include lack of money, unemployment, cultural conflicts and discontinuities, and adjustments to physical illness. The clergyman would be expected to refer those problems that he lacks concrete resources for solving and to counsel those not needing concrete help.

Each clergyman in the sample reported collaborating with others in the deviance-controlling system. These collaborations include contacts with non-professionals acting as informal sources of social control as well as with those professionally concerned with the control of deviance. A clergyman who participates in a system of divided labor with others can be thought of as having a high level of activity across the boundary of his counseling role.

A boundary activity score was computed from the clergyman's responses. Each clergyman was given one point for each of three signs of articulation with the larger controlling system. Thus, a score of two or three reflects relatively high boundary activity, while a score of zero or one, low activity and a relatively impermeable role

[11] Two fundamentalist clergymen refused to be interviewed, although eight attempts were made in each case.

[12] Downtown churches with congregations of mixed economic status were classed as middle class, while those with only a few elderly wealthy people and a majority of very poor parishioners were classed as working class. The latter churches were once fashionable, but have been left in areas of [illegible] and decay.

TABLE 1

INCIDENCE AND DISPOSITION OF PROBLEM TYPES

	Clergy Reporting		Per Cent Referring (of Those Reporting)
	No.	Per Cent (N = 59)	
Problems less than half reporting clergy refer:			
Transition states:			
Premarital counseling	49	83.1	42.9
Marriage and parent-child counseling	38	64.4	18.4
Counseling adolescents	29	49.1	17.2
Loneliness in old age or widowhood	29	49.1	13.8
Bereavement	14	23.7	00.0
Adjustment to puberty	14	23.7	00.0
Problems associated with the menopause	6	10.2	16.7
Adjustment to retirement	4	6.8	00.0
Deviant behavior:			
Marital conflict	37	62.7	48.6
Parent-child conflict	32	54.2	18.8
Criminal and quasi-criminal behavior	28	47.5	28.6
Infidelity	25	42.5	20.0
Depression and pathological guilt	21	35.6	47.6
Hostility, aggression, acting out	11	18.6	45.4
Sexual deviations	9	15.3	44.4
In-law and intergenerational disputes	9	15.3	11.1
General interpersonal conflict	9	15.3	33.3
"Neuroses" and "phobias"	7	11.9	42.8
Irresponsibility and immaturity	7	11.9	14.3
Neglect of children	6	10.2	33.3
Exigencies:			
Adjustment to interfaith marriages	16	27.1	6.3
Adjustment to physical illness	14	23.7	14.3
Adjustment to interethnic marriages	9	15.3	00.0
Unemployment (temporary financial aid)	7	11.9	28.5
Counseling kin of ill clients	6	10.2	16.7
Adjustment to economic dependency	3	5.1	00.0
Problems more than half reporting clergy refer:			
Transition states:			
Problems of child-bearing and adoptions	7	11.9	85.7
Deviant behavior:			
Vague mental disorders	45	76.3	71.1
Unwed mothers	42	71.2	78.6
Alcoholism	36	61.0	69.4
Sexual problems in marriage	22	37.3	63.6
"Psychoses," "paranoia," "schizophrenia"	15	25.4	73.3
Divorce and separation	15	25.4	66.7
"Psychopaths" and "mental defectives"	4	6.8	100.0
Exigencies:			
Insufficient income	42	71.2	57.1
Physical illness	18	30.5	88.9
Child care and support	18	30.5	55.6
Legal problems (non-criminal)	11	18.6	63.6
Housing problems	9	15.3	77.7
Senility	8	13.6	62.5
Transients	6	10.2	83.3

boundary. The three indexes themselves form a Guttman-type scale. The items are:

1. *Receipt of referrals or information about clients from sources outside the congregation.* This characteristic was considered to reflect the accessibility of the clergyman as a helper, and also others' recognition of this role in the helping system. Forty-two clergymen reported this item.
2. *At least one referral by the clergyman to an outside agency in the month prior to the interview.* This activity was taken to indicate that the clergyman perceives certain problems as outside his competence. Referrals indicate that he allows people to leave his sphere of influence and also that he perceives himself as acting with others in a system of divided labor. Twenty-eight clergymen reported this item.
3. *An active rather than a passive form of referral.* Some clergymen participate more actively in the referral process than others; these report that they telephone agencies to inquire if a service is available, make appointments for parishioners, or even accompany them to the agency. Other clergymen suggest to a client or parishioner where he may go for help but make no actual contact with the agency. These we call passive referrals. A passive style of referral suggests that the clergyman is less accessible for co-operative solutions of problems and is less influenced by the norms of the agency world. Twenty-six clergymen reported active participation in referrals.

TABLE 2

NUMBER OF CLERGYMEN BY BOUNDARY-ACTIVITY SCORE AND DENOMINATION

Denomination	Total No.	Boundary Score*	
		0–1	2–3
All clergymen	59	25	34
Roman Catholic	10	2	8
Liturgical Protestant	12	5	7
Other Protestant	26	10	16
Fundamentalist	8	8	0
Jewish	3	0	3

* Fundamentalists have a significantly higher proportion of 0 and 1 scores than all other clergymen. $P = .0005$ by Fisher's exact test.

The counseling of non-parishioners is another indication of the clergyman's accessibility, and forty-nine of the fifty-nine clergymen report this activity. While it does not discriminate well among them, and is therefore not included in the boundary score, this accessibility of the clergyman distinguishes him, together with some church-sponsored charitable agencies, from secular helping agencies, all of which have formal criteria of admission.

II. FINDINGS

As the articulation of the clergyman's counseling role was the focus of this study, the boundary activity score was examined for its relationship with the other variables. It is not, however, considered to be an independent variable. All the relationships reported appear to be causally reversible.

As Table 2 shows, boundary-activity score is independent of denomination, except for the eight fundamentalists, all of whom have scores of 0 or 1. It seems reasonable to attribute the low scores of these pastors to the fact that they are leaders of small exclusive sects, but we will return to this point later. For the remainder of the clergymen in our sample, the boundary characteristics of the counseling role must be dependent on factors other than denomination and its associated theological differences.

Among the social-structural variables that have been linked to the clergyman's practices and attitudes are congregation size and socioeconomic class.[18] In Table 3

[18] See Fichter, *op. cit.*, and Russell R. Dynes, "Church-Sect Typology and Socio-economic Status," *American Sociological Review*, Vol. XX (October, 1955); Thomas F. Hoult, *The Sociology of Religion* (New York, 1958); Max Weber, *Essays in Sociology* (New York, 1946); Louis Bultena, "Church Membership and Church Attendance in Madison, Wisconsin," *American Sociological Review*, XIV (1950), 364–388; and Walter Goldschmidt, "Clan Denominationalism in Rural California Churches," *American Journal of Sociology*, LXI (1944), 348–55.

we see that boundary score is directly associated with the socioeconomic level of the congregation, and that within the working-class churches size is also associated. It may be, however, that size is itself related to status; the larger working-class churches tend to have stable congregations while the smaller ones are more likely to be "store-front" churches with impoverished or transient congregations.

Bearing in mind that clergymen of the largest middle-class churches have the most permeable role boundaries and those of the smallest working-class churches the least, we note that the twenty small working-class churches include seven of the eight fundamentalists, the remaining thirteen being distributed among all the remaining denominations except Roman Catholic. Controlling for church size and socioeconomic level, we find that although six of these seven fundamentalists have scores of zero, compared with six of the thirteen other remaining clergymen, this difference is not significant.[14] In short, for all churches, differences in the boundary activity appear to be associated with differences in characteristics of congregations other than denomination.

When the characteristics of the clergymen are examined, differences in boundary score are not found to be significantly associated with age. The proportion of young clergymen, 23–34 years of age, having high boundary scores is 60 per cent, which is approximately the same as the comparable proportion of 67 per cent of the clergy who are 60 years of age and older. The proportion having high boundary scores of those who are 35–44 is 52 per cent, and of those who are 45–59, 62 per cent. This is surprising because younger clergymen might be expected to have received training for their counseling roles that would bring them into contact with social workers, physicians, psychologists, and so on, and, because of this, might possibly have become integrated into the deviance-controlling system. Details of each clergyman's training as a counselor were not available, however, so a direct test of this hypothesis is not possible.

The clergymen were divided into two groups according to whether or not they had received higher education at other than a theological or Bible school. While their level of education was found to be associated with boundary score, a much closer association was found between boundary score and the *concordance* between the clergyman's education and the socioeconomic status of his congregation.[15] Educated clergymen in middle-class churches

TABLE 3

NUMBER OF CLERGYMEN BY BOUNDARY-ACTIVITY SCORE AND CONGREGATION SIZE AND CLASS

Congregation Type	No.	Boundary Score	
		0–1	2–3
All congregations	59	25	34
Middle-class congregations	31	3	28*
More than 1,000 members	16	1	15
Less than 1,000 members	15	2	13
Working-class congregations	28	22	6*
More than 1,000 members	8	3	5†
Less than 1,000 members	20	19	1†

* Middle-class clergy have more high scores than working-class. $\chi^2 = 25.8$, degrees of freedom = 1, $P < 1.0001$.

† Clergymen of large working-class congregations have more high scores than those of small congregations. $P = .003$, Fisher's exact test.

[14] We have suggested above that the fundamentalists are qualitatively different from other denominations, but it is possible that, as they are the smallest and poorest churches, they represent only the final type in a series with graded boundary permeability. The numbers are so small, however, that the issue remains open.

[15] The middle-class concordant clergy includes: four Roman Catholic, five Episcopal, three other liturgical, sixteen Protestant, and three Jewish. The working-class discordant group includes: six Roman Catholic, one Episcopal, two other liturgical, five Protestant, one fundamentalist. The clergy concordant with working-class congregations includes: five Protestant, seven fundamentalist, and one liturgical.

and less-well-educated clergymen in working-class churches are considered concordant with their congregations. Educated clergymen in working-class congregations were considered discordant. There were no less-well-educated clergymen in middle-class congregations. An examination of the boundary scores of the clergymen classified according to their concordance with their congregation reveals that twenty-eight of the thirty-one clergy concordant with their middle-class congregations have high boundary scores, as compared with six of the fifteen clergy discordant with their working-class congregations and none of the thirteen clergy concordant with their working-class congregations. Comparing just the two working-class groups, the boundary score is significantly higher ($P = .018$, Fisher's exact test) among the discordant group than among the concordant group. The clergymen of middle-class congregations have been shown in Table 3 to have higher boundary scores than those of working-class congregations. We see, then, that boundary score is strongly associated with concordance between the clergyman and his congregation, even when class is held constant.

Clergymen with high boundary scores report a higher rate of counseling activity[16] than those with low scores. Of the seventeen with a large case load, fifteen have high boundary scores. At the other extreme, only three of the fifteen with small case loads have high boundary scores. The group with medium case loads occupies an intermediate place with sixteen of the twenty-seven having high boundary scores. These differences are statistically significant ($z = 3.91$; $P < .00005$, Wilcoxon signed ranks). In addition, the clergy with high boundary scores also report more types of problems in all three categories. This may mean that the impermeable boundary reduces the numbers of applicants, or it may mean that there are few applicants and thus the clergyman is not called upon to make the kind of contact that would give him a high boundary-activity score. Furthermore, those clergymen who have more contacts with the rest of the deviance-controlling system and who also see a larger number of clients may discriminate more finely between similar problems and thus tend to report a larger number of types. For example, a clergyman who counsels one marital problem a year calls it just that; a clergyman counseling fifty marital problems and referring ten of them to other agencies may differentiate those associated with cultural conflict from those arising from sexual maladjustment, and so on.

The clergymen from this sample of churches, regardless of their boundary conditions, report that they refer many more clients to other agents than are referred to them and they refer to many agencies that never reciprocate with referrals to them. In all, they report referring to more than 50 agencies—and the average clergyman refers to 5.4. The most popular referral targets are Planned Parenthood Association, County Welfare Department, Alcoholics Anonymous, and physicians—all agencies specializing in a specific concrete service. The clergy themselves receive referrals from an average of only 0.63 agencies, most commonly from physicians. Many Protestant clergymen expressed discontent with this asymmetry of referrals.[17]

In other studies in this series, it has been noted that social workers appear to consider clergymen "too judgmental" and have complained of their inability to "give up" the client. Clergymen, on the other hand, appear to suspect social workers of an "amorality" that will undermine their parishioners' spiritual values. Some clergymen say that social workers "hold onto" clients longer than is necessary; they complain that social agencies will not give them

[16] The size of the clergyman's case load was ranked by two coders independently, using answers to all relevant interview questions. All differences were then resolved between the coders.

[17] For a discussion of this strain see George Todd Kalif, "Pastoral Use of Community Resources," *Pastoral Psychology*, November, 1950.

information that they need in order to help the parishioner.

In summary, we have found the following:

1. The hypothesis that the articulation of the clergyman's counseling role to the remaining deviance-controlling system is related to the characteristics of the congregation is lost regarding denomination, but upheld regarding size and socioeconomic status. This relationship is not, however, a simple one—concordance between education and socioeconomic status predicts boundary activity quite accurately, but discordance does not predict at all.

2. Clergymen with low boundary scores report less counseling and describe fewer types of problems brought for counseling. Nevertheless clergymen from all types of congregations report counseling all of the three major types of problems. Deviant behavior is referred frequently; transitional problems and exigencies, less frequently.

3. The clergyman refers more clients than he receives and he uses more agents than use him. This asymmetry is related to both the readiness of the average person to approach a clergyman for help and the tensions between clergymen and some other supporting and controlling groups.

We can now discuss some possible interpretations of these findings and in particular suggest new variables that might throw more light on the clergyman's counseling role. Before doing so, however, it should be repeated that the analysis rests upon the clergyman's image of his counseling role, and although for some purposes this does not matter, systematic distortions might lead to an erroneous picture of the clergyman's position in the total integrative system.

III. DISCUSSION

The findings of this study might be interpreted in a number of ways. It might be proposed that different norms develop in congregations of different social-structural types and that these norms might differ in regard to the amount of time properly spent by the clergyman on nonspiritual matters. Perhaps the most parsimonious interpretation, however, can be developed by considering the clergyman's role in the greater system and particularly the strains to which it is subject. These strains are of two kinds—those concerned with disparities between the clergyman's reference groups and his membership groups and those concerned with the position of the clergyman as counselor in the larger deviance-controlling system.

Strains between reference groups and membership groups.—The clergyman's membership in his congregation can be expected to have three important outcomes. First, he will develop, in interaction, some sentiments that he shares with them. Second, he will develop, again through interaction, diffuse, solidary bonds with some members, and third, he will be identified with his congregation by those outside it.

If the clergyman has a college education he will be likely to share some attitudes with other college-trained professionals including social workers, psychologists, and physicians; such a clergyman might be expected to appear on committees with other professionals and also to know them socially.[18] Referrals from clergymen of middle-class churches should be facilitated by shared values and personal friendships, and the larger the church, the more such bridges it is likely to have. Finally, social workers and psychiatrists who know a clergyman personally are more likely to

[18] Seven clergymen within our sample are board members of agencies affiliated with the Community Chest. Of these, six are from middle-class congregations, as we would expect. The lone clergyman from a working-class congregation seems a special case. He has retired from administrative positions in denominational agencies and has recently agreed to come out of retirement to minister to a small congregation that cannot afford to hire a full-time clergyman because it is in a depressed transitional area of town. His position in an agency board—a church affiliated one—seems to be a function of his past activity in agencies rather than his present ministry.

exempt him from stigma of "judgmental clergyman," and to consider him "professionalized" or "sophisticated," and thus to co-operate with him in the management of some clients. Such a relatively conflict-free situation may well lie behind the high boundary scores of the educated clergymen of middle-class congregations.

In contrast to these educated clergymen, those in working-class congregations cannot expect so high a level of compatibility among their own attitudes, those of their congregations, and those of the agency system. Such clergymen will not have access to outside resources through overlapping memberships, although they may have personal friendships among other professionals, share many viewpoints with them, and use them as reference groups. Many studies have shown, however, that these same professionals prefer middle-class clients,[19] and the clergyman, if he has personal contacts among agency professionals, will know this and perhaps be reluctant to risk a rebuff by interceding for his parishioners. Such a situation has the conditions necessary for a classical role conflict and may be the reason for the lesser use of the agency system reported by this group of clergymen. If, on the other hand, the clergyman does not have personal contacts among professionals, he will not be subject to the same conflict, but the professionals will tend to identify him with this congregation and be doubly reluctant to accept referrals of working-class clients from an unknown, working-class, and "judgmental" clergyman. If this is so, these latter clergymen should resemble the final group, the less-well-educated clergymen in the working-class churches, and a critical test of the formulation would lie in seeing whether the discordant clergymen who referred clients had personal contacts in the target agencies while those who did not refer had none.

[19] See Jerome K. Myers and Leslie Schaffer, "Social Stratification and Psychiatric Practice: A Study of an Out-Patient Clinic," *American Sociological Review*, XIX (June, 1954), 307–10.

In working-class churches where the clergyman's lack of higher education renders him concordant with his congregation, we would expect the clergyman to have a solidary relationship with his congregation but little access to the agency system. This, together with the agencies' own preference for middle-class clients, may explain the scant use made of outside resources by these clergymen.

It is interesting that less-well-educated clergymen do not appear in our sample in middle-class churches, although it is a theoretical possibility for some Protestant denominations. Such a situation may be inherently unstable because the clergyman would be expected to have difficulty establishing bonds with either the congregation or the agency system while at the same time bonds that existed between the latter groups might be expected to exclude him.

Strains between the clergyman and the rest of the controlling system.—We have hypothesized that role conflicts arise when certain kinds of clergymen minister to certain kinds of congregations. Some other types of strains can be thought of as affecting all clergymen because of the relationship of their role to the rest of the system.

The first type of strain seems to arise just because the clergyman is a familiar and accessible figure whom people feel they can approach. His accessibility forces him into a referral role. He sorts and sifts those who come to him, and allocates those that he cannot help to other services—often after a period of trial-and-error counseling. This allocation process involves defining the parishioner as sick or deviant, and he is almost always reluctant to do this when the problems are transitional or arise in exigencies. For example, clergymen often give financial help during temporary unemployment so that parishioners will be spared the humiliation of applying for public assistance. In other words, unless the clergyman is totally iso-

lated from the control system, his position in it imposes on him the obligation of allocating clients to services, and this involves him in decisions that are difficult to make. It also involves him in an automatic asymmetry with respect to the rest of the agency system. He cannot at once be the allocator and the target agent,[20] and so he perceives himself as giving more than he gets.

Besides the strain arising from the clergyman's position, strains inhere in the counseling function. Although there is an increasing specialization among agencies offering the so-called face-to-face services, it is questionable that counseling can be distinguished clearly from intensive casework. Because of haziness about where one leaves off and the other begins, considerable attention is paid to the training required for each. Pastoral counseling is perceived as requiring less training than casework, and so the social worker tends to regard the clergyman as an amateur. His feeling of responsibility for the total client is interpreted as "incomplete professionalization" and his willingness to deal with the respondent's moral well-being is seen as judgmental—a term of anathema. All of this need not lead to conflict if the clients qualifying for casework services were clearly different from those qualifying for counseling. Our studies suggest, however, that the clergyman and the social agency compete for what is often called the "motivated client," and this competition exacerbates the tension between the two groups. The helping task might be divided between clergymen and social workers either on the basis of function or on the basis of the target population, but as neither is done, each tends to regard the other redundant. Such solidarity as exists between clergyman and social worker is based on a commonality of interest, as we have argued above, and not on interdependence. The clergymen of this study appear to have much less strain in their dealings with physicians, lawyers, housing authorities, and other agents whose specific functions do not overlap with their own.

Finally, the membership of the clergyman in the group he counsels puts him in a position similar to that of the physician who will not treat his family and dislikes treating his friends. For all clergymen, this strain may act as an encouragement to refer. Those whose structural situation cuts them off entirely from the world of social agencies may be reducing the strain by circumscribing their counseling activities.

Returning to our point of departure—the division of labor among the integrative agents—we can see that the clergyman's position in the system involves him in a dual role. He is a counselor, but because he is approached so early in the search for help, he is called upon to allocate many clients to other agencies. The frustration that the clergyman experiences because of the asymmetry of his relationship with other agents appears to be offset by the rewards attendant upon his historic role as counselor—a role that he views as an intrinsic part of his ministry.

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[20] In this he differs from the physician. In our own studies we have found (see, e.g., n. 4) that the general practitioner is also a gatekeeper in the sense that people turn to him when informal resources fail, but he receives as many patients by referral as he sends to other agencies, and thus does not perceive himself as supplying other people with patients and receiving none in return.

ORTHODOXY, CHURCH PARTICIPATION, AND AUTHORITARIANISM[1]

JOHN D. PHOTIADIS AND ARTHUR L. JOHNSON

ABSTRACT

Data from a sample of 300 church members are analyzed to provide indirect evidence on the relationship between religiosity and ethnic distance, specifically on correlations between religious orthodoxy and church participation with authoritarianism viewed as an intervening variable. Other personality variables and formal education are used as controls by means of partial correlation analysis.

Findings suggest (1) that authoritarian and prejudiced persons tend to retain orthodox beliefs or to become strong orthodox believers and (2) that persons who are authoritarian and orthodox, as well as persons who are not, become more tolerant through extended church participation.

Research relating the contribution of religiosity to the development of attitudes toward minority groups is inconsistent and ambiguous.[2] Generally, this research has consisted of ex post facto studies employing hypotheses which when tested implicate rather than establish or demonstrate the validity of theoretical considerations. The difficulty of securing adequate control groups requires care in the manipulation and control of variables in studies claiming relationships based on indirect evidence.

This study is an extension of the analysis of data presented by Photiadis and Biggar, based on questionnaires completed by a sample of three hundred men and women living in a South Dakota community of about 8,000 and attending three Protestant churches. Photiadis and Biggar used three scales to define religiosity: orthodox belief, church participation, and the extrinsic-intrinsic belief continuum. Because they suspected that these religiosity variables are differentially related to personality variables known to be related to prejudice, they tested the relationship between religiosity variables and ethnic distance, controlling for six personality variables (anomia, status concern, conservatism, authoritarianism, withdrawal tendencies, and antisocial tendencies). In addition, formal education was controlled for because it was found to be related to both ethnic distance and personality variables as well as to religiosity.

Their data indicated that ethnic distance, as measured by the Bogardus Social Distance Scale, is related negatively to church participation but positively to orthodox belief, the latter being true only when the influence of church participation is held constant. The positive relationship between orthodoxy and ethnic distance was explained as possibly due to the intervening role of authoritarianism. Of all the personality variables used, authoritarianism[3] is the only one found to be related positively and significantly to ethnic distance when the remaining variables—those of personality, formal education, and religiosity—were controlled. Frenkel-Brunswik[4] sug-

[1] This paper deals with further analysis of data presented by J. Photiadis and J. Biggar in a previous article, "Religiosity, Education and Ethnic Distance," *American Journal of Sociology*, LXVII (May, 1962), 666–72.

[2] For studies of this nature and for description of the variables in the following paragraph, see *ibid.*

[3] The following five attributes have been used to define authoritarianism: (1) The most important thing to teach children is absolute obedience to their parents. (2) Any good leader should be strict with people under him in order to gain their respect. (3) There are two kinds of people in the world: the weak and the strong. (4) Prison is too good for sex criminals. They should be publicly whipped or worse. (5) No decent man can respect a woman who has had sex relations before marriage. The authoritarianism scale was a five-item version of the California F-Scale developed by the Department of Scientific Research of the Jewish Committee.

[4] Else Frenkel-Brunswik, "Comprehensive Scores and Summary of Interview Results," in T. W. Adorno *et al.*, *The Authoritarian Personality* (New York: Harper & Bros., 1950), pp. 468–86.

gests that the relationship between orthodoxy and ethnic distance exists because orthodox believers conditioned by an authoritarian doctrine became authoritarian, and in turn prejudiced, because of the relationship between authoritarianism and prejudice. The negative relationship between ethnic distance and church participation was retained regardless of which variables were controlled. These two reverse relationships (the negative relationship between church participation and ethnic distance and the positive relationship between orthodoxy and ethnic distance) support Allport's contention that the role of religion is dual.[5]

The analysis presented here tests the hypothesis that religious orthodoxy and church participation are differentially related to ethnic distance through a differential relationship with the intervening variable of authoritarianism. The hypothesis is tested by controlling for formal education and five personality variables through partial correlation analysis.

FINDINGS

Zero-order correlations showing the relationship between orthodoxy and each of the personality variables were presented by Photiadis and Biggar.[6] These correlations indicated that all six personality variables were related positively to orthodoxy, although status concern and withdrawal tendencies were not significantly so. Table 1 presents the relationship between each personality variable and orthodoxy when the remaining personality variables, formal education[7] (which has been found to be closely related to them), and church participation[8] are held constant. When these variables are held constant, anomia, conservatism, withdrawal tendencies, and antisocial tendencies are not related to orthodoxy. The same is true of these variables when correlated with ethnic distance. In zero-order correlations their relationships with ethnic distance are positive and significant, but when the remaining variables are controlled for, these relationships disappear.[9] Status concern, found unrelated to orthodoxy in a zero-order correlation, is found to be related to it negatively when the remaining variables are controlled (Table 1).

TABLE 1

SEVENTH-ORDER PARTIAL CORRELATIONS AND THEIR SIGNIFICANCE, WITH ORTHODOXY AS DEPENDENT VARIABLE

Partial Correlation Coefficient	F Test*	Conclusion
$X_{a1\cdot234567b} =$ .050	.717	Orthodoxy not significantly related to anomia.
$X_{a2\cdot134567b} =$ −.158	7.418	Orthodoxy related negatively to status concern.
$X_{a3\cdot124567b} =$ .074	1.591	Orthodoxy not significantly related to conservatism.
$X_{a4\cdot123567b} =$ .204	12.663	Orthodoxy related to authoritarianism positively.
$X_{a5\cdot123467b} =$ .034	.345	Orthodoxy not significantly related to withdrawal tendencies.
$X_{a6\cdot123457b} =$ −.0002	.000	Orthodoxy not significantly related to antisocial tendencies.
$X_{a7\cdot123456b} =$ −.228	15.932	Orthodoxy negatively related to education.
$X_{ab\cdot1234567} =$ .246	18.796	Orthodoxy related to participation positively.

* An F value greater than 3.84 and 6.64 is required for significance at the 5 and 1 per cent levels respectively, with 1 and 291 degrees of freedom.

[5] Allport suggests that religion both makes and unmakes prejudice! While the creeds of the great religions are universalistic, all stressing brotherhood, the practice of these creeds is frequently divisive and brutal (Gordon Allport, *The Nature of Prejudice* [Reading, Mass., Addison-Wesley Publishing Co., 1955], pp. 444–46).

[6] Photiadis and Biggar, *op. cit.*, p. 670, Table 1.

[7] *Ibid.*

[8] These variables have been controlled for by the use of partial correlation analysis (*ibid.*, p. 669).

[9] *Ibid.*, Tables 1 and 2.

Formal education, in both a zero-order correlation and a partial correlation, is negatively related to orthodoxy (Table 1) and negatively related to ethnic distance.[10]

Authoritarianism is the only personality variable related positively to orthodoxy when the remaining variables are controlled for. It is also the only variable related positively to ethnic distance when remaining variables are controlled.

By indicating then that authoritarianism is related in a similar fashion to orthodoxy and ethnic distance, the above data support the thesis that orthodoxy is related to ethnic distance through the intervening role of authoritarianism.

This finding supports that part of Frenkel-Brunswik's thesis which suggests that orthodoxy and authoritarianism are related. She also suggests, however, that this relationship exists because authoritarian individuals are conditioned by the orthodox doctrine. This latter part of her thesis could be discounted on the grounds that orthodoxy does not precede authoritarianism, but rather, that authoritarian individuals are simply attracted by a doctrine that is authoritarian. Further analysis of data on church participation supports this second alternative.

Orthodoxy and church participation have been found closely related both with a zero-order correlation[11] and with remaining variables controlled. Social theorists have dealt extensively with this relationship, using ritual instead of church participation. Church participation is a form of ritual.[12] For example, Kingsley Davis suggests that "ritual helps to remind the individual of the holy realm, to vivify and strengthen his faith in this realm."[13] Concerning the type of ritual in which church participation belongs, he suggests that "ritual is especially effective if performed by several individuals, because then the factor of group stimulation (plus dramatic continuity) heightens the emotionality and makes the subjective impression all the more vivid."[14] Durkheim[15] and Chapple and Coon[16] discuss these two elements as being mutually dependent. They refer to the ritual as a conditioning factor reinforcing the individual's religious beliefs which in turn increase his participation in ritual.[17]

Accepting the point that church participation and orthodox belief are mutually dependent and reinforce each other, one should also expect from Frenkel-Brunswik's thesis that orthodoxy leads to authoritarianism, to find a relationship between authoritarianism and church participation. The present data, however, do not show such a relationship to exist, nor is there any relationship between church participation and the remaining personality variables. In the case of a zero-order correlation, church participation was not related to any of these personality variables except status concern,[18] where the relationship was negative.

Since these personality variables are closely interrelated,[19] it is necessary to test

[10] *Ibid.*, Table 2.

[11] *Ibid.*, p. 670, Table 1.

[12] According to Davis, "ritual is the active side of religion . . . it can include any kind of behavior known, such as weaving of special clothing, the recitation of special formulas . . . singing, dancing, weeping . . . the religious character of the behavior . . . does not come from the behavior itself but from the attitudes taken towards it" (Kingsley Davis, *The Human Society* [New York: Macmillan Co., 1958], p. 534).

[13] *Ibid.*

[14] *Ibid.*

[15] Émile Durkheim, *The Elementary Forms of Religious Life* (New York: Macmillan Co., 1915), p. 101.

[16] E. Chapple and C. Coon, *Principles of Anthropology* (New York: Henry Holt & Co., 1942), chap. iii.

[17] For Durkheim "these two elements of religious life are too closely connected with each other to allow any radical separation. In principle, the cult is derived from beliefs yet it reacts upon them" (*op. cit.*, p. 101). Chapple and Coon discuss this mutual dependence in terms of Pavlov's experiment and the conditioned response theory of learning (E. Chapple and C. Coon, *op. cit.*, chap. iii).

[18] Photiadis and Biggar, p. 670, Table 1.

[19] *Ibid.*, p. 670, Table 1.

whether this lack of relationship between them (authoritarianism in particular) and church participation is not due to mutually exclusive attributes. When each relationship is tested, controlling for the remaining variables (Table 2), none of the variables, including authoritarianism, appears to have any relationship to church participation, as was true in the zero-order analysis. Assuming then that orthodox belief and church participation are mutually dependent, one could say that these data more readily support the thesis that authoritarian individuals are more attracted by the orthodox doctrine, than the thesis that orthodox believers become authoritarian through being conditioned by a doctrine that is authoritarian.[20] Allport,[21] discussing authoritarianism and its relationship to prejudice, suggests that it is mainly a product of early socialization.

Finally, when Tables 1 and 2 are examined together they offer further information as to the source of what Allport calls the dual role of the church in group relations. This role was explained previously in terms of two reverse relationships: the positive relationship between orthodoxy and ethnic distance, and the negative relationship between church participation and ethnic distance.[22] The present data offer further insight into this dual role of religion, suggesting that orthodoxy is related positively not only to ethnic distance but also to authoritarianism, which in turn is related to ethnic distance. Church participation, on the other hand, is related negatively to ethnic distance and is unrelated to authoritarianism. In other words, the dual role of religion exists because strong orthodox believers are less tolerant and more authoritarian, while high participants in church are more tolerant (which may be due to the teachings of these churches), but not different concerning authoritarianism.[23]

TABLE 2

SEVENTH-ORDER PARTIAL CORRELATIONS AND THEIR SIGNIFICANCE, WITH PARTICIPATION AS DEPENDENT VARIABLE

Partial Correlation Coefficient	F Test*	Conclusion
$X_{ba \cdot 1234567} =$ .246........	18.796	Participation related to orthodoxy positively.
$X_{b1 \cdot a234567} = -$.036........	.367	Participation not significantly related to anomia.
$X_{b2 \cdot a134567} = -$.088........	2.286	Participation not significantly related to status concern.
$X_{b3 \cdot a124567} = -$.005........	.008	Participation not significantly related to conservatism.
$X_{b4 \cdot a123567} = -$.046........	.616	Participation not significantly related to authoritarianism.
$X_{b5 \cdot a123467} = -$.036........	.377	Participation not significantly related to withdrawal tendencies.
$X_{b6 \cdot a123457} =$ .038........	.415	Participation not significantly related to antisocial tendencies.
$X_{b7 \cdot 123456} =$ .128........	4.846	Participation related to education positively.

* An F value greater than 3.84 and 6.64 is required for significance at the 5 and 1 per cent levels respectively with 1 and 291 degrees of freedom.

[20] It is plausible that orthodoxy leads to a certain degree of authoritarianism. Such an effect, however, is not indicated with the present data, either because it is not significant, or because it is offset by some other element such as an aspect of the doctrine which suggests non-authoritarian behavior. For example, one teaching could foster authoritarianism by saying, for instance, that Jesus is the Son of God and there is no argument about it, thus reinforcing the black-and-white attitude. On the other hand, another teaching may suggest leniency for sins similar to those included in certain of the five items measuring authoritarianism (see n. 5); for instance, such a teaching as the forgiveness by Jesus of Mary Magdalene and his love and forgiveness of children.

[21] Gordon Allport, "Prejudice: Is It Societal or Personal?" *The Journal of Social Issues*, XVIII, No. 2, 120–34.

[22] Photiadis and Biggar, *op. cit.*, pp. 666–72.

[23] We might speculate on the type of participation that influences authoritarianism and the types

In conclusion one could say that concerning the role of the church in group relations, these data support, but do not prove, the thesis that a statistically significant number of more authoritarian individuals (who because of their authoritarianism are also more prejudiced) have the tendency to become strong orthodox believers or to retain already held orthodox beliefs. Individuals who are more authoritarian and prejudiced, as well as individuals who are less authoritarian or less orthodox, become more tolerant by extended participation in church activities.

UNIVERSITY OF MINNESOTA

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of personalities who participate. Compartmentalized or integrated religious activity and attitudinal orientations are plausible interpretations, which are incapable, however, of being tested by these data.

THE LOCAL-COSMOPOLITAN SCIENTIST[1]

BARNEY G. GLASER

ABSTRACT

In contrast to previous discussions in the literature treating cosmopolitan and local as two distinct groups of scientists, this paper demonstrates the notion of cosmopolitan and local as a dual orientation of highly motivated scientists. This dual orientation is derived from institutional motivation, which is a determinant of both high quality basic research and accomplishment of non-research organizational activities. The dual orientation arises in a context of similarity of the institutional goal of science with the goal of the organization; the distinction between groups of locals and cosmopolitans derives from a conflict between the two goals.

Several studies in the sociology of occupations and of organizations have concluded that some professionals in organizations tend to assume a "cosmopolitan" orientation that manifests itself in their working for professional goals and the approval of colleagues throughout their professional world, in focusing on a professional career, and in a concomitant lack of loyalty to and effort for the organization. Other professionals tend to assume a "local" orientation that manifests itself in their lesser commitment to the profession, in more concern with the goals and approval of the organization, and in focusing on an organizational career.[2] With the growing movement of scientists into research organizations, there has been some interest by sociologists of science in studying the many problems and strains generated by the often conflicting professional and organizational demands and practices that, in turn, generate the adoptive cosmopolitan and local types of orientations.[3] A partial list of these problems might include varying incentive systems, differential emphasis on publication of research results, types of authority and supervision related to the professional need of autonomy, divergent and conflicting influences on work situations, assignments and research problem choices, budgets of time and money, kinds of compatible work groups, focus of performance, multiple career lines and commitments.

The major goals of many research organ-

[1] Revised version of a paper delivered at the 1962 meetings of the American Sociological Association. I am indebted to the encouragement of Alvin W. Gouldner and the editorial help of Anselm L. Strauss in preparation of this paper.

[2] The terms "cosmopolitan" and "local" were first used by Merton to describe different types of community leaders (Robert K. Merton, *Social Theory and Social Structure* [Glencoe, Ill.: Free Press, 1957], pp. 387–420). For a formulation of cosmopolitan and local as organizational types see Alvin W. Gouldner, "Cosmopolitans and Locals: Toward an Analysis of Latent Social Roles," *Administrative Science Quarterly*, II (1957–58), 281–306, 444–80; see also Alvin W. Gouldner, "Organizational Analysis," in Robert Merton, Leonard Broom, and Leonard Cottrell (eds.), *Sociology Today* (New York: Basic Books, 1959), pp. 410–19. For particular studies, see Peter M. Blau and W. Richard Scott, *Formal Organizations* (San Francisco: Chandler Publishing Co., 1962), pp. 64–74; Leonard Reissman, "A Study of Role Conceptions in Bureaucracy," *Social Forces*, XXVII (1949), p. 308; Theodore Caplow and Reece J. McGee, *The Academic Marketplace* (New York: Basic Books, 1958), p. 85 and *passim;* Harold Wilensky, *Intellectuals in Labor Unions* (Glencoe, Ill.: Free Press, 1956), pp. 129–53; Warren G. Bennis *et al.*, "Reference Groups and Loyalties in the Out-Patient Department," *Administrative Science Quarterly*, II (1958), pp. 481–500.

[3] William Kornhauser, *Scientists in Industry* (Berkeley: University of California Press, 1962), esp. chap. v; Simon Marcson, *The Scientist in American Industry* (New York: Harper & Bros., 1960); Donald C. Pelz, "Some Social Factors Related to Performance in a Research Organization," in Bernard Barber and Walter Hirsch (eds.), *The Sociology of Science* (New York: Free Press of Glencoe, 1962), p. 357; Herbert A. Shepard, "Nine Dilemmas in Industrial Research," *Administrative*

izations, particularly industrial research organizations,[4] are, of course, not consistent with the major institutional goal of science: advancing knowledge by basic research. They often emphasize goals of application, product development, and expert service. The scientist seeking a professional career (one based on pursuing the institutional goal) in an organization of this type becomes a "cosmopolitan," by and large directing his efforts to professional goals, rewards, and careers. Insofar as the cosmopolitan is always looking within the community of research organizations for better professional positions and conditions[5] and has little "local" loyalty to inhibit his mobility, the result is a high organizational turnover. A professional career may be impeded by a too long stay in the industrial context. Indeed, insofar as the industrial organization needs basic research, it becomes detrimental for it to try to induce the cosmopolitan to focus his efforts on the major organizational goals—product development, application, and service—since that refocusing may reduce the quality of his basic research contributions.[6]

Whereas studies of industrial research organizations have usually found scientists who have either a primarily local *or* cosmopolitan orientation, I shall try to demonstrate a local-cosmopolitan orientation among highly motivated scientists in an organization devoted to the institutional goal of science. The congruence of goals reduces in considerable measure, if not completely the strains between organizational and professional requirements that tend to generate distinct local and cosmopolitan types. My principal criterion for ascertaining the general orientation of these investigators will be the directions of their work effort. First, I investigate the general performance-reward process of science; then I investigate the efforts of those who do well in their scientific performance to meet organizational demands. From these findings on their professional and organizational contributions, I infer that the orientation of these scientists is both local *and* cosmopolitan. I conclude with a discussion of the implications of this formulation for the developing theory about local and cosmopolitan orientations of professionals in organizations.

The data for the analysis consist of answers given to survey questionnaires in 1952 by the total resident research staff (332) of a large government medical research organization devoted to basic research.[7] In addition, some letters and documents give further information on the organization. My demonstration will be an effort to *explore* for plausible relations between variables, *not* to develop a strong case built on hard fact. While secondary analysis is well suited for exploratory work, to achieve the latter with old data is probably impossible. Accordingly, I shall use somewhat crude indexes and consider small differences that are consistent, highly suggestive, and that lead to an integrated picture of the local-cosmopolitan process. Since I am only suggesting, not testing, my language will not be riddled by the qualification rhetoric required in more rigorous demonstrations; my inferences will be de-

Science Quarterly, I (1956), 346; Hollis W. Peter, "Human Factors in Research Administration," in Rensis Likert and Samuel P. Hayes, Jr. (eds.), *Some Applications of Behavioral Research* (Paris: UNESCO, 1957), p. 142; Clovis Shepard, "Orientations of Scientists and Engineers," *Pacific Sociological Review*, Fall, 1961, p. 82. Robert Avery, "Enculturation in Industrial Research," *IRE Transactions in Engineering Management*, March, 1960, pp. 20–24; Fred Reif, "The Competitive World of the Pure Scientist," *Science*, CXXXIV (1961), 1959.

[4] Kornhauser, *op. cit.*, p. 133; Leo Meltzer, "Scientific Productivity in Organizational Settings," *Journal of Social Issues*, No. 2 (1956), p. 38; Marcson, *op. cit.*, pp. 81–82, 104; Shepard, *op. cit.*, p. 347.

[5] Kornhauser, *op. cit.*, p. 130.

[6] *Ibid.*; see also Shephard, *op. cit.*, and Pelz, *op. cit.*, p. 358.

[7] I am indebted to Donald C. Pelz of the Survey Research Center, University of Michigan, for providing me with these data.

signed to guide future research on local-cosmopolitan theory along (I believe) useful lines; and my primary effort will be to generalize as opposed to describing a real situation in detail.

THE PERFORMANCE-REWARD PROCESS

Motivation.—In the institution of science perhaps the most important goal for the *typical* scientist is to advance the knowledge in his field by some form of basic research. A scientist, especially in training but throughout his career, is constantly reminded by colleagues that it is his job to advance knowledge by some increment, large or small. He internalizes the goal, and becomes, using Parson's term, "institutionally motivated" to achieve it.[8] Therefore, before we know anything about the distinctive personality of this or that scientist, we can hypothesize that to some degree he will be motivated to advance knowledge by virtue of his professional training and that his research performance will tend to vary directly with the degree of his institutional motivation.

Insofar as the research scientist is motivated to advance knowledge, both his *research work* on problems, hypotheses, and methods as well as his *results* are centrally involved because he has the potential for advancing knowledge at either stage.[9] Irrespective of failures in results, he may have been quite original in his research work, and vice versa, he may have run rather a routine project into a contributing result.

As a measure of motivation to advance knowledge, I have selected the following two items that tap the (*a*) work and (*b*) result stages of the advancing knowledge process.[10]

"How much do you want? How important is (it) to you?"

(*a*) Freedom to carry out my own ideas; chance for originality and initiative.

(*b*) Contributing to basic scientific knowledge.

Degree of importance: (1) utmost, (2) considerable, (3) some or little, (4) no opinion.

Over half the investigators felt both freedom and contributing were of the utmost importance. Each item was dichotomized between "utmost" and the remaining categories since this was where the direction of association consistently changed in cross-classification with criterion variables. The two items were fairly strongly related (coefficient of association = .70). Investigators were considered to have high motivation if they felt both freedom in work and contributing results were of the utmost importance. Fifty-six per cent (186) of them were in this category. Among those of lower motivation, 27 per cent (89) were high on one item and 17 per cent were low on both items.

For further analysis I dichotomized the index into high and low, distinguishing those who were high on both items from all others. Three justifications for this are: (1) In many cross-classification checks,

[8] Institutional motivation has been dealt with extensively in: Talcott Parsons, *Essays in Sociological Theory* (Glencoe, Ill.: Free Press, 1954), chaps. ii, iii, Merton, *op. cit.*, pp. 214, 531, 555, 558–59; Robert K. Merton, "Priorities in Scientific Discovery," *American Sociological Review*, December, 1957, pp. 640–41. It should be noted that advancing knowledge as I deal with it here is institutional, a part of a normative pattern, not a mode of orientation that is simply natural to man. Thus, I make the distinction between institutional motivation (motivation based on internalized norms and goals) and typical human motives (assertive, friendly, ambitious, egotistic, etc.) as elements of concrete motivation.

[9] Advancing knowledge is a process that, for any one scientist, is composed of many events. This process has at least two broad stages: research work and research results. Bernard Barber, in talking of "inventions and discoveries," says "they have two aspects, that of process and that of products, and these aspects must be distinguished" (*Science and the Social Order* [Glencoe, Ill.: Free Press, 1952], p. 193).

[10] I follow the procedure for index construction outlined and discussed by Paul F. Lazarsfeld, in Merton, Broom, and Cottrell (eds.), *op. cit.*, chap. ii, pp. 47–67; in "Evidence and Inference in Social Research," *Daedalus*, LXXXXVII, No. 4 (1958), 100–109; and with Wagner Thielens, *The Academic Mind* (Glencoe, Ill.: Free Press, 1958), pp. 402–7.

the middle group proved to be more like those low on both items than those high on both items. Therefore, the index is reducible on statistical evidence.[11] (2) We only need a dichotomized variable to establish general relations between variables. (3) The dichotomization is at the median, saving cases for necessary cross-tabulation.

Performance.—The performance score (developed three months after the survey data were collected) consisted of the assessments by colleagues in the work situation of each investigator's current research.[12] Each assessment was based on five criteria: (1) originality and creativeness, (2) wisdom and judgment, (3) rigor of thought and precision of methods, (4) persistence, industriousness, and efficiency, and (5) contribution to the work of others. Three criteria (2, 3 and 4) focus directly on the research work, and two (1 and 5) focus mainly on research results. Thus, this index is based on the same aspects of advancing knowledge as the motivation index. Bearing out my hypothesis on the positive relation between motivation and performance, 19 per cent more of the highly motivated scientists (compared to those with less motivation) have been judged by their colleagues to have high quality performance.

Recognition.—Concomitant with the development of institutional motivation is the expectation of reward for achievement of the institutional goal.[13] The strong institutional emphasis of science on this achievement-reward pattern is noted by Merton: "originality can be said to be a major institutional goal of modern science at times the paramount one, and recognition for originality a derived, but often as heavily emphasized, goal."[14]

The institutional emphasis on professional recognition holds for the research organization under study.[15] A memo to all personnel described the promotion process as follows:[16] The immediate supervisor recommends the investigator to the institute director for promotion. If the latter agrees, he recommends the investigator's case to the promotion board. The board then thoroughly examines the investigator. A sample of his publications is read; prior and current supervisors are asked about him; and his qualifications are judged in terms of the following criteria: (1) quality of work he has been engaged in, (2) capacity to develop, (3) capability in relation to other investigators, (4) reputation in his field, (5) personal characteristics and ability to get along with others, and (6) ability in the non-scientific work associated with his present and prospective position. If he passes this examination, he is recommended for promotion to the director of the organization, who follows the advice of the board in most cases.

The first four criteria clearly relate to

[11] On reduction of property space see Alan Barton, "The Concept of Property Space in Social Research," in Paul F. Lazarsfeld and Morris Rosenberg (eds.), *The Language of Social Research* (Glencoe, Ill.: Free Press, 1955).

[12] This performance score cannot be construed as a measure of recognition, since, to be sure, the scientists were not made aware by the research team of their colleagues' evaluations. The essence of recognition is that it is a *known* reward for one's work. For a complete discussion of the construction of this index of research performance see Donald C. Pelz *et al., Human Relations in a Research Organization* (Vol. II; Ann Arbor: Institute for Social Research, University of Michigan, 1953), Appendix C; and *Interpersonal Factors in Research* (Ann Arbor: Institute for Social Research, University of Michigan, 1954), Part I, chap. i, Appendix A.

[13] See Parsons, *op. cit.*, pp. 53–54, 143–44, 230–31, 239, for the formulation that institutional norms reciprocally define relations between two classes of people or two positions.

[14] Merton, "Priorities . . . ," *op. cit.*, p. 645.

[15] This is not the only government medical research organization that bases promotions on professional recognition. There would seem to be many others. Meltzer reports for his national sample of 3,000 physiologists that publication productivity for those in government was the same as those in the university and that publication was as strong a factor in promotions in both contexts (Meltzer, *op. cit.*).

[16] Charles V. Kidd, "Resolving Promotion Problems in a Federal Research Institution," *Personnel Administration*, XV, No. 1 (1952), 16.

the investigator's professional recognition by focusing on his past, present, and potential ability to advance knowledge. I have shown elsewhere that professional recognition is also positively linked with getting along with others and with accomplishing non-scientific work.[17] Given the emphasis on professional recognition for advancement, it seems reasonable to assume that this reward (recognition) for achievement will maintain motivation for further achievement.

The promotion process clearly indicates the importance of two types of professional recognition: (1) the immediate supervisor's evaluation and (2) publications. Therefore if each type of recognition is measured and combined in an index, we can approximate completeness in measuring both the fundamental range of professional recognition required by the organization, and an important patterned form of professional recognition for research work and results. Thus all three indexes are based on the two broad stages of advancing knowledge.

The questionnaire did not include information on actual supervisor's evaluation nor did it include information on actual publications (extent or quality). It did include two items that measure *felt* recognition from supervisors and in publications. They are:

A. "How do you feel about the way your chief makes *evaluations* about the *quality of work you are doing?*" (1) accurate, (2) partly accurate, (3) no attempt, (4) no answer.

B. "In scientific or other professional papers about work to which you have made some contribution, *is proper credit given to your own contribution* by means of authorship or acknowledgment?" (1) always, (2) usually, (3) seldom, (4) no opinion.

Over half the investigators feel they receive adequate recognition from the supervisor (53 per cent say "accurate") and in publications, whether by authorship or acknowledgment (72 per cent say "always"). To construct an index of felt professional recognition I have dichotomized each item between the highest category and all others. This dichotomization occurs as close to the median as possible, and at a statistical breaking point. In many cross-classifications of each item with other variables, the direction of association consistently changed between the highest category and the remaining categories. In combining these two variables into an index of felt recognition, 44 per cent of the investigators are high on both items; 37 per cent of the investigators are high on one item; and 19 per cent are low on both items. I have dichotomized the index between high and all others (low) for the identical statistical and substantive reasons earlier applied to the motivation index.

As suggested, professional recognition tends to maintain institutional motivation in this organizational context. Nineteen per cent more of those scientists who feel they have achieved high recognition (compared to those with low recognition) are highly motivated to advance knowledge.[18]

Process.—The next step is to show in one table the following process: recognition for advancing knowledge (which indicates past performance) tends to maintain motivation (a time sequence based on common observation), which in turn tends to result in high quality research performance (measured three months later). This will give us the basic links of the circular, general performance-reward process in science: research performance leads to professional recognition, which maintains motivation to

[17] See my *Organizational Scientists: Their Professional Careers* (Indianapolis: Bobbs-Merrill, forthcoming), chaps. vi and vii, and see below for the relation of performance process to accomplishing non-scientific work.

[18] For other evidence that recognition supports motivation see Donald C. Pelz, "Motivation of the Engineering and Research Specialists" (*General Management Series,* No. 186 [New York: American Management Association], p. 30). He reports that for a national sample of 3,000 physiologists, the number of publications and acknowledgements is positively related to intensity of motivation.

advance knowledge, which in turn leads to more performance.

In Table 1 the magnitude of association between recognition and performance is *diminished* when the intervening effect of motivation is removed. Therefore, high motivation tends to be a link between attaining recognition and accomplishing high quality research performance, thus tentatively demonstrating the performance process.[19] As a social pattern, this circular process will continue if the performance measured here results anew in recognition.

TABLE 1

RECOGNITION, MOTIVATION, AND PERFORMANCE

	Recognition (Per Cent)		
	Average	Less	Difference
High performance....	56 (144)	44 (188)	+12
Proportion with high performance and:			
High motivation...	60 (96)	53 (90)	+ 7
Low motivation....	46 (48)	37 (98)	+ 9

At this point I wish to suggest that, besides research performance, it is also possible to predict behavior associated with research on the basis of intensity of institutional motivation. This is borne out by one indicator of research behavior: the amount of time in a typical work week the scientist puts into "performing my own professional work (or work under the guidance of my chief) such as research, professional practice, professional writing, etc." Fifteen per cent more of the highly motivated investigators work 21 hours a week or longer on personal research. Furthermore, 11 per cent more of those who work 21 or more hours a week on their own research have a high quality performance score.

TABLE 2

MOTIVATION, RESEARCH TIME, AND PERFORMANCE

	Motivation (Per Cent)		
	High	Low	Difference
High performance....	57 (186)	38 (146)	+19
Proportion with high performance who put:			
Twenty-one or more hours per week into own research..	60 (142)	43 (89)	+17
Less than twenty-one hours per week into own research..........	48 (44)	35 (57)	+13

In combining motivation, personal research time, and performance, Table 2 demonstrates that the highly motivated investigators will tend to put more time into their own research work, and that this time, in turn, will tend to result in higher quality performance.[20] The magnitude of associa-

[19] Various sources exist for a full discussion of Lazarsfeld's elaboration analysis of which this is the MI type. For the primary source see Paul F. Lazarsfeld, "Interpretation of Statistical Relations as a Research Operation," in Lazarsfeld and Rosenberg (eds.), *op. cit.*; see also Lazarsfeld and Patricia L. Kendall, "Problems of Survey Analysis," *Continuities in Social Research*, eds. Lazarsfeld and R. K. Merton (Glencoe, Ill.: Free Press, 1950), and Herbert Hyman, *Survey Design and Analysis* (Glencoe, Ill.: Free Press, 1955), chap. vii. One could say that the table also shows that motivation leads to recognition, which in turn leads to performance (14 per cent and 16 per cent are less than 19 per cent). But this is the same process I have described in the text. For motivation to result in recognition implies that there was some performance intervening; for recognition to lead to performance implies that there was some motivation intervening.

[20] I have based this finding on the one-time sequence. It is also possible that some investigators may have developed a high degree of motivation because of putting in more than 21 hours per week. Hard work could generate interest. Therefore, we may have another time sequence in the performance process of longer hours in research leading to

tion between motivation and performance is *diminished* when the intervening effect of personal research time is removed.[21] This finding adds a subsidiary link to the performance-reward process that is diagramed in Figure 1.

SCIENTISTS AS ORGANIZATIONAL MEN

As a link in the performance process, time in own research has direct relevance to the research organization. Insofar as this process supposedly results in the continual fulfilment of the institutional goal of advancing knowledge, one might be tempted to say that this is favorable for the organization since this is why the research organization has been created. *But is the process favorable?* Scientists in any organization have other activities and duties, besides their own personal research, that must be accomplished as part of their organizational commitment. The typical investigator cannot be his own scientist all week long, as is indicated by the fact that the median number of hours put into "own professional work" in a typical week is 29.8.

The question, therefore, arises as to whether investigators with high motivation sacrifice their other organizational commitments for their personal research because of strong desires to advance knowledge.[22] If they do, and since this factor is a link in the performance process, then perhaps the above findings have unfavorable consequences for the organization. This process may require too much time for personal research, which may be disruptive for the organization as regards the scientists' fulfilling their other organizational commitments.

FIG. 1.—The performance-reward process in science

Table 3 provides one answer to this question. The extra time that the highly motivated scientists put into their own research is carried forward, as their weekly time schedule accumulates, with no sacrifice to other professional and organizational activities or commitments. The longer hours put into their own research (15 per cent difference) are maintained by highly motivated investigators as time is consecutively added on (1) for other professional productive work (14 per cent difference), such as performing services for others and work-

high motivation which results in high performance. However, this is not so. In comparing proportions downward in Table 2 among those with high motivation, 12 per cent more who work 21 or more hours a week on their own research have a high performance score. Among those with low motivation, 8 per cent more who work 21 hours or more a week on their own research have a high performance score. The original relation between time in own research and performance is 11 per cent. Therefore, high motivation, instead of being an intervening variable between time and performance, is a condition that creates a slightly stronger relation between the two. This is, of course, the time sequence I have originally assumed, which shows it is the sequence that prevails in the population under study.

[21] I use the "21 or more hours a week" break in the distribution, since it is at this point that the consistent direction of association between time and motivation changes. The distribution ranges from 7 per cent who work less than 15 hours a week on their own research to 7 per cent who work over 46 hours a week.

[22] That this is an important consideration for the organization is indicated by one of the six criteria used in evaluating the scientists for potential promotions: "Writing or editorial ability, effectiveness on boards and committees, ability to organize his and others' work, administrative judgment and other traits relevant to his performance on his current job and the job for which he is being considered" (Kidd, *op. cit.*). This criterion indicates that the scientist's worth to the organization is based also on the non-scientific work he has been asked to do.

ing with close colleagues; (2) for non-productive professional work (21 per cent difference), such as attendance at meetings and seminars, reading and dealing with people other than close research associates; and (3) for a total work week (17 per cent difference), which includes all other organizational activities beyond their professional ones.

In fact, in response to the question,

TABLE 3

MOTIVATION AND WORK ACTIVITIES

Consecutive Addition of Hours per Week Spent on Various Work Activities	Motivation (Per Cent)		
	High*	Low†	Difference
1. Own research: twenty-one or more hours	76	61	+15
2. Plus other professional productive work: thirty-six or more hours	63	49	+14
3. Plus non-productive professional work: forty-one or more hours	69	48	+21
4. Plus other organizational activities for total work week: fifty-one or more hours	65	48	+17

* N = 186.
† N = 146.

"How much time per week are you now spending on activities which could be shifted to other people or eliminated without impairing your present scientific or other professional work?" more highly motivated investigators suggested that less time be shifted to other people. Thus, in line with not sacrificing organizational work for their own research, the highly motivated investigators are less ready than those with low motivation to shift any additional work load of organizational life upon other men. Indeed, it would have been understandable if they had been more ready to shift activities not directly pertinent to their professional pursuits to other personnel, since they are motivated to advance knowledge, and any activity that intruded into this effort might appear burdensome. *It would seem, then, that high institutional motivation tends to make these scientists both hard-working investigators and hard-working organizational men.*

THE DISTINCTION BETWEEN COSMOPOLITAN AND LOCAL

This finding suggests that those scientists who are highly motivated to advance knowledge will be assets to the organization in two ways: (1) achieving the organizational goal, which is the same as the institutional goal of science, and (2) meeting non-scientific organizational requirements that take time from research. Thus, the organization will tend both to persist and to maintain its prestige (through accumulated individual successes) within the community of scientific organizations. The latter aim is very important for attracting and recruiting more capable, highly motivated scientists. Persistence and maintenance of prestige through achievements of the institutionally designated goal need not always be related. There are numerous examples in the literature that show that attempts to meet requirements for persistence can subvert organizational goals.[23]

This finding—that both research and non-research activities seem important and compatible to highly motivated scientists—*indicates, by the criterion of direction of work efforts, that these scientists are both cosmopolitan and local oriented.* They are oriented to achievement of the institutional goal and honorary rewards, and hence toward professional colleagues everywhere and toward success as members of their profession. They are also oriented to their responsibilities within the organization that provides them with the facilities for advancing scientific knowledge and thus gaining recognition, and with a prestigeful base for that cluster of organizational rewards called a promising career.

Further data support the presence of this dual orientation among highly moti-

[23] The foremost example is Philip Selznick's *TVA and the Grass Roots* (Berkeley: University of California Press, 1953).

vated scientists. As hard-working cosmopolitans oriented to all professional colleagues they are more interested in contacts outside the organization as sources of information, in a move (if necessary) to a university environment (however, motivation does not account for more plans to move), and in belonging to an organization with prestige in the scientific world. Also, they feel a greater sense of belonging to and involvement with professionals within the organization. With respect to the professional or institutional goal, any suggestion of a change from basic research as the only organizational goald to its coexistence with applied research will be cause for concern.

As hard-working locals, the more highly motivated investigators desire an important job in the organization and association with persons who have high status and important responsibilities. In addition, more of them have a strong sense of belonging to the organization and are interested in higher level jobs that are most compatible with the institutional goal. That is, they tend to be interested in the supervision of subordinate scientists rather than in supervision of the organization.

In sum, this congruence of organizational and institutional goals generates a local-cosmopolitan scientist when the scientist is highly motivated to advance basic knowledge. Devotion to both profession and organization is, in this case, not incompatible, as it tends to be for scientists in industry.

LOCAL-COSMOPOLITAN THEORY

This dual orientation of highly motivated scientists is especially important since, with few exceptions, the research literature characterizes *scientists* as either cosmopolitan or local. They are presented as two distinct types of scientists whose orientations and activities are, if not directly opposed to each other, not related. Shepard, in discussion dilemmas in industrial research, has said, "The research staff itself is likely to be divided into what Robert Merton calls the 'cosmopolitans' and 'locals.' "[24] In his book on industrial scientists, Marcson reports that "it is possible to distinguish between two types of laboratory staff people—professionally oriented and organizationally oriented."[25] Peter reports of a seminar on problems of administering research organizations, "In the first two of the seminars, some time was spent discussing another bimodal distribution of scientists, those described as 'cosmopolitan' and those called 'locals.' "[26]

I suggest that cosmopolitan and local can also be seen as two dimensions of orientation of the same scientist, each activated at the appropriate time and place as determined by the organizational structure within which he works. The question now arises as to whether or not there is a conflict between my findings of a cosmopolitan-local orientation and the body of literature that treats the two orientations as distinct. Is one view more correct than the other? If we ask the question, "Under what conditions has each distinction emerged?" then we find that each of the views is accurate and applicable to the particular organizational situation under analysis.

The distinction between cosmopolitan and local scientists emerged during the study of research organizations in which the institutional goal of advancing knowledge is more or less in conflict with a major organizational goal of applying knowledge. For example, in reviewing industrial research organization studies, Shepard states that the scientist's "motto" is "How much do we know about this?" whereas the businessman's motto is "What is the value of this to the company?"[27] This conflict results in a "problem person" in the cosmopolitan and in a "good employee" in the local.

Scientists take sides in the conflict ac-

[24] "Nine Dilemmas . . . ," *op. cit.*

[25] *Ibid.*, p. 18. [26] *Ibid.*

[27] *Ibid.* A conflict in goals is also the criterion for separating local and cosmopolitan scientists used by Marcson, *op. cit.*, and Peters, *op. cit.*

cording to their goal priority; hence the social scientist studying the organization uses this criterion to divide scientists into two groups. The cosmopolitan group makes trouble for management in primarily pursuing the institutional goal and career, and the local group creates little problem in primarily pursuing the company goal and career. In sum, this distinction is a device for understanding organizational problems such as communication of results, turnover, multiple career lines, differential incentive systems, needs for loyalty versus expertise, and so forth.[28]

TABLE 4

SCIENTISTS' ORIENTATION

Institutional and Organizational Goals	Professional Motivation		
	High	Medium	Low
Same	Basic research Local-cosmopolitan		Local
Different	Cosmopolitan	Applied research Local-cosmopolitan	Local

Cosmopolitan and local as dual orientations of the same scientist emerged in our analysis of a research organization that emphasized the institutional goal. As there was little or no conflict between goals, there was no necessity to take a priority stand, or of being split into groups. Because of this congruence of goals, a local orientation helps to maintain the opportunity to pursue research and to have a career at a highly prestiged locale, both thoroughly consistent with the cosmopolitan orientation.[29] In using the notion of dual orientation, we end up talking of organizational benefits, not problems.

Further, I have found this dual orientation among highly motivated scientists, whereas Shepard, as well as the other authors cited, talks of all scientists. Thus, the two conditions that generate the emergence of either groups of cosmopolitan and local scientists, or scientists with a cosmopolitan-local orientation, are (1) compatibility of the organizational with the institutional goal, and (2) highly motivated scientists versus all scientists.

One of the exceptions to viewing local and cosmopolitan scientists as different groups in the literature on scientists is the "mixed type" offered by Kornhauser.[30] The "mixed type" is oriented to both company and profession and is interested in "facilitating the utilization of technical results." This applied orientation existed under the conditions of a conflict between the institutional goal and the company goal and is an accommodation seemingly in favor of the company. Thus, to date we have two general types of local-cosmopolitan scientists arising under different sets of specific conditions: (1) the basic research local-cosmopolitan and (2) the applied research local-cosmopolitan.

Table 4 locates the various general ori-

[28] That the distinction between types of scientists has much potential use in the analysis of problems surrounding the research organization's need for both loyalty and expertise is forcefully brought out in Gouldner, "Cosmopolitans and Locals," *op. cit.*, pp. 465-67.

[29] Blau and Scott, *op. cit.*, pp. 70-71, in comparing county agency caseworkers and Bennis' data on professional nurses, note that opportunities for a professional career in an organization coupled with restricted opportunities in competing organizations generate local orientations among professionals. Whether they still remain cosmopolitan or not was not discussed. Their analysis is, therefore, consistent with mine on the local dimension.

[30] Kornhauser, *op. cit.*, p. 122. Another exception is Avery's (*op. cit.*), "The career question confronting the technical man is not, typically, whether to commit himself wholly to localism or cosmopolitanism. Rather he is likely to be constrained to try to extract advantages from both sources." Gouldner (*op. cit.*), and Blau and Scott (*op. cit.*), also have mixed types in their tables but do not discuss them in text. They focus on the distinct groups. Caplow and McGee also note a mixed orientation among professors in high-prestige university departments (*op. cit.*), p. 85 (see also Warren G. Bennis, *op. cit.*, pp. 481-500).

entations of scientists to organization and/or profession likely to be generated by the two cited conditions: (1) congruence of institutional and organizational goals and (2) degree of institutional (or professional) motivation.

Last, the concern among the scientists in this study over the potential organizational emphasis upon the applied research goal suggests a few hypotheses about possible changes. If the organization starts to emphasize applied research, those highly motivated to do basic research may give up the basic research cosmopolitan-local orientation and become a definite group of cosmopolitans. The professional motivation of some may drop a little and then they are likely to become applied research local-cosmopolitans. The potential conflict between institutional and organizational goals may generate these changes, which then could result not only in the loss of benefits to the organization cited in this paper but also in the accumulation of problems cited by those writers who have developed the distinction between cosmopolitan and local as two types of scientists.[31]

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[31] For an analysis of the generation of cosmopolitan and local factions because of a change in goals see Paula Brown and Clovis Shepard, "Factionalism and Organizational Change in a Research Laboratory," *Social Problems*, April, 1956, pp. 235–43.

OCCUPATIONAL COMPOSITION AND THE METROPOLITAN HIERARCHY: THE INTER- AND INTRA-METROPOLITAN DIVISION OF LABOR[1]

OMER R. GALLE

ABSTRACT

The occupational composition of communities varies widely, and no community has a "typical" occupational structure. Each local community is an integrated part of the national system of urban places and related regions. A major determinant of the internal social structure of cities is their specialized relationships with the total system of cities. Variations in the occupational composition of large metropolitan areas can be attributed primarily to the occupational structure of their "profile industries," those "city-building" industries in which the city specializes. Occupational composition of "city-serving" non-profile industries is remarkably similar in all large metropolitan areas. Studies of community structure should specify the impact of intercommunity relationships on intracommunity patterns.

INTRODUCTION

That small communities vary in occupational composition is evident in the contrasts between a one-company mining town and a college town. Large communities also display marked variations in occupational structure. For example, in 1950 the Washington, D.C., Standard Metropolitan Area (SMA) employed 27.5 per cent of its labor force as clerical and kindred workers, whereas 11.8 per cent of the Charleston, West Virginia, SMA labor force were similarly employed.[2] Wilkes-Barre–Hazleton had 43 per cent of its labor force employed as operatives and kindred workers; in the Miami SMA 11 per cent were so employed.

The occupational structure of a community is perhaps the most important single aspect of its social structure, and has implications for a wide range of community processes.[3] Some speculations about these implications will be presented later. The major task of this paper, however, will be to demonstrate the sizable variations in occupational structure that exist among the large metropolitan areas of the United States and to examine the relationship between the occupational structure of a community and its position in a national system of cities.

Occupational composition has been found to vary with city size,[4] and with functional specialization.[5] Size, however, is but one index of a city's interrelations with other cities, as is functional specialization. A more complex classification will be useful, particularly in assessing differences between metropolitan areas, all of which are at one end of the size scale.

The concept of a system of cities.—In preindustrial times, the city was considered the core of an independent community, and together with its immediate hinterland constituted an integrated and self-sufficient

[1] Paper No. 14 in the series, "Comparative Urban Research," issuing from the Population Research and Training Center, University of Chicago, under a grant from the Ford Foundation. A preliminary version of this paper was read at the 1962 annual meetings of the American Sociological Association. The author would like to thank Otis Dudley Duncan, Karl E. Taeuber, Alma F. Taeuber, and Robert W. Hodge for helpful comments and criticisms. Most of the computational work was ably performed by Mark Warden.

[2] The following data are gathered from U.S. Bureau of the Census, *Census of Population, 1950* (Washington, D.C.: Government Printing Office, 1953), II, Part I, Table 90, 150–51.

[3] See, e.g., Otis Dudley Duncan and Beverly Duncan, "Residential Distribution and Occupational Stratification," *American Journal of Sociology*, LX (March 1955), 493–503; Frank W. Notestein, "Class Differences in Fertility," *Annals of the American Academy of Political and Social Science*, November, 1936, pp. 1–11; and Liston Pope, "Religion and the Class Structure," *Annals of the American Academy of Political and Social Science*, March, 1948, pp. 84–91.

[4] Otis Dudley Duncan and Albert J. Reiss, Jr., *Social Characteristics of Urban and Rural Communities, 1950* (New York: John Wiley & Sons, 1956), pp. 95–102.

[5] *Ibid.*, Part IV, esp. Tables 124, 137, 138, and 139.

entity.[6] The rise of modern technology, with its rapid means of transportation and communication, has changed this situation. An industrial nation such as the United States can be viewed as an integrated system of urban centers and interrelated regions, with each segment of the total system contributing its unique component to the total division of labor.

In this paper, attention will be focused on a set of fifty-six large metropolitan areas, comprising all SMA's of 300,000 or more population in 1950. These SMA's contained 43.6 per cent of the total population of United States in that year. Any classification of metropolitan areas according to their positions in a national system of cities must utilize a combination of criteria, including size and function. Such a classification has been developed by Duncan and his collaborators, taking account of population size; per capita measures of value added by manufacture, wholesale sales, business service receipts, non-local loans, and demand deposits; and a detailed study of the industrial structure and trade areas of each SMA.[7] The classification scheme is shown in Table 1.

The "metropolitan hierarchy" depicted in Table 1 is multidimensional and identifies eight classes of metropolitan areas. Among the manufacturing SMA's (the left side of the table), the "Diversified Manufacturing with Metropolitan Functions" is the only one of the three groups to show strong relationships with moderately large contiguous hinterlands. Each, however, encounters strong competition for these surrounding areas from other centers close by. The "Diversified Manufacturing with Few Metropolitan Functions" are SMA's of somewhat smaller size and have less developed trading and financial activities than manufacturing centers with metropolitan functions. The "Specialized Manufacturing" SMA's have the lowest average per capita wholesale sales and the highest average per capita value added by manufacture of the eight groups of SMA's. Because of their special functions, all three groups of manufacturing SMA's have relationships with many parts of the country, so that much of what would be considered their "hinterlands" are of a diffuse and non-contiguous nature.

Each of the "Regional Metropolises" has a fairly large contiguous hinterland over which it exerts relatively uncontested dominance. These SMA's have the highest average per capita wholesale sales for any of the groups of SMA's in the table and show some strength in manufacturing, although not as much as the manufacturing types of SMA's. The "Regional Capitals" are in many ways similar to the regional metropolises, and many of the differences between these two types of SMA's can be attributed to size. The regional capitals exert their dominance over smaller contiguous hinterlands; they have a slightly lower average per capita wholesale sales, and an average per capita value added by manufacture lower than all groups of SMA's, except the special cases. The "Dually Classified Regional Capitals," while not separated into a distinct group in *Metropolis and Region*, are so treated here. These SMA's combine some of the characteristics of both major types of SMA's (the regional

[6] Amos H. Hawley, *Human Ecology* (New York: Ronald Press Co., 1950), pp. 239–45. For a more detailed exposition of the ideas outlined in this discussion of the "system of cities" concept, see also Chauncy D. Harris, "The Market as a Factor in the Localization of Industry in the United States," *Annals of the Association of American Geographers*, XLIV, No. 4 (December, 1954), 315–48; Brian J. L. Berry and W. L. Garrison, "Recent Developments in Central Place Theory," *Papers and Proceedings of the Regional Science Association, 1958;* Edgar M. Hoover, "The Concept of a System of Cities," *Economic Development and Cultural Change*, Vol. III (1955); Donnell M. Pappenfort, "The Ecological Field and the Metropolitan Community: Manufacturing and Management," *American Journal of Sociology*, LXIV, No. 4 (January 1959), 380–85.

[7] For a detailed description of classification procedures see Otis Dudley Duncan, William Richard Scott, Stanley Lieberson, Beverly Davis Duncan, and Hal H. Winsborough, *Metropolis and Region* (Baltimore: Johns Hopkins Press, 1960), chap. xi, and esp. pp. 259–75.

centers and the manufacturing centers), with moderate evidence of both manufacturing activity and regional relationships.

Whereas the six types of SMA's described above are relatively homogeneous groups, the remaining two are not. The "National Metropolises," the five largest SMA's are "not products of regional forces, but are created by larger factors in the total national economy."[8] New York and Chicago clearly stand apart from the other three in such measures as per capita wholesale sales, business service receipts, nonlocal loans, and demand deposits. The other three SMA's are transitional between national metropolises and other types of SMA's (as noted by their secondary classification symbols in parentheses in the table). The "Special Cases," listed at the bottom of the table, are also a heterogeneous group. They form a residual category

[8] *Ibid.*, p. 266.

TABLE 1

THE METROPOLITAN HIERARCHY*

National Metropolis (N)
- New York
- Chicago
- Los Angeles (D)
- Philadelphia (D)
- Detroit (M)

Diversified Manufacturing with Metropolitan Functions (D)
- Boston (N)
- Pittsburgh (N)
- Saint Louis
- Cleveland
- Buffalo
- Cincinnati

Diversified Manufacturing with Few Metropolitan Functions (D-)
- Baltimore
- Milwaukee
- Albany–Schenectady–Troy
- Toledo
- Hartford
- Syracuse

Specialized Manufacturing (M)
- Providence
- Youngstown
- Rochester
- Dayton
- Allentown–Bethlehem–Easton
- Akron
- Springfield–Holyoke
- Wheeling–Steubenville
- Charleston, West Virginia

Regional Metropolis (R)
- San Francisco (N)
- Minneapolis–St. Paul
- Kansas City
- Seattle
- Portland
- Atlanta
- Dallas
- Denver

Dually Classified Regional Capitals (CD & CM)
- Louisville (CD)
- Birmingham (CM)
- Indianapolis (CD)
- Columbus (CD)
- Richmond (CD)

Regional Capital (C)
- Houston
- New Orleans
- Memphis
- Omaha
- Fort Worth
- Nashville
- Oklahoma City
- Jacksonville

Special Cases (S)
- Washington, D.C.
- San Diego
- San Antonio
- Miami
- Tampa–St. Petersburg
- Norfolk–Portsmouth
- Wilkes-Barre–Hazleton
- Knoxville
- Phoenix

* Adapted from Otis Dudley Duncan, William Richard Scott, Stanley Lieberson, Beverly Davis Duncan, and Hal H. Winsbo[illegible] *Metropolis and Region* (Baltimore: Johns Hopkins Press, 1960), p. 271.

containing SMA's that vary widely in function, from the nation's capital (Washington, D.C.), to tourist centers (e.g., Miami), to concentration in coal mining (Wilkes-Barre–Hazleton), to military centers (e.g., San Diego). In the following analysis, the national metropolises and the special cases, because of their internal heterogeneity, will not be studied; attention will be focused on the six groups of relatively homogeneous SMA's.

TABLE 2

MEAN PER CENT DISTRIBUTION OF EMPLOYED CIVILIAN LABOR FORCE, EIGHT BROAD OCCUPATIONAL CATEGORIES, BY TYPE OF SMA*

Occupational Category	Type of SMA					
	Regional Metropolis	Regional Capital	Dually Classified Regional Capital	Diversified Manufacturing, with Metropolitan Functions	Diversified Manufacturing with Few Metropolitan Functions	Specialized Manufacturing
Total	100.00	100.00	100.00	100.00	100.00	100.00
Professional, technical, and kindred workers	10.70	9.25	9.46	9.88	10.41	8.74
Managers, officials, and proprietors, including farm	12.45	11.76	9.99	9.63	10.11	9.32
Clerical and kindred workers	17.79	15.88	15.89	15.81	16.64	12.83
Sales workers	9.08	8.45	8.44	7.90	7.81	7.08
Craftsmen, foremen, and kindred workers	14.70	14.98	15.09	16.44	16.42	16.90
Operatives and kindred workers	15.44	16.41	20.40	21.47	21.53	28.33
Service workers, including private household	11.85	13.87	12.45	10.51	10.02	8.71
Laborers, including farm and mine	6.80	8.15	7.08	7.26	5.90	7.01
Occupation not reported	1.19	1.25	1.20	1.10	1.16	1.08

* Compiled from Bureau of the Census, *Census of Population, 1950* (Washington: Government Printing Office, 1953), II, Part 1, Table 90, 150–51.

OCCUPATIONAL DIFFERENTIATION

In the following analysis we will investigate more systematically the variations in occupational composition of large metropolitan communities. Table 2 shows the mean percentage employed in each of the eight broad Census occupational categories for each of the six homogeneous groups of SMA's. The manufacturing SMA's tend to have a greater proportion of their labor force classified as craftsmen, foremen, and kindred workers, and as operatives and kindred workers, while the regional centers have relatively more workers in the categories of managers, clerical, sales, and service workers. The dually classified regional capitals tend to be in between the two extremes of the regional metropolises and specialized manufacturing centers.

It is clear from the results shown in Table 2 that the classification scheme presented in *Metropolis and Region* is helpful in tracing some of the regularities of occupational differentiation. This classification system states that, among the large metropolitan communities of the United States, there exist fairly distinct groupings of cities that differ in accordance with the dissimilar tasks they perform within the national economy. Table 2 indicates that not only do these different types of communities perform dissimilar tasks, but they also vary in their occupational composition. In order to draw the causal inference that the cities

TABLE 3

Mean Per Cent Distribution, by Type of SMA, of Total Employed Civilian Labor Force of Profile Industries and of Non-profile Industries

Occupational Category	Type of SMA					
	Regional Metropolis	Regional Capital	Dually Classified Regional Capital	Diversified Manufacturing, with Metropolitan Functions	Diversified Manufacturing, with Few Metropolitan Functions	Specialized Manufacturing
	A. The Total Employed Civilian Labor Force					
Total	100.00	100.00	100.00	100.00	100.00	100.00
Professional, technical, and kindred workers	10.70	9.25	9.46	9.88	10.41	8.74
Managers, officials, and proprietors, including farm*	12.45	11.76	9.99	9.63	10.11	9.32
Clerical and kindred workers	17.79	15.88	15.89	15.81	16.64	12.83
Sales workers*	9.08	8.45	8.44	7.90	7.81	7.08
All blue-collar workers, including occupations not reported*	49.98	54.66	56.22	56.78	55.03	62.03
	B. The Profile Industries					
Total	100.00	100.00	100.00	100.00	100.00	100.00
Professional, technical, and kindred workers	8.96	9.28	6.99	8.29	9.61	5.69
Managers, officials, and proprietors, including farm*	11.88	9.35	5.65	4.82	4.47	2.76
Clerical and kindred workers*	27.00	21.51	17.56	15.90	19.33	11.70
Sales workers*	10.21	6.52	4.27	2.38	1.86	1.04
All blue-collar workers, including occupations not reported*	41.95	53.34	65.53	68.61	64.73	78.81
	C. The Non-profile Industries					
Total	100.00	100.00	100.00	100.00	100.00	100.00
Professional, technical, and kindred workers	10.98	9.44	10.32	10.58	10.64	10.52
Managers, officials, and proprietors, including farm	12.72	12.56	11.61	12.11	12.65	13.29
Clerical and kindred workers	14.77	13.79	15.13	15.82	15.18	13.43
Sales workers*	8.85	9.08	9.97	10.73	10.50	10.73
All blue-collar workers, including occupations not ported	52.68	55.13	52.97	50.76	51.03	52.03

* The Kruskal-Wallis H-test indicates that this extreme clustering of rankings within groups would occur with a probability of less than 0.001 if all groups had the same mean rank, and the rankings were in fact random over all groups (see William H. Kruskal and W. Allen Wallis, "Use of Ranks in One-Criterion Variance Analysis," *Journal of the American Statistical Association*, XLVII, No. 260 [December, 1952], 583–621).

Source: Bureau of the Census, *Census of Population, 1950* (Washington, D.C.: Government Printing Office, 1953), Vol. II, Parts 1–49, Table 84.

have dissimilar occupational structures, *because* they perform different tasks in the national economy more information is needed.

The industrial profile.—The industrial profile of a community consists of all those industrial categories in which the given community employs a substantially greater proportion of its labor force than does the labor force of the United States as a whole.[9] It is the group of industries in which a community specializes and that tie that community to the national system of cities. It follows from this that the non-profile industries of the various communities will be quite similar, since these industries consist mainly of those activities that any city of this size needs to maintain itself. The concept of industrial profile is in many respects similar to the economists' idea of economic base, or Alexandersson's notion of "city-forming" industries.[10]

Occupation and industry are two quite different concepts, and the classification of the labor force by one does not necessarily imply a set distribution with respect to the other. However, insofar as the division of labor along functional lines seems to be logically pre-eminent in the determination of a community's role in the national economy, industrial structure is here viewed as a basic determinant of occupational structure. Thus, the unique differences between types of cities in their occupational composition should be a function of the different modes of industrial specialization. The occupational structure of the non-profile industries may be expected to be quite similar in all types of cities.

Using the cross-tabulation of occupation by industry for each SMA that is available from the 1950 Census,[11] the occupational distributions of profile and non-profile industries have been compiled.[12] For each of the six homogeneous groups of SMA's, Table 3 shows the mean occupational distribution for profile and non-profile industries and for the total experienced civilian labor force. For convenience, all the blue-collar occupations have been considered together. With this exception, the occupational distributions for the total labor force, shown in panel A of Table 3, are identical with the mean occupational distributions exhibited in Table 2. That much of the variation in occupational composition is accounted for by the industrial profiles of the respective cities can be seen from Table 4.

TABLE 4

ESTIMATED VARIANCES AND STANDARD DEVIATIONS OF OCCUPATIONAL PER CENT DISTRIBUTIONS OF TOTAL EXPERIENCED CIVILIAN LABOR FORCE, PROFILE INDUSTRIES, AND NON-PROFILE INDUSTRIES OVER ALL 42 SMA'S

Occupational Category	Variances		
	Total Labor Force	Profile Industries	Non-profile Industries
Professional, technical, and kindred workers	1.93	20.18	1.76
Managers, officials, and proprietors, including farm	2.24	13.83	1.40
Clerical and kindred workers	5.47	47.45	1.82
Sales workers	0.72	14.63	1.17
Blue collar workers, including farm and mine	24.54	240.32	7.39

[9] For a detailed discussion of the construction and use of industrial profiles, see *ibid.*, pp. 200–230.

[10] Gunnar Alexandersson, "City-forming and City-serving Production," in Harold M. Mayer and Clyde F. Kohn (eds.), *Readings in Urban Geography* (Chicago: University of Chicago Press, 1959), p. 111.

[11] Bureau of the Census, *Census of Population, 1950,* Vol. II, Parts 2–49 (the state volumes), Table 84.

[12] Modifications were made in the industrial profiles of the SMA's to facilitate their use with the intermediate industrial classification used in the occupation by industry table for SMA's of the Census volumes. The original profiles were constructed from the detailed classification. For a more detailed explanation of the procedure followed in the modification of the industrial profiles, see the complete report (Omer R. Galle, "Community Structure and the Metropolitan Hierarchy" [unpublished research report, Population Research and Training Center, University of Chicago, 1962]).

In this table, the variances for each of the five occupational groups over the forty-two SMA's for workers in profile industries, in non-profile industries, and in the total labor force are shown. All five of the variances for the profile industries are significantly larger than their non-profile counterparts.[13] The question now arises as to whether the variations in occupational composition are systematic with respect to the groups of cities delimited by the authors of *Metropolis and Region*. The results of an analysis of variance are indicated in Table 3.

With only minor exceptions, the results in Table 3 are consistent and quite striking. Within the profile industries, the differences between types of SMA's in the mean percentages in each occupational category are much larger than the differences between the occupational distributions for the total labor force. The regional centers, led by the regional metropolises, have relatively more workers in white-collar occupations, while the manufacturing SMA's have much greater proportions of their labor forces (in the profile industries) in blue-collar occupations. The differences between the means in the profile industries are significant well past the 0.001 level of probability for all occupational categories except the professional, technical, and kindred workers. The mean proportion of blue-collar workers in specialized manufacturing centers (78.81 per cent) is almost twice as large as the corresponding figure for the regional metropolises (41.95 per cent). Similarly, the regional metropolises have a mean of 11.88 per cent of their profile-industry labor force in the managers, officials, and proprietors category, as compared with only 2.76 per cent for the specialized manufacturing centers.

Among non-profile industries the systematic differences evident for the total labor force and for the labor force in profile industries are, with the exception of the sales workers category, not present. Even the blue-collar category, which is probably the most heterogeneous occupational group, does not show a significant H-value in the analysis of variance.

With regard to the profile industries, the regional metropolises have the highest mean percentage employed in the sales workers category, and the specialized manufacturing centers the lowest. In the non-profile industries, on the other hand, the regional metropolises have the lowest mean percentage employed as sales workers, and the specialized manufacturing centers the highest (along with the diversified manufacturing centers with metropolitan functions). That is, there is almost a complete reversal in the direction of the differences between the means in the non-profile sales workers. It is possible that in the central-place metropolises, such as the regional centers, some of the activities that are generally thought to be non-profile (maintenance activities) are pre-empted by the profile industries that are already specialized in this area, and serving more than the local community, while in the non-central-place oriented cities (the manufacturing SMA's) this is not the case.[14]

Of the five occupational categories shown in Table 3, only the category of professional, technical, and kindred workers group fails to show any significant systematic differences between the six types of SMA's, either for the profile industries, the non-profile industries, or the total labor force. It should be noted here that all these occupational groupings are quite broad and

[13] K. A. Brownlee, *Statistical Theory and Methodology in Science and Engineering* (New York: John Wiley & Sons, 1960), p. 220.

[14] The significant differences in the sales category in non-profile industries may also be due to an imprecise method of "operationalizing" the industrial profile concept for use in studying occupational differentiation. In modifying the industrial profiles to the occupation by industry table of the Census volumes, only those industries which were listed separately in the "industrial profile" tables in *Metropolis and Region* were included in the "operational" industrial profile. This meant that some non-local services, such as wholesale trade and others, were almost always included in the regional centers, and almost always excluded in the manufacturing centers, though in fact they should have been included in all SMA's.

consequently quite heterogeneous. This is especially true for the professionals; within this broad category there are physicians, judges, dentists, and other "service" professionals whose work is generally ubiquitous in all communities of sufficient size. There are also engineers, chemists, natural scientists, and other technical workers whose work is not primarily dependent upon the general public, but upon the industrial structure of the city. We thus have two trends working at cross-currents with each other. Those communities with a high level of industrial activity would have a large proportion of the technical component of the professional category, while those cities that serve as distributive and integrative central places for a large hinterland would tend to have more of the service type of professional.[15]

DISCUSSION

The basic purpose of this study has been to specify the relevance of a community's place in the general system of cities for its occupational structure. In light of this relationship, the importance of the industrial-profile concept should be emphasized. In fact, the industrial profile comprises a different set of industries for each city. Since occupational structure is determined to a great extent by industrial structure, much of the observed variation in the occupational composition of profile industries for the different types of SMA's may be accounted for by the varying composition of the industrial profiles. It is also possible that some of the variation is due to different occupational compositions of the same industries in two or more different types of communities. Although this would be the next step in further work, no attempt has been made here to differentiate between these two effects. The avowed purpose of the study, however, has already been accomplished. If the reasoning with respect to the industrial profile is accepted, then we have shown that the differing occupational structures of large metropolitan communities are accounted for in large part by the specialized tasks these communities perform in the national economy.

It must be emphasized that this is not a circular statement. Industry and occupation are not the same. When classified by industry, the labor force is grouped into categories of productive functions for the community. When classified by occupation, the labor force is sorted into groups that denote the specific tasks performed by individuals, regardless of what they are producing as a final product. The original classification of SMA's was delimited by a careful study of the productive and distributive functions of each area. That the occupational distributions of the different types of communities exhibit systematic variation because of this is an empirical finding and supports the basic hypothesis of this paper.

Implications.—According to Rossi, "We know a great deal from the geographers and ecologists about the location of human settlements, how to classify cities by their important functions, but we do not have much knowledge about how these factors affect the social lives of the communities."[16] In this paper, a systematic variation in occupational composition has been shown, and the reasons for these variations have been identified. Although the implications for the "social lives" of the communities involved are more difficult to specify, some speculations concerning the consequences of the observed differences in occupational composition may be advanced.

Duncan and Schnore have suggested that the perspective of the human ecologist has a primary contribution to make to the study of power in the local community.[17]

[15] In the original analysis, this was in fact found to be the case (see Galle, *op. cit.*).

[16] Peter H. Rossi, "Theory, Research, and Practice in Community Organization," *Social Science and Community Action*, ed. Charles R. Adrian (East Lansing: Board of Trustees, Michigan State University, 1960), p. 22.

[17] Otis Dudley Duncan and Leo F. Schnore, "Cultural, Behavioral and Ecological Perspectives in the

Hawley has suggested that "dominance" in the local community is attached to those functional units that control the flow of sustenance into it.[18] In a "company town," for example, where one firm employs the majority of the labor force, that company occupies a unique power position in the community. Whereas dominance is more diffuse and the situation therefore more complicated in the large metropolis, the profile industries play comparable dominant roles in these areas. The dominance may be less centralized, but the functional niche is similar. Although the industries that are in this dominant position may or may not exercise their "functional" power, they have the potential to do so. Other groups may organize for the prime purpose of combating this corporate power and gaining some control over community decisions.[19] Rossi has suggested that in large cities voluntary organizations, such as civic groups and the Community Chest, may play an important role in the power complex of the community.[20] The dominance of the profile industries may also be manifested in relationships with the political machinery of the community.

In his paper on "Community Power and Urban Renewal Success," Hawley examined the effect of the size of the managerial component of the labor force on the success of a city in urban renewal programs.[21] His data show an inverse relationship between the relative numbers of managers, officials, and proprietors in a city's labor force and the amount of progress achieved on the city's urban renewal program. He concludes that a smaller managerial component indicates a centralization of the control of "power" in the community, which in turn facilitates the control over decisions such as whether to undertake or continue programs of urban renewal.

Pinard has examined a number of demographic variables and their relation to the decision of a community to accept or reject fluoridation of their water supply.[22] He found that the communities that had a low proportion of managerial and professional workers had greater success in passing fluoridation measures. He also confirms Hawley's suggestion that power can be used to block collective action by pointing out that the intensity of approval *or* disapproval is greater in the communities with a smaller "social elite."

Neither Hawley nor Pinard examined the causes of variation in the relative size of the upper white-collar occupations between cities. Similarly, we have not examined the SMA's studied here with respect to success in urban renewal planning or fluoridation projects. Combining our results with the results of their two papers, however, it can be predicted that the power structure varies systematically with the functional position of the community in the system of cities.

It could be argued that since the major portion of the variation in percentage of managers is accounted for by the profile industries, and since the profile industries do not employ the major proportion of the labor force in any of the SMA's, the power accruing to these industries will manifest itself only in their internal structure. This is significant in and of itself, but the unique position that the profile industries hold suggests community-wide repercussions in the power structure. The industrial profile of a city consists of those industries that relate the local community to the national system. Hawley

Study of Social Organization," *American Journal of Sociology*, LXV, No. 2 (September, 1959), 139.

[18] Hawley, *op. cit.*, pp. 229–30.

[19] *Ibid.*, pp. 210–11.

[20] Peter H. Rossi, "Theory and Method in the Study of Power in the Local Community" (a paper presented at the 1960 annual meetings of the American Sociological Association), pp. 38–39. It is, of course, possible that the executives of the profile industries utilize these voluntary organizations as one channel through which their power is exercised.

[21] Amos H. Hawley, "Community Power and Urban Renewal Success," *American Journal of Sociology*, LXVIII, No. 4 (January, 1963), 422–31.

[22] Maurice Pinard, "Structural Attachments and Political Support in Urban Politics: The Case of Fluoridation Referendums," *American Journal of Sociology*, LXVIII, No. 5 (March, 1963), 513–26.

has suggested that these industries are important in the community-wide power structure. "The relative numbers in the key industry should prove decisive. . . . What constitutes a key industry, of course, is contingent upon the function the city performs for the regional and national society."[23] In any case, the papers of Hawley and Pinard, and the results presented here, along with the preceding speculative remarks suggest that the ecological study of community power is well worth more detailed investigation, both with respect to the internal dynamics of power within the city, and with respect to generalizations about differences in power structures between cities.

As these brief remarks indicate, we feel that a more careful study of the interrelations between the position of a city in the national system of cities, its occupational structure, and other internal characteristics of the city may be a useful means of getting at aspects of power structure, and other community social processes, especially in the large metropolitan areas where the "community study method"[24] has yet to be proven economically feasible. In the course of this type of study, it may be well to re-examine the utility of a typological classification of cities, such as the one from *Metropolis and Region*. Cities actually vary in a more or less continuous manner, and categorizing them into distinct groups is a rather crude technique. It may be more appropriate to try to construct a multiple-variable approach to community structure, utilizing many if not all of the variables used in the *Metropolis and Region* scheme, and perhaps other variables, in an attempt to study a more specific aspect of community structure or community social life. The use of a multiple-variable approach would also allow analysis of a wider range of cities and a more detailed examination of the relevant aspects of a city's position in the national system of cities.

CONCLUSION

It has been suggested that it is fruitful to view the local community as an integral part of the total system of urban places and related regions in the national economy. Local cities perform different tasks in the maintenance and growth of the system as a whole that cause them to become differentiated with respect to each other in their internal aspects as well.

In applying our hypothesis to actual data, we have considered only the largest metropolitan areas of the United States as of 1950, and have examined the internal division of labor for these areas as reflected in their occupational composition. We have demonstrated that the occupational structure of a city exhibits systematic variation with respect to the community's place in the system of cities. These differences can, in large part, be attributed to the industrial profile of the city—that part of the city's economic activity that relates the local community to the system of cities. This, we feel, is the major contribution of this paper.

The position a city holds in the national system of cities is a factor determining its social structure. We would suggest that community researchers who try to argue that their particular community is "typical" of all American communities are doomed to failure.[25] It might be more meaningful to try instead to specify where their community stands with respect to the national system of cities, and to consider whether this fact makes any difference in their powers of generalization. It is our firm belief that in many cases it will.

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[23] Hawley, "Community Power and Urban Renewal Success," *op. cit.*, p. 431.

[24] Conrad M. Arensberg, "The Community-Study Method," *American Journal of Sociology*, LX, No. 2 (September, 1954), 109–24.

[25] Almost all community studies make some attempt to generalize to some larger universe of communities. For one of the more lyrical examples see W. Lloyd Warner, *Democracy in Jonesville* (New York: Harper & Bros., 1949), p. xv. For a more recent example see Robert A. Dahl, *Who Governs?* (New Haven, Conn.: Yale University Press, 1961), pp. 1–8.

ALIENATION AND SOCIAL LEARNING IN A REFORMATORY[1]

MELVIN SEEMAN

ABSTRACT

In a reformatory setting the learning of information relevant to correctional matters is shown to be dependent upon the inmates' degree of alienation (i.e., powerlessness). Three kinds of information, differing chiefly in their usefulness for managing one's own destiny, were presented to the prisoners. This information concerned (1) the immediate reformatory situation, (2) achieving successful parole, and (3) long-range prospects for a non-criminal career. The essential prediction was that inmates scoring low in powerlessness would show superior retention of the parole material, since this material most clearly implies the possibility of personal control over events. The findings confirm this prediction. Examination of the inmates' social backgrounds shows that these results cannot be attributed to differences in intelligence or criminal history. Furthermore, the superior learning of the unalienated prisoners is shown to be associated with achievement-oriented behavior within the prison and on the outside. The relevance of these associations between alienation and social learning is indicated both for learning theory proper and for sociological concerns regarding alienation in contemporary society.

Social psychologists have a fundamental interest in the problem of social learning. For the psychologist, learning theories have traditionally been the intellectual core of his craft; and for the sociologist, an understanding of the socialization process—not only in the family but also in the shop and the professional school—has been a prime order of business. This paper seeks to bridge these interests by examining learning data that have relevance for both learning theory proper and for our interpretations of the socialization process.

The conceptual vehicle employed in effecting this rapprochement between psychological and sociological interests in social learning was the idea of "alienation." The term is used here to refer to powerlessness—strictly speaking, to the individual's low expectancy that his own behavior can determine the occurrence of the goals or rewards he seeks.[2] The basic proposition under test is that the individual's expectation for control over events is a crucial factor in the learning process. Such a demonstration would be critical in two senses: first, in its import for learning theory; and second, in its import for sociological theories about alienation in modern society.

It has been noted that many of the experimental paradigms that are applied to human learning are based primarily on the

[1] This research was supported by Contract No. AF 49 (638)-741 monitored by the Air Force Office of Scientific Research of the Office of Aerospace Research, and by a grant for data analysis from the University of California at Los Angeles. The bulk of the work under the Air Force contract has consisted of laboratory studies of learning under varying conditions of "internal versus external control" (i.e., varying expectancies for the control of reinforcements). For an early statement of the relation of that work to the problem of alienation, see J. B. Rotter, M. Seeman, and S. Liverant, "Internal Versus External Control of Reinforcements: A Major Variable in Behavior Theory," in N. F. Washburne (ed.), *Decisions, Values, and Groups*, II (London: Pergamon Press, 1962), 473–516. The work on internal versus external control has been conducted largely by J. B. Rotter, S. Liverant, and D. P. Crowne of the Ohio State University, to whom I am deeply indebted for assistance in the conception and execution of the present study. Generous help was provided by Mr. Minton, the warden of the Chillicothe Reformatory, and by Dr. Kramish, its chief psychologist. I wish to acknowledge the aid of John Tinker and Gerald King as research assistants. Helpful readings of an earlier draft of this paper were provided by Richard Jessor, Harold H. Kelley, Richard T. Morris, and Julian B. Rotter.

[2] This version of alienation as powerlessness is presented more fully in an earlier paper, which sketched the historical sources of five alternative meanings of alienation and then sought to provide a viable research formulation of each of these five variants (M. Seeman, "On the Meaning of Alienation," *American Sociological Review*, XXIV [December, 1959], 783–91).

chance, luck, or experimenter-controlled situation. In these traditional studies animal subjects are employed, or human subjects are engaged in relatively simple conditioning; and in neither case can it be said that individual skill or complex symbolic assessments concerning the locus of control in the situation are much involved. Rotter and his colleagues have shown that the learning pattern is quite different under conditions where the subject conceptualizes the problem as one involving his personal control or skill rather than one that is governed by chance factors or experimenter control. These studies have brought sharply into focus some of the general doubts that have been expressed concerning the applicability of classical learning theory to complex human learning.[3]

Thus, the idea of powerlessness extends downward, as it were, in its potential for reorganizing the relatively "microscopic" studies of laboratory learning—microscopic in the sense that both the behaviors demanded and the social structural context for their performance are highly simplified. But the idea of powerlessness also extends upward in its significance, being an integral element in sociological descriptions of a variety of "macroscopic" concerns: the occurrence of mass movements; the conditions of political democracy; and the like. In a recent study, for example, it was demonstrated that the "poor learning," typical for laboratory situations that are defined as being externally controlled, occurs also in the formal institutional setting of a tuberculosis hospital. Among patients matched with respect to socioeconomic characteristics and hospital experience, patients who scored high on powerlessness had less objective knowledge about their condition. This differential knowledge about health matters was evidently revealed in their ward behavior, as indicated by the fact that multiple and independent staff describers of the patients were in agreement concerning the low information possessed by the more alienated patients.[4]

This demonstration concerning health information, however, raises several important questions. First, since the design did not involve the introduction of new knowledge to be learned, it can be argued that the causal chain remains quite unclear: the study may tell less about poor "learning" produced by powerlessness and more about powerlessness that comes from inadequate knowledge. This problem is not academic when one is seeking to develop a systematic set of *learning* studies in both laboratory and field situations.

Furthermore, the hospital study made no distinction among kinds of knowledge; hence one is left wondering how general-

[3] To put it much too briefly, these studies show that (1) the individual learns less from his experience in the situation that is conceived to be chance-controlled, and (2) the extinction patterns in skill-controlled situations challenge the standard proposition that training under conditions of partial reinforcement (as against 100 per cent reinforcement) produces greater resistance to extinction. For the most recent demonstration of these points, and a discussion of their significance for learning theory, see J. B. Rotter, S. Liverant, and D. P. Crowne, "The Growth and Extinction of Expectancies in Chance-controlled and Skilled Tasks," *Journal of Psychology*, LII (July, 1961), 161–77.

The "general doubts" about learning theory have been common in sociological social psychology; but for a recent expression of similar doubts on the psychological side see S. Koch, "Some Trends of Study I: Epilogue," in S. Koch, *Psychology: A Study of a Science*, III (New York: McGraw-Hill Book Co., 1959), 729–88.

[4] These findings are detailed in M. Seeman and J. W. Evans, "Alienation and Learning in a Hospital Setting," *American Sociological Review*, XXVII (December, 1962), 772–82 (see also P. M. Gore and J. B. Rotter, "A Personality Correlate of Social Action," *Journal of Personality* [in press]).

It should be explicitly noted that alienation has been discussed both as a feature of the *situation* (i.e., situations that are defined as externally versus internally controlled), and as a characteristic of the *person* (i.e., persons who hold generalized low expectancies for personal control). The laboratory studies have been uniform in showing that differences in situations—chance versus skill tasks—produce differential learning patterns; but predictions about learning based on individual differences in powerlessness have been less reliable in the laboratory setting.

ized the learning tie with powerlessness may be and, indeed, whether the poor health knowledge shown by the alienated patients would be paralleled in almost any domain (suggesting, e.g., a highly generalized withdrawal of interest on their part, or fundamental differences in I.Q. or capacity to learn).

The present study attempts a demonstration relevant to these questions by examining the relation between alienation and social learning in a reformatory, where both the inmate's learning and his institutional behavior can be jointly considered. But why in a reformatory? The specific relevance of the idea of "internal versus external control" to the development of delinquent conduct has been nicely described by Cloward and Ohlin in a section of their recent work that is subtitled *The Process of Alienation:* "It is our view that the most significant step in the withdrawal of sentiments supporting the legitimacy of conventional norms is the attribution of the cause of failure to the social order rather than to oneself. . . . Whether the failure blames the social order or himself is of central importance to the understanding of deviant conduct."[5]

Obviously, it is not only in the original development of deviant behavior that orientations regarding powerlessness are critical. It is possible to conceive of the reformatory and its associated training apparatus as a vast learning mechanism; but one in which the essential features of powerlessness dominate institutional life—where, for example, paroles are denied and inmates are left in ignorance of the reasons, and where the inmate culture is a response to the more or less total threat to personal control.[6]

If the learning implications discussed above hold in this institutional case, one would expect that different degrees or kinds of learning would characterize those reformatory inmates who hold an alienated view of their circumstances, for they believe that what happens to them is not a function of their own action or skill, but of forces (whether of fate or society) that lie outside their own control. It is to the test of this proposition of differential learning as a consequence of differential alienation that this paper is addressed.

METHOD

The study was designed to achieve a controlled demonstration, while at the same time exploiting the natural setting of the reformatory and the daily correctional concerns of the inmates. Four kinds of data were gathered, the first two of which were central: (1) the alienation measure, (2) indexes of learning for several kinds of corrections-relevant information, (3) a measure of "social desirability" (used basically as a control for discriminating subjects whose high needs for social approval might be distorting other tests responses), and (4) background information, including data on criminal careers, I.Q., and achievement-test scores.

The alienation measure.—The test of the inmates' sense of powerlessness was composed of forty forced-choice items offering a contrast between internal and external control. Two illustrative items are:

I more strongly believe that:

1. —— *a*) Even if the odds are against you, it's possible to come out on top by keeping at it.
   —— *b*) A person's future is largely a matter of what fate has in store for him.

[5] R. A. Cloward and L. E. Ohlin, *Delinquency and Opportunity* (Glencoe, Ill.: Free Press, 1960), p. 111. See also R. K. Merton's discussion of the relation between anomie and the development of ideologies explaining failure in terms of fate, luck, or chance (*Social Theory and Social Structure* [rev. ed.; Glencoe, Ill.: Free Press, 1957], pp. 147 ff.).

[6] For a description of these alienative features of prison life see G. M. Sykes, *The Society of Captives: A Study of a Maximum Security Prison* (Princeton, N.J.: Princeton University Press, 1958). Sykes describes the social roles and argot terms of the prison largely as "alienative modes of response to the specific problems posed by imprisonment" (p. 106).

2. ——a) Nowadays people just don't realize what an important role luck plays in their lives.
——b) There is really no such thing as luck.

The test covers a wide range of behaviors (e.g., making friends, war and politics, occupational success, school achievement, etc.); hence it is a measure of the individual's generalized expectancies for control of events. The test used here is one of several variants that have been used in both laboratory and community studies.[7]

Social desirability.—The measurement of an individual's attitudes concerning personal control may activate American cultural values regarding mastery, generating pressures to respond as an "internal"; or the test may generate pressures to respond defensively as an "external," to cover one's failure by attributing it to fate or unmanageable external forces. Such pressures are merely illustrative, and they are, of course, not unique to the alienation scale; they are a part of any test-taking situation where the subject is engaged in presenting an image of himself. As a possible control for these pressures, the Marlowe-Crowne "social desirability" scale was included in the battery of tests. This scale is a thirty-three-item, true-false questionnaire that has been shown to be related to needs for approval and social conformity, and that requires (for the high scorer) that the subject depict himself in a culturally approved, but highly improbable, light. He must, for example, consistently say "true" (or "false" in the reversed items) to statements like the following: "I would never think of letting someone else be punished for my wrongdoing," or "No matter who I'm talking to, I'm always a good listener."[8]

The learning indexes.—One would like to have measures that reflect the inmates' actual learning about matters that count in the institution and on the outside, and especially, perhaps, an index that reveals the institution's impact (e.g., new skills in dealing with others, new knowledge about job requirements, etc.). But since these are notoriously elusive criterion measures, an alternative procedure was adopted. A set of twenty-four information items was constructed dealing with correctional matters. These items were then presented to the inmates, who were subsequently tested on the retention of this material. Since this is the unique and key feature of the design, a number of clarifications are in order. Most important is the fact that the items represent *three different kinds of knowledge*. These three varieties of knowledge, which differed essentially in their potential usefulness for managing one's own destiny, were:

1. Knowledge concerning *the immediate reformatory situation* at Chillicothe.[9] These items contained information that is merely descriptive and involved only a

[7] This test (called the internal versus external control, or I-E, scale) is largely the work of the late Professor Shephard Liverant and his colleagues at Ohio State. It is a refinement of an earlier test used by E. J. Phares, and revised by W. James, in doctoral work at Ohio State on skill and chance orientations. Shorter versions of the I-E scale have been used by Seeman and Evans (see n. 4 above), and by Arthur Neal in his work on alienation in the community (see "Stratification Concomitants of Powerlessness and Normlessness: A Study of Political and Economic Alienation" [unpublished Ph.D. dissertation, Ohio State University, 1959]).

[8] For further information concerning this test see D. Marlowe and D. P. Crowne, "Social Desirability and Response to Perceived Situational Demands," *Journal of Consulting Psychology*, XXV (April, 1961), 109–15. The practice of controlling for social desirability is not nearly so common as it might be, considering the general recognition of its importance. For a recent note on this point in the context of a prison study see S. Wheeler, "Socialization in Correctional Communities," *American Sociological Review*, XXVI (October, 1961), 697–712, esp. p. 709, n. 29.

[9] The Chillicothe Reformatory is a medium-custody federal institution that is officially defined as dealing with young offenders who are amenable to rehabilitation. The average age of the inmates in the present sample was twenty-one years. The average daily population at the institution is approximately thirteen hundred inmates.

minimal time perspective or concern for planning. Sample items:

a) It costs about $5 per day to keep an inmate at Chillicothe.

b) A survey at Chillicothe showed that 65% of the men had no disciplinary actions on their record.

2. Knowledge about *parole matters*, involving concern for the intermediate and foreseeable future and/or an awareness of the planned character of impending events. Sample items:

a) In 60% of the cases where parole is delayed beyond the date set by the parole board, difficulty in arranging employment is the main reason for the delay.

b) In the federal prisons, about 20% of those who are returned for parole violation have failed to report while on probation but have not actually committed a crime.

3. Knowledge concerning *long-range opportunities* involving the relatively distant future (e.g., anticipated employment trends or projected programs for ex-convicts). Sample items:

a) It's been estimated that the demand for unskilled workers will continue to decline in the 1960's, with the loss of about 2,500 such jobs per year.

b) A sizable increase in federal funds for vocational training of young adults who have a prison record is predicted by 1965.

The twenty-four items were systematically interspersed and were presented to the inmates in a session that came some six weeks after the original testing on alienation and social desirability. One problem was the development of a suitable rationale for the presentation of these information items so that the inmates would be motivated to read them, without at the same time arousing test-taking sets or achievement motives. The rationale used is given in the abbreviated directions: "We would like to get your reactions to some statements that are listed below. First, we'd like to get your judgment about how interesting these statements are. . . . You can do this by putting a check in either column 1 or column 2 for each statement [col. 1 headed 'Not too interesting to me'; col. 2, 'Pretty interesting to me']." As each inmate completed these interest ratings, he was given a second form that carried the same twenty-four items arranged in a multiple-choice test format.

Two significant points about the learning scores can be derived from these procedures. First, it is possible to compare the learning of these three kinds of knowledge (designated below as "reformatory," "parole," and "long-range" knowledge); and in this procedure each individual serves as his own control on such variables as intelligence, criminal history, or test-taking attitudes. Second, since the items were constructed by the research staff from documents that are not readily available to inmates, differences in previous exposure to opportunities for learning this information cannot interfere with the alienation effects in any significant way.[10]

This latter safeguard does not apply to another measure of knowledge that was used, a true-false test (also constructed by the author) composed of twenty items of the following character:

1. A "hung" jury is about as good as a verdict of "innocent" since a man cannot be tried twice for the same offense.
2. Under a mandatory sentence, where parole is not permitted, a man cannot earn reduction of his term for "good time."

Despite problems with differential experience as a factor in the acquisition of this information (e.g., some men having had a jury trial and others not, some being under mandatory sentence, etc.), this true-false test was used as a means of deriving data that would be more or less comparable with that obtained in the afore-

[10] The sources for the information items included, e.g., U.S. Department of Justice, *Federal Prisons, 1959 Annual Report*, and D. R. Cressey (ed.), *The Prison: Studies in Institutional Organization and Change* (New York: Holt, Rinehart & Winston. 1961).

mentioned study of tuberculosis patients (see n. 4).

The sample.—For the first testing session, the reformatory staff made available 120 inmates who met the criterion of having an intelligence quotient of at least 100 and a ninth-grade education (a criterion established to avoid reading problems). These men were tested in groups of approximately twenty each; and a number of interviews were conducted to check in a rough way on the correspondence between test responses and the prisoners' past activities or future perspectives. As anticipated, there were some losses in the sample six weeks later because of discharges and disciplinary restrictions. The final sample consisted of eighty-five inmates who took all of the tests and for whom background information was compiled by the reformatory staff.

RESULTS

Our central hypothesis can be put as follows: Since the alienated inmates hold low expectancies for control, they will learn less (and presumably exhibit less interest in) information that is objectively quite relevant to their careers but implies planning or taking active steps to control future contingencies. The information items concerning parole are of precisely this character; and the prediction was that the more alienated inmates would treat this kind of knowledge as though it were irrelevant. This hypothesis predicts a substantial discrepancy, between the alienated and relatively unalienated inmates, in the learning of the parole items; and this difference in learning, it is held, is not merely a reflection of intelligence or of test-taking skills that happen to be associated with low alienation. The latter point leads to the further prediction that the differences in learning between the high and low alienation groups will be less significant when we are dealing with the items that concern the immediate situation at Chillicothe (where the problem of planning and control is not directly invoked), and probably also less significant for the "long-range" items (where the forces at work and the means of control are difficult to grasp).

These comments tell us what might be expected, and Table 1 presents what was found. The distributions on alienation and social desirability were dichotomized at the median; and the retention scores (i.e., the number of items correct out of a total of eight possible points) for each of the three kinds of knowledge are given in the series of four-celled replications. It should be recalled that these are knowledge scores achieved by the same persons responding to different kinds of information.

The most striking feature of Table 1 is the effect of alienation upon learning of the parole-relevant items, and the absence of such an effect for the other two kinds of knowledge. The only statistically significant difference is the difference (between inmates who are high and low in alienation) in learning the parole material.[11]

This difference in the learning of material that bears on a critical phase of the convict's career cannot be attributed to the usual kinds of contaminating variables that might be disguised as alienation. It is not, for example, a matter of intelligence: for one thing, the correlation in this sample between I.Q. and alienation was, to all intents, zero (.01). In addition, if it were simply a matter of intelligence

[11] The differences were tested using a computer program, devised by W. J. Dixon, for analysis of variance involving unequal cell frequencies (see W. J. Dixon and F. J. Massey, *Introduction to Statistical Analysis* [2d ed.; New York: McGraw-Hill Book Co., 1957]). Some may wonder how large the difference in alienation is between the "high" and "low" groups in Table 1. This can best be given through their respective means: for the "highs," the mean alienation score was 14.30 (S.D. = 4.4); and for the "low" group the mean alienation was 5.62 (S.D. = 2.3). The distribution in Table 1 indicates that there is a correlation between low alienation and high social desirability (the r in this sample was — .36), suggesting that the control for social desirability may be useful to maintain. As anticipated, proclaiming one's mastery tends to go along with a general tendency to present a socially acceptable image.

we should find similar learning differences across the several kinds of items. The same kind of argument applies to such variables as social class, criminal experience, and the like.

Ignoring the social desirability variable (which does not seem to be a critical control in the data of Table 1), we can obtain the correlations between alienation and each of the three learning scores for the entire sample. The r between alienation and parole learning was $-.23$, a figure that is significant at the 0.05 level using a two-tailed test ($N = 85$). The correlation between alienation and "long-range" learning was $-.09$; and for the "reformatory" items the r was $-.16$ (neither of these latter two correlations being statistically significant).

It is important to think further about these correlations, for though the pattern is quite consistent with our general argument (and, reasonably enough, with the results in the analysis of variance) the degree of relationship is quite low. It is entirely possible that all three of these correlations are attenuated by the narrow range of the knowledge scores (0–8 in each case).[18] But that is a technical trouble; and there is another, more substantively interesting, source of "trouble." These over-all correlations do not take into account the inmate's relation to the institutional system of the prison—a system that has been described, in many recent studies of prison society, in terms of informal inmate roles and types. Two of these types have been labeled, in the argot of the prison, the "Square John" and the "Real Con," the essence of the difference between them being that the latter are dedicated to the criminal culture, while the former are oriented to the conventional norms of the prison authorities and the rehabilitation apparatus.

This distinction may well be critical here, since the items concerning parole not only imply that a high *expectancy* for con-

TABLE 1

MEAN SCORES AND STANDARD DEVIATIONS FOR RETENTION OF THREE KINDS OF KNOWLEDGE BY INMATES WHO SCORE HIGH AND LOW IN ALIENATION AND IN SOCIAL DESIRABILITY*

Alienation	Reformatory Knowledge		Parole Knowledge†		Long-Range Knowledge	
	Low Social Desirability	High Social Desirability	Low Social Desirability	High Social Desirability	Low Social Desirability	High Social Desirability
High ($N=38$):						
Mean	6.50	6.31	4.04	3.81	5.41	5.50
Standard deviation	1.03	1.17	1.32	1.47	1.79	1 83
Low ($N=47$):						
Mean	6.80	6.81	5.05	4.74	5.60	5.78
Standard deviation	1.83	1.18	1.39	1.79	1.91	1.18

* The four cells have unequal N's (but the same N's across the three kinds of knowledge). Beginning with the cell at the top left, and reading clockwise within the "reformatory knowledge" cells, the N's were 22, 16, 27, and 20.

† The only significant difference in this table is the difference in parole learning for the high *versus* low alienation groups ($t = 2.544$, significant at the .02 level with a two-tailed test).

[18] When the three information tests are combined to form a score on "total knowledge," the correlation with alienation remains relatively low ($-.18$) and not statistically significant. In a sense, the combination into a total score goes contrary to our notion that the several types of information will be responded to somewhat differently. The intercorrelations among the three information scores show that the expected pattern of *relative* independence holds as between the "reformatory" and "parole" knowledge scores ($r = .50$); but that these two scores correlate more highly with the "long-range" information ($r = .80$ and .81, respectively).

trol (*i.e.*, planning) is valid but also that the conventional *values* surrounding parole are valid. One might expect that, for those who reject these values, differences in alienation would be largely irrelevant to learning differentials, since for these unconventional inmates the parole and rehabilitation features of the prison represent the alien world of the prison authorities. Put more abstractly, motivation to learn is seen as being dependent not only upon *expectancies* for control of one's outcomes, but also upon the *value* one places upon the outcomes to which the learning is relevant.

Despite the fact that full information on the inmate social system was not available, there is a provisional way to explore this notion that it is among the conventional inmates that the correlation between alienation and parole learning will be high. A rough index of each prisoner's orientation toward conventional norms of the prison was obtained from the reformatory records concerning "merit earnings." These are money and/or time "points" earned by the inmate's demonstration (chiefly in work and vocational training) of his dedication to rehabilitation goals. In the present sample, fifty-nine inmates had no record of such merits, as against twenty-six who had earned at least one commendation. The correlations between alienation and the three learning scores are presented separately for these two groups in Table 2.

TABLE 2

CORRELATIONS BETWEEN ALIENATION AND THREE KINDS OF LEARNING FOR "CONVENTIONAL" INMATES (WHO HAD ACHIEVED MERIT EARNINGS) AND "UNCONVENTIONAL" INMATES (WHO HAD NO MERIT EARNINGS)

Merit Earning	*N*	Reformatory Knowledge	Parole Knowledge	Long-Range Knowledge
Yes* (conventional)	26	−.26	−.40†	−.34
No (unconventional)	59	−.14	−.16	−.00

* Inmates were classified as "Yes" if they had at least one formal merit earning on their record, these earnings being an established reward for performance reflective of positive effort toward rehabilitation.

† Significant at the 0 05 level, using a two-tailed test.

The results in Table 2 support two conclusions, both quite consistent with the previous discussion. First, among the unconventional inmates, differences in alienation are not related to learning. Second, among those who are committed to rehabilitation values, it makes a significant difference whether these values are accompanied by expectations for personal control; and, as predicted, the effect of alienation is uniquely high (— .40) where the parole items are concerned.[18]

There is yet another way to observe the significance of alienation for parole learning. Since the learning scores were based upon short tests, the results were examined item by item, determining for each item

[18] The two groups in Table 2 are quite similar on most of the variables for which information is available (e.g., age, I.Q., length of time they have been at the reformatory, and education). Nor do they differ in alienation. They do differ, however, in the months of their term that remain to be served (the mean length of sentence remaining for the unconventionals was 51.1 months, while for the conventional group it was 34.2). The correlations in Table 2 were computed as partials controlling for length of term remaining, and there was no major change in the pattern (e.g., among the conventionals the partial r for parole learning became — .43, and the partial r for reformatory learning was — .22).

In Table 2, "conventional" and "unconventional" serve as control categories. The variable controlled is "value of rehabilitation goals." Though it is not directly germane to the alienation problem, one

the percentage of those high in alienation and those low in alienation who scored correctly on a given item. This procedure allows a comparison of the items on which the two groups showed little difference in correctness of response, and those with a wide disparity favoring either the high or low alienation groups. These results cannot be presented in detail here, but two conclusions may be drawn from them. First, the parole items are the most consistent in showing superior learning on the part of the inmates who are low in alienation (they show superior recall on seven of the eight parole items). Second, it is two of the parole items that show the greatest learning difference between the alienated and unalienated inmates; and these two items clearly involve the notion of taking active steps to control one's destiny:

1. About 85 per cent of successful parolees (non-violators) have taken part in some kind of voluntary education program during their prison term.
2. In 60 per cent of the cases where parole is delayed beyond the date set by the board, difficulty in arranging employment is the main reason for the delay.

On the first of these items, 49 per cent of the inmates who are low in alienation answered correctly, as compared with 22 per cent correct among the alienated group (a 27 per cent superiority in learning for the unalienated inmates). And on the second item, 53 per cent of the unalienated answered correctly, as compared with 30 per cent among those high in alienation.

In sum, what we find is a consistent and interrelated demonstration—in the analysis of variance, in the correlations that take account of the inmates' orientations to prison norms, and in the item analysis—of the fact that the inmate's sense of personal control is a factor in determining what he actually learns concerning his criminal career. It cannot easily be argued, in this case, that the direction of causation goes the other way (i.e., that poor knowledge leads to the sense of powerlessness). Though that may very well be true under given circumstances, the procedure employed here does not encourage such an interpretation: the inmates were first tested on powerlessness, then presented with information that it is entirely unlikely they already possessed and tested for their learning of that information.

A closely related question, however, immediately arises: Do the inmates' ratings of their interest in the several kinds of information show a similar connection with alienation? It will be recalled that these ratings were obtained merely as a by-product, that is, as a technique for getting the inmates to read the information items for some reasonable purpose not associated in their minds with preparation for a test. But one might argue that the interest ratings should reflect precisely what the learning scores reflect, unless other motives in the test situation enter to distort matters (as they well might, since it is easy enough to express interest and quite another thing to show correct recall). That is, interest is the intervening variable that produces the differential learning: presumably, the prisoners who are high in powerlessness do not learn information that implies planning and control because they are not interested in such (by their lights) useless information.

The interest scores (obtained by summing, for each kind of knowledge and for the total, the frequency with which an individual checked the category "pretty interesting") yield data that are quite congruent with the above learning results and their interpretation. In the first place, if

might ask whether, with *alienation* controlled, there is a relation between merit earnings and parole learning—i.e., whether valuation of the goal of rehabilitation exerts an effect on learning that is independent of alienation. The answer is that it does not. The mean parole learning of those who have merit earnings and those who do not is very similar; and the correlations between frequency of merit earnings and amount of parole learning are, in effect, zero for both the "highs" and the "lows" in alienation.

we ignore alienation differences for the moment, expressed interest and correct learning are related in the predictable fashion ($r = .34$, significant at the 0.01 level, where $N = 84$ and the scores involved are total interest and total knowledge).

But what is more germane, there is a connection between interest and alienation. Table 3 presents the relevant correlations in a form directly paralled to the learning data in Table 2. If we remember that the mere checking of an interest category is subject to various social pressures not applicable to the learning scores, the results in Table 3 are reasonably co-ordinate with Table 2 at the main points: among the conventional inmates alienation is not related to interest in the reformatory items ($r = .05$), but, as predicted, it is considerably more relevant for the parole material ($r = .30$); while among the unconventional inmates alienation is, as our interpretation suggests, irrelevant to parole interest ($r = .05$).

One further point, deriving from a feature of Table 1, should be made concerning the interest material. It can be seen there that the "low alienation" inmates are consistently superior in learning (though the difference is slight, except for the critical parole material). One may wonder whether this consistency suggests that we may have among the "low alienation" group not only high expectancies for control but also high generalized interest in the material involved in the study, or, in other words, whether generalized interest rather than generalized alienation is not as good an explanation of our results.

The data in Table 4, however, do not appear to support the thesis of generalized interest. Here, we focus upon the crucial parole material and ask, in effect, whether

TABLE 3

CORRELATIONS BETWEEN ALIENATION AND EXPRESSED INTEREST IN THREE KINDS OF INFORMATION FOR "CONVENTIONAL" INMATES AND "UNCONVENTIONAL" INMATES*

Merit Earning	N	Reformatory Information	Parole Information	Long-Range Information
Conventional	26	−.05	−.30	−.28
Unconventional	59	−.01	−.05	−.22

* The correlation of −.30 between parole interest and alienation for the "conventionals" approximates the 0.10 significance level using the conservative two tailed test (required for 0.10 significance is an r of .33).

TABLE 4

CORRELATIONS BETWEEN LEVEL OF EXPRESSED INTEREST IN PAROLE INFORMATION AND LEARNING OF THREE TYPES OF INFORMATION, FOR INMATES HIGH AND LOW IN ALIENATION

Alienation	N	Reformatory Knowledge	Parole Knowledge	Long-Range Knowledge
High	38	−.01	.03	.02
Low	47	.09	.43*	.29

* Significant at the .01 level (two-tailed test).

the parole interest score is a specific or generalized predictor of learning. The data show the correlations between parole interest and the learning of the three types of knowledge; and these correlations suggest that generalized interest is not a good explanation of the learning data. Among the inmates who were low in alienation (where generalized interest would be expected if it is to appear anywhere), high interest in the parole material is associated only with parole learning: it tells very little indeed about the inmates' learning of the reformatory information.[14]

TABLE 5

MEAN SCORES AND STANDARD DEVIATIONS ON TRUE-FALSE KNOWLEDGE FOR INMATES HIGH AND LOW IN ALIENATION AND HIGH AND LOW IN SOCIAL DESIRABILITY*

Alienation	Low Social Desirability	High Social Desirability
High:		
Mean	14.95	14.53
Standard deviation	2.11	2.05
N	22	15
Low:		
Mean	15.22	14.70
Standard deviation	1.67	2.02
N	18	27

* None of the differences in this table are significant. The slight difference in N's per cell between this table and Table 1 was occasioned by a few inmates who failed to complete the true-false test.

We have shown, then, a pattern of differential learning that is associated with alienation, and a pattern of interest expression that certainly does not contradict the interpretation of that learning, and in many important features tends to support that interpretation. Two questions remain: (1) Do the scores on the true-false test, measuring prior knowledge rather than controlled new knowledge, follow the same pattern? (2) Are the inmates' social characteristics or delinquency histories significantly involved in the alienation and social learning relationship?

The answer to the first of these questions is given in Table 5, where it is shown that the differences go in the correct direction (for both high and low social desirability). However, the differences are very small and are far from being statistically significant. The correlation, for the total sample, between true-false knowledge and alienation was an insignificant —.08. The presumption is that differential experiences with jury trials, mandatory sentences, felony convictions, state divorce laws regarding convicted criminals, etc., may here override the presumed connection between alienation and learning.

The final question concerns the connection between the inmate's personal charac-

[14] For brevity's sake, additional material bearing on the tenability of the thesis of "generalized interest" has not been presented. For example, a table modeled after Table 1 but presenting interest rather than learning scores yielded no statistically significant differences. Its trends give little support to the thesis under review.

A more complicated argument concerning differential interest has been advanced by Richard Jessor. Its substance is that the critical difference in parole learning (see Table 1) may be simply a consequence of two facts: (1) the unalienated inmates are generally more involved in the test-taking process, and (2) this greater involvement shows up in their superiority on the harder items in the test —namely, the parole items. Thus, it is not so much alienation per se, but test-taking attitudes and item difficulty that produce the differential parole learning. This is a perceptive argument; but I do not think that the evidence when taken *in toto* encourages such an interpretation, even if the design cannot fully negate it. I refer to the fact, e.g., that the correlations in Table 2 for parole knowledge ought not to be so widely discrepant if it is simply a matter of a tie between low alienation and superior learning of the hard material: that kind of connection ought to show up among the unconventionals, too. As a means of testing this matter as directly as possible within the data, a rank-order correlation between item difficulty and superiority of learning (on the part of the unalienated group) was computed. If we say that the harder items are those that the supposedly highly motivated low alienation group does poorly on, then Jessor's thesis would require over the entire test (24 items) a significant correlation between an item's rank on "hardness" and the degree of superiority in retention shown by the unalienated inmates. The obtained Spearman *rho* was an insignificant .19 (where $N = 24$ items).

teristics or prison history and his learning. This is not so much a search for possible confounding of the learning results (since each person serves as his own control in the comparison across kinds of learning). It is more a search for clearer understanding of the way in which our controlled quasi-experimental miniature of learning relates to more macroscopic indexes relevant to learning and to criminal history that may be derived from the inmates' actual careers.

Table 6 presents this information, showing the correlation of the three basic learning scores with the "learning-relevant" and "criminal-history" indexes taken from the inmate's prison record. The correlation of the latter variables with alienation and social desirability are also presented. Most of the variables in Table 6 are self-explanatory, but two require explanation. First, the "achievement score" is intended as an index of achievement *relative to capacity*. It is based upon the difference between each inmate's I.Q. and his score on the Stanford Achievement Test. Second, "dormitory assignments" are not merely a matter of housing, but of *graded* quarters that are used as rewards for inmates. The dorms range from low-custody and less primitive quarters to high-custody and inferior quarters; and this measure probably reflects both the time the inmate has served and his avoidance of trouble-making conduct.

TABLE 6

CORRELATIONS BETWEEN VARIOUS BACKGROUND FACTORS RELATING TO LEARNING AND TO CRIMINAL HISTORY AND THE MAJOR VARIABLES ($N = 85$)

Background Factors	Learning Scores			Alienation	Social Desirability
	Reformatory Knowledge	Parole Knowledge	Long-Range Knowledge		
Learning-relevant variables:					
Age	.07	−.07	.02	.04	.17
I.Q.	.11	.07	.09	.01	−.08
Education	.24*	.10	.19	.08	−.09
Achievement†	.19	.31‡	.26*	−.27*	.11
Criminal-history variables:					
Months in reformatory	−.08	.00	.18	−.11	.05
Months until term expires	−.05	−.10	−.04	.18	.03
Number of arrests	−.05	.13	−.03	.10	−.22
Dormitory assignments§	−.02	.10	−.08	.10	−.03

* Significant at the 0.05 level (two-tailed test).

† The "achievement" score was based on a comparison of the inmate's I.Q. with his score on the Stanford Achievement Test.

‡ Significant at the 0.01 level (two-tailed test).

§ The "dormitory assignment" score was derived from the institution's grading of quarters on the basis of their tightness of custody and degree of primitiveness (a high grade being given a low numerical score).

The main features of Table 6 are quite clear, not to say dramatic. Age and I.Q. are irrelevant to the learning scores; but continuation past the ninth grade and achievement relative to capacity are quite relevant. This may be read to mean that sheer capacity and exposure (I.Q. and age) are not crucial to the learning involved here. What is more important is the individual's willingness (by remaining in school and by behaving in terms of his capacity) to expend his effort in the learning process. That is, of course, precisely what one would anticipate among those who are committed to the notion that personal control of one's outcomes is possible; and the correlation of −.27 between alienation and achievement relative to capacity directly reflects the fact that

those who are high in powerlessness are low in such achievement (though not in I.Q.). It is important to note, too, that this achievement score, which correlates significantly with alienation, also correlates most highly with the learning *of the parole material* (.31, significant at the 0.01 level); but, predictably, its correlation with learning of the reformatory-relevant material is considerably lower and not significant (.19). The most interesting feature of these results, when taken with what has gone before (esp. Table 2), lies in the fact that the inmate's learning of corrections-relevant information is shown to be related not only to his generalized expectancies for control but to his behavior—both outside the prison and inside it—which presumably reflects such expectancies. And, what is more, the results consistently show that the learning is selective in that it takes into account the personal control implications of what is there to be learned.[15]

As far as the criminal career data in Table 6 are concerned, it would seem that the length of time inmates have been at the reformatory, the frequency of arrests, dormitory assignments, or the time remaining to be served are not involved in any important way. Still, two things make one cautious about dismissing prison "time" as being irrelevant to the relation between alienation and learning. First, there is the obvious fact that "doing time" is the essence of prison life. Second, Wheeler's recent study of "prisonization" showed that inmates who were in the late phase of their prison term (i.e., those who had less than six months remaining) were involved in a "recovery process"—a kind of anticipatory socialization through

TABLE 7

MEAN SCORES AND STANDARD DEVIATIONS FOR LEARNING OF PAROLE INFORMATION BY INMATES HIGH AND LOW IN ALIENATION, BY LENGTH OF TIME REMAINING UNTIL EXPIRATION OF SENTENCE*

Alienation	Late Phase (1–29 Months Remaining)	Middle Phase (30–62 Months Remaining)	Early Phase (63 or More Months Remaining)
High:			
Mean	4.09	4.00	4.00
Standard deviation	0 90	1.30	1.57
N	11	13	13
Low:			
Mean	5.16	4.86	4.50
Standard deviation	1.12	1 76	1.99
N	19	14	14
Mean difference	1 07†	0.86	0.50

* The three time phases were obtained by a simple trichotomy of the sample so as to produce approximately equal *N*'s in the three time groups.

† The difference between high and low alienation for the late phase is significant at the 0.02 level ($t = 2.760$) using the two-tailed test.

[15] It would be possible, of course, to construct a battery of "reformatory knowledge" items that would deal directly with planning or managing one's outcomes within the world of the reformatory. Such items, however, were avoided in the present design, where the reformatory information concerned such relatively innocuous matters as the average age of the guards or the cost per day for inmate care. There is no implication here that low alienation is relevant only to the learning of respectable parole information; or that the highly alienated (in the powerlessness sense) will readily gravitate to the less respectable prison subculture. The assumption is that *expectancies* for control can determine various kinds of learning, depending largely upon the personal *values* involved.

which those who were soon to return to the community began to shed the prison culture and orient themselves toward conventional values.[16]

But in view of our earlier discussion, it seems reasonable to suggest that this relearning process in the "late phase" should be affected by the inmate's sense of control. But otherwise, the parole material used here should be well learned by inmates who are relatively close to discharge, but this should hold true only for the inmates who are low in alienation. This proposition asserts that among those who have low expectancies for control the relative nearness of discharge should not motivate parole learning; hence, learning will be relatively constant in this alienated group regardless of time. But among those inmates who are low in alienation, the poorest parole learning ought to be found among those who have the longest term remaining.

The data in Table 7 tend to support this notion that alienation is a factor in the recovery process. In this table, the sample has been trichotomized on the variable "time remaining to be served"; and the parole learning scores are shown for the alienated and unalienated inmates in these time categories. Among the alienated inmates the mean parole learning in the several time categories is quite similar. But among those who are low in alienation the poorest learning is found among those who have the longest prison term remaining. And if we look at the matter in terms of the *difference* in learning within a given time category, we find that the greatest discrepancy in learning of the parole information occurs where it is predicted; namely, among those inmates who are relatively close to discharge.

[16] Wheeler, *op. cit.*, esp. pp. 706 ff. In the course of testing Clemmer's notion of "prisonization," Wheeler notes that social learning is at the core of much sociological work in this area: "The viewpoint expressed by Clemmer is based on a direct social learning theory highly similar to Sutherland's theory of differential association" (p. 698).

CONCLUSION

One might simply say that alienation has been shown to affect the learning of parole information. But what is the significance of this demonstration? In the first place, the design employed makes a reasonably firm case for the view that we are actually dealing with a learning phenomenon—that is, that the individual's expectancies for control govern his subsequent attention and acquisition. The demonstration thus bears upon the pervasive sociological theme of the mass society and its alienative consequences. For in these writings it is commonplace to argue that the bureaucratic, specialized, and isolated individual becomes convinced of his own powerlessness, and turns his attention away from control-relevant learning: becomes politically apathetic and volatile, frantic in leisure; ignorant in international affairs. It has been shown not only that this is a reasonable interpretation, but that the logic applies in the more restricted institutional domains of health and delinquency. What that logic has to do with the sociologists' concern for union democracy, mass communication, and the like need not be detailed again.[17]

In addition, this demonstration relating to a dominant sociological viewpoint has been carried out through the use of a systematic social psychological theory—namely, social learning theory—in which the key terms are those of expectancy and reward value.[18] Thus, in our analysis of the inmates' learning of parole material, it becomes plain that prediction is enhanced when measures of both expectancy and value are employed: alienation in the sense of low *expectancy* for control surely tells us something about the inmates' learning of parole information; but when these expectancies are coupled with an

[17] For a related discussion on this point see Seeman and Evans, *op. cit.*

[18] See esp. J. B. Rotter, *Social Learning and Clinical Psychology* (New York: Prentice-Hall, Inc., 1954).

index of the inmates' *valuation* of the rehabilitation goal, a considerably clearer portrait of the learning process emerges.

Furthermore, this demonstration of the relevance of alienation for learning can be seen as an extension of the laboratory studies of learning under conditions of "internal versus external control." These studies have likewise embodied social learning theory, taking as their core problem the clarification of learning theory itself rather than the analysis of contemporary society. It has been shown, in effect, that these two kinds of problems are by no means as far removed from each other as one might suppose. The evidence is clear that the construct that has been variously called powerlessness, expectancies for control, or alienation is indeed important in the learning process—and not only among college students, or in the laboratory, or with simulated tasks, but among people who are exposed to material that is highly relevant to their immediate careers.

Finally, this study explores the degree of generality that can be attributed to the alienation phenomenon. It is, certainly, not so general as to govern all kinds of learning; and one ought to be cautious about conceiving of alienation as a kind of global characteristic which, so to speak, infects the person's generalized performance. But it is equally important to discover that the inmates' expectancies for control not only govern his learning of specific information regarding parole. The effect of alienation is reflected in the inmate's behavior both within the reformatory and on the outside, as is shown by the fact that his parole learning is related to the merit earnings he gets within the institution and to his achievement record on the outside (i.e., "living up to his I.Q.").

In short, the various controlled tests of which this paper is a part have implications for the psychologists' interest in learning theory, as well as corollary implications concerning the powerlessness consequences that are typically postulated when sociologists speak of alienation in modern society.

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RESIGNATION AS A COLLECTIVE BEHAVIOR RESPONSE[1]

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ABSTRACT

Panic as a collective behavior response to social crises has received considerable attention from social scientists, a classic case being the "Invasion from Mars" broadcast of 1938. Rational behavior in crises has also been well documented. In several recent incidents in which air raid sirens have sounded unexpectedly there seemed to be neither panic nor rational behavior. Rather the general response was to do nothing, even among those who believed that the sirens signalled an air attack. In the most recent incident reported here, the failure to respond effectively to the signal seems due to an attitude of resignation. Some aspects of this attitude are discussed.

An early work concerned with collective aspects of group behavior was that of Le Bon who described the "crowd" as being emotional, suggestible, and irresponsible.[2] Cantril's study of the public response to the "Invasion from Mars" radio broadcast in 1938 gave evidence of resulting widespread panic.[3] The Cantril study has provided a panic model of mass behavior in crisis situations—a model in which people cry, flee by driving at dangerous speeds, misinterpret stimuli, and generally become hysterical.

Research reviews of a wide variety of disaster studies suggest a rational action model.[4] One such review states, "Panic, mob action, looting, psychotic and neurotic breakdowns, and other forms of anti-social, maladaptive, or irrational behavior are much rarer in occurrence or shorter in duration than is generally supposed. . . . The resiliency and recuperability of the disaster victims . . . are almost invariably greater than would be expected by those people who have not experienced the disaster. The officials and decision-making center are often demoralized sooner, and become more pessimistic than the general population. . . . When danger is recognized as imminent and personal, people seek safety or escape and their behavior is generally adaptive. . . . Flight is one means of escaping danger. It is more often orderly and controlled, rather than disorderly or panic stricken."[5]

Given the possibility of nuclear war, practical considerations related to defense planning as well as theoretical issues impel an inquiry into possible human behavior under such circumstances. It may be that the Civil Defense research review quoted above offers conclusions based on situations as closely approximating nuclear war as are obtainable. There is, however, another line of evidence developed recently that

[1] The writer wishes to express appreciation to Arthur D. Morrell and Ralph L. Garrett of the Office of Civil Defense, to Elihu Katz of the University of Chicago for making materials available, and to the latter two and George W. Baker of the Disaster Research Group, National Academy of Sciences-National Research Council, for critical readings of an earlier draft of this paper. Responsibility for data and interpretation rests solely, of course, with the writer.

[2] Gustave Le Bon, *The Crowd* (London: Ernest Benn Limited, 1896).

[3] Hadley Cantril, *The Invasion from Mars* (Princeton, N.J.: Princeton University Press, 1940); see also John Houseman, "The Men from Mars," *Harper's Magazine*, December, 1948, pp. 74–82.

[4] Committee on Disaster Studies, National Research Council, National Academy of Sciences, *The Problem of Panic* (Office of Civil and Defense Mobilization Technical Report TR-18, Washington, D.C., June, 1955 [formerly TB-19-2]); Office of Civil Defense Mobilization, *A Brief Review of Salient Specific Findings on Moral and Human Behavior under Disaster Conditions* (OCDM BC 31196 [Battle Creek, Mich., April 19, 1958]) (mimeographed). The former report contains an extensive bibliography of both technical and popular works.

[5] Office of Civil Defense Mobilization, *op. cit., passim*.

may be able to contribute additional insights, and that is evidence from incidents in which civil defense sirens have been sounded unexpectedly. Three such occurrences have been studied by social scientists.[6] This paper will report on a fourth one.

Such studies have definite limitations. Perhaps the most serious is that the period of emergency was quite short in each case—five or ten minutes or so. Such a period of time would permit little more than confirmatory behavior as individuals attempted to verify the warning signal. It is possible that an officially *confirmed* signal of an actual attack would produce another type of behavior. Nevertheless, these incidents represent one of the closest approximations available to an actual attack and thus should have both theoretical and practical value.

The three previous siren-sounding incidents took place in large metropolitan centers—Oakland, California, in 1955, when unidentified planes were detected approaching the United States in midmorning; Washington, D.C., in 1958, when workmen accidentally set off the system one afternoon; Chicago, Illinois, in 1959, when the sirens were set off deliberately but with little warning in the evening to celebrate the White Sox' winning the American League pennant. Responses in all such incidents seemed to be neither those of panic nor of rational action.

[6] The studies are summarized in Raymond W. Mack and George W. Baker, *The Occasion Instant* (Publication 945, National Academy of Sciences–National Research Council, Washington, D.C., 1961). The studies are: William A. Scott, *Public Reaction to a Surprise Civil Defense Alert* (Ann Arbor, Mich.: Survey Research Center, 1955); *Operation 4:30: A Survey of the Responses to the Washington, D.C., False Air Raid Warning* (working paper [Washington, D.C.: Office of Civil Defense Mobilization, 1959]); Elihu Katz *et al.*, *Joy in Mudville* (NORC Report No. 75 [Chicago: National Opinion Research Center, University of Chicago, June, 1960]) (mimeographed working paper).

THE OSHKOSH INCIDENT

Unlike the other three cities, Oshkosh, Wisconsin is relatively small—slightly under 50,000 in population. Because of its size and probable lack of strategic value it would not be a likely prime target in case of war. On the other hand, the incident occurred near the height of a period of international crisis over Berlin. Less than four months before the incident, the President had given a speech recognizing "the possibilities of nuclear war,"[7] the Berlin wall had been established, and Russia had begun a series of nuclear tests culminating with a superbomb. Civil defense and fallout shelters were receiving much attention in mass communications media. Wisconsin was one of the states most severely affected by National Guard mobilization. The fact that the preceding mobilization had taken place just prior to World War II was commonly mentioned.

On Sunday, November 12, 1961, at about 8:45 A.M. a policeman on duty at the department office accidentally slipped and, in trying to catch himself, hit the civil defense siren switch. The siren went through a complete cycle before it could be stopped. Although the siren is tested regularly, the customary time is at noon on Saturdays. Thus the siren ran for a time when it was not scheduled and when its only meaning (barring accident) could be to signal an air attack. Within five minutes the local radio station reported that the alarm was accidental, but during this time there was no information that an attack was not impending.

Data on the incident were obtained from eighty-six interviews with Oshkosh residents. The sample was selected by taking the first dwelling units listed in every second column of the Oshkosh telephone directory. The relative stability of the community and the absence of substantial minority groups and those with economic hardships tend to make for a high proportion (87 per cent) of telephone-subscribing

[7] *Time*, October 20, 1961, p. 21.

families. Sample bias probably occurs in underrepresentation of those at the extremes of age and economic circumstances, single adults, and the highly mobile. Interviewing was done by advanced undergraduate students of the writer and was completed within ten days of the incident.

Of those interviewed, seventy people were in town and heard the siren. Responses to the two most relevant questions are given in Table 1.

For the sample as a whole there was practically no panic behavior. The closest to such behavior was described by a young husband who said that his wife "grabbed her coat and a transistor radio and told me to get our daughter and get out of the house." He did not know where she could have wanted to go.

Part of the lack of panic behavior may be accounted for by the common tendency to assume that the siren did not indicate a real attack. On the other hand, even those who did think that there was an attack did not report panic behavior. One such person said that he "couldn't think of anything to do so I didn't do anything." Another stated, "I don't have a shelter to go to so I just kept on with what I was doing." Respondents who were in church observed that others "got excited" or held "whispered conversations."

COMPARISON WITH OTHER INCIDENTS

It is interesting that, despite all the war tensions at the time of the Oshkosh incident, only a minority of the sample interpreted the siren as signaling an attack. A problem arises, however, in attempting to determine just what proportion of people did "believe" the signal. Only ten Oshkosh respondents (14 per cent) said that they thought it was an attack. Yet, another 23 per cent admitted that they considered the possibility of attack. It may be that some respondents have a strong resistance to thinking or talking about nuclear attack. They may be able to admit the possibility indirectly, however, with an answer such as "I wasn't sure" or "I thought it was a mistake but I turned on the radio to check." If this "intermediate" or "disguised" belief is granted, then the proportion of Oshkosh respondents who believed the signal *to some degree* was 37 per cent.

Even if intermediate believers are included, it is interesting that in *none* of the four studies did even as many as half the subjects report interpreting the signal as meaning what it "should" mean when given at any time other than that of a scheduled test.

TABLE 1

FIRST THOUGHTS AND BEHAVIOR OF RESPONDENTS WHO HEARD SIREN

($N = 70$)

	No.	Per Cent
"What thoughts did you have when you heard the siren?"		
Was not a real attack	36	51
Mistake or accident	12*	.....
Planned test	14*	.....
Did not elaborate	10*	.....
Considered possibility of attack	16	23
Signal of actual attack	10	14
Misinterpreted signal (emergency vehicle, storm warning)	4	6
Other or no response	4	6
"What did you do when you heard the siren?"†		
Did nothing in response to siren	32	46
Turned on or attended to radio	32	46
Looked outside or through window	10	14

* Part of the group of 36 who thought that siren did not mean actual attack.

† Some respondents made more than one response.

Another finding common to all four studies is that only a small proportion of those studied took protective action—6 per cent in Oakland, 5 per cent in Washington, and 2 per cent in Chicago. No one in Oshkosh reported taking protective action. Almost half of the Oshkosh sample (46 per cent) made no overt response to the signal. There seemed to be *no common reason,* however, for the lack of response. Statements ranged all the way from "I got scared, I didn't know what to do" through an emotionally neutral "I watched if every-

body else stopped their cars. They didn't so I kept right on driving" to an optimistic "I was so certain it was a mistake I didn't even turn on the radio."

DISCUSSION

Earlier in this paper two models for collective behavior in crisis situations were discussed—the panic model and the rational action model. The behavior reported in these four civil defense incidents does not seem to correspond with either model. There was very little evidence of panic, and the failure to take protective action can hardly be considered rational action.

If, however, we are to evaluate the rationality of behavior we must have a conception as to what constitutes rationality, particularly under extreme circumstances. Given the rumor of a flood, it might be agreed that fleeing to higher ground would constitute rational action. Given a signal that a nuclear attack is about to occur and given that the person has no "higher ground" (i.e., a blast or fallout shelter), what is rational behavior? None of the Oshkosh respondents had a fallout shelter prepared and available. Over one-fourth of the respondents expressed active opposition to the idea of civil defense for a variety of reasons—"it wouldn't work," "I don't believe in it," "I wouldn't want to be alive after an attack," and fatalistic resignation ("If it's my time to go, I'll go.").

Given such lack of protection, is it any less rational than other courses of behavior for the person to do nothing? An older woman in Oshkosh stated that she had no shelter to go to and didn't know what else to do so she just kept working in her kitchen. A student who was studying when the siren went off knew that she had no place to go if there were an attack and that she could benefit from her studying if there were not, so she kept on studying. Are such responses less rational than those of the fourteen people who said that they would go down in their basements in the event of a real attack, although a basement in itself will not offer adequate protection from fallout?

A classic example of panic may be found in the situation of a fire in a crowded theater. In this situation we have a basis for evaluating the rationality or effectiveness of behavior. The goal is to get out, and behavior would be considered rational if it helps people to attain their goal effectively. If the behavior hinders the attainment of the goal (i.e., if people rush to the door and block each other when they might better get out by waiting and taking their turn) then we may judge the behavior as irrational. If, however (one thinks that) there is a fire outside the theater also, so that there appears to be no advantage in attempting to escape, can we judge his behavior as irrational if he stays in his seat and does nothing to get out? The latter situation might more closely approximate that of modern warfare. Given the substantial numbers of people in each incident who did not believe the signal, it is not surprising that most people made little or no response to it. Yet one may ask why a signal of such importance—implying the destruction of people and property and the disruption of society—should be so misunderstood or disregarded.

Comparison may be made with the "Invasion from Mars" incident. Despite the fact that the "invasion" was fictional, its supposed consequences of national disaster came close to approximating the effects of nuclear war. Cantril states that "thousands of Americans became panic-stricken by the broadcast."[8] Why, when faced with a similar threat of national disaster in the civil defense incidents, did people not also respond with panic?

As mentioned above, signal interpretation did not seem closely related to behavior. In Chicago even many of those who believed the signal took no action either because they believed that action was futile or they did not know what to do.[9] Many Oshkosh respondents showed similar reactions. One woman, who believed the signal and was in church at the time, reported

[8] Cantril, *op. cit.*, p. vii.

[9] Katz, *op. cit.*, pp. 61–63.

thinking that church was "the perfect place to die." Another woman thought, "If it comes, it comes," while a man who was working in his yard at the time stopped when the siren sounded, saying to himself that "the Russians can finish this." Others said that they did not do anything in response to the siren because they didn't know what to do. Such behavior may be fostered by the not uncommon belief that there is no defense from nuclear attack—that even fallout shelters will not provide protection or that life would not be possible or worthwhile afterward. In short, apparently people showed neither panic nor protective behavior because they had already given up. It seems appropriate to denote such an attitude by the term resignation.

Sanford Gifford, a psychiatrist, has recently written, "We may believe simultaneously that no nation would dare to begin nuclear war and that if war occurred by accident, life would no longer be worth living. From either direction we reach the same conclusion, that nothing can be done, thus justifying a *fatalistic passivity* that is strangely comforting rather than alarming. This passivity is enhanced by other attributes of modern war—that resistance is impossible against universal destruction."[10] This view seems in harmony with the interpretation being made here.

In accounting for the Mars panic Cantril said, "The invasion of the Martians was a direct threat to life, to other lives that one loved, as well as to all other cherished values. . . . Frustration resulted when no directed behavior seemed possible. . . . One was faced with the alternative of *resigning oneself* and all of one's values to complete annihilation or of making a desperate effort to escape from the field of danger, or of appealing to a higher power."[11] Although Cantril thus recognized the possibility of resignation as a behavior alternative, he apparently did not consider it seriously and concluded that in a situation in which a person stood to lose all his values at once, "Panic was inescapable."[12]

In a recent book Smelser has analyzed panic, defining it as "collective flight based on hysterical belief."[13] He considers a number of conditions and their interrelationships which are necessary in order for panic to take place. A full discussion of these conditions with respect to the civil defense incidents is beyond the scope of this paper. Smelser's analysis indicates, however, that the development of panic is a complex additive process and that the threat of loss of life or values will not in itself make panic inevitable.

There are probably many instances that could be studied in which people or groups have been faced with a high degree of certainty of loss of life or important values. The inmates of Nazi concentration camps would be a case in point.[14] Sinking ships would be another. On the "Titanic," there was no panic even when the last lifeboat with room for 47 was ready to be lowered and there were still 1,600 people on the ship. Men escorted women and children to the lifeboat and then stepped back. After it was lowered those remaining were calm and quiet.[15] The reaction seems to have been resignation rather than panic.

Sherif has recognized the possibility of resignation, stating, "In cases in which positive action leading to the primary goals is barred by circumstances, a person usually indulges in individual or collective 'substitute activities' which may, at times ac-

[10] Sanford Gifford, "Death and Forever," *Atlantic*, March, 1962, p. 89. (Italics added.)

[11] Cantril, *op. cit.*, p. 199. (Italics added.)

[12] *Ibid.*, p. 201.

[13] Neil J. Smelser, *Theory of Collective Behavior* (New York: Free Press of Glencoe, 1963), chap. vi.

[14] Bruno Bettelheim, *The Informed Heart* (Glencoe, Ill.: Free Press, 1960). Handlin has argued that lack of active resistance was due to a positive religious philosophy rather than to passive resignation. However, such a philosophy seems characteristic of neither Old Testament Hebrews nor modern Israelis (see Oscar Handlin, "Jewish Resistance to the Nazis," *Commentary*, November, 1962, pp. 398–405).

[15] Walter Lord, *A Night To Remember* (New York: Henry Holt & Co., 1955).

quire pathological features. Resort . . . to fate-like resignation [is one example]."[16] This paper suggests that there might be circumstances under which resignation would not be pathological but could be considered a type of rational behavior. It may be useful to make the distinction between rational *action* and rational *behavior*, the former being a subtype of the latter and referring to purposeful directed behavior, for example, orderly withdrawal from a threatened area. Resignation would then be another subtype characterized by rational *inaction* in a situation in which no type of action would appear to be more appropriate than doing nothing.

To define resignation in this way would seem to remove it from the category of collective behavior, which is characterized by action or mobilization based on collective beliefs.[17] Yet resigned inaction is an alternative to collective action,[18] and resignation appeared in the present study to be supported by collective beliefs. Thus, it is this writer's opinion that resignation might usefully be related to the framework of collective behavior.

It is maintained in this paper then that there seem to be at least three models for behavior reactions to crisis situations—rational or adaptive action, panic, and resignation. Resignation may help to account for the seemingly inexplicable tendency for people to ignore or fail to believe a signal (i.e., civil defense siren) which is intended to warn of an event of potentially serious consequences. A signal or warning is of value primarily to the extent that one can act upon its information in a meaningful or goal-directed fashion. Given an attitude of resignation about modern warfare plus th lack of what is believed to be adequat means of protection in case of war, the si nal assumes little importance. Failure t respond to it may be due to the lack o effective behavior alternatives with whic to respond.

Why the lack of alternatives? Holdin aside the question of the physical effective ness of shelters, a nationwide shelter-build ing program would obviously have profoun military, political, and economic implica tions.[19] At the private level one factor con tributing to the tendency of the individua not to provide himself with a shelter may b that his way of life and system of values ar so "locked into"[20] present social functionin that he is resigned to the possibility of ex tinction were that functioning to be seri ously disrupted.

The concept of resignation seems to b relevant in interpreting various phenomen in the civil defense area. This paper ha questioned whether resignation is neces sarily "abnormal." The acceptance of res ignation is alien to the traditions of ou society, yet modern life both in our ow society and in others may well presen more opportunities for it than in the past— modern weapons of warfare, the individua in the large organization, and the difficult of revolting against a totalitarian govern ment could all be examples. The write suggests that resignation may be of valu in explaining human behavior in a variet of circumstances.

WISCONSIN STATE COLLEGE

[16] Muzafer Sherif, *An Outline of Social Psychology* (New York: Harper & Bros., 1948), p. 408.

[17] Smelser, *op. cit.*, pp. 8 and 71–73.

[18] An example of this is the recent statement of a Negro leader: "Unless we do something, the Negro in the future will no longer react with resignation but with bitterness and hostility. . . . We're liable to get some real violence" ("I Like the Word Black," *Newsweek*, May 6, 1963).

[19] See, for example, David R. Inglis, "Shelters and the Chance for War," and Bernard T. Feld "More Important than Shelters," *Bulletin of the Atomic Scientists*, April, 1962, pp. 18–22 and 8–11.

[20] Cf. Smelser, *op. cit.*, p. 133. To this writer it is not automatically pathological not to want to live under drastically changed circumstances. An analogous situation might be that of the person who continues working, knowing it might shorten his life, despite a medical condition. Such a person is defended by Samuel A. Levine, M.D., in "Exercise and Heart Disease," *Atlantic*, July, 1963, p. 44

LETTERS TO THE EDITOR

ERRATUM

The *Journal* regrets an error in the article by Richard H. Hall, "The Concept of Bureaucracy: An Empirical Assessment," which appeared in our July, 1963, issue. The first two figures in the last line of Table 3 (p. 37), for "Technical qualifications," should have read: — .032 under "Hierarchy of Authority" and — .300 under "Division of Labor" and *not* .032 and .300 as reported.

CASSTEVENS AND PRESS, "THE CONTEXT OF DEMOCRATIC COMPETITION IN AMERICAN STATE POLITICS"

April 30, 1963

To the Editor:

In their March, 1963, article in the *American Journal of Sociology,* Thomas Casstevens and Charles Press attempt to test the notion that "competitiveness between parties results in more benefits for citizens because the parties are forced to bid for votes. Two measures were used to determine whether competitive states were more associated with welfare-oriented policies than non-competitive states: per capita expenditures on public welfare in 1948 and average December payments of old age assistence in 1948" (p. 541).

Although no measures of association were actually employed, the authors conclude that for both indicators an association of the expected kind exists. It is unfortunate that the authors have selected two inappropriate measures of state welfare effort. Variation in state effort on payments to the aged is not measured by the above indicator, since the federal government contributed from one-third to two-thirds of the total payments in 1948. The payment per recipient tells nothing of the effort by the state to extend welfare coverage to the widest number of people in need but only how much it gave to the people it decided were eligible. The wide variation in the cost of living between politically competitive northern urban states and southern rural states contributes to the "association" found by the authors but simply increases the spurious nature of their conclusions.

The second indicator is meaningless to a reader since the authors do not tell us what is involved in its computation. This indicator actually refers to combined federal and state funds allocated by states to local areas for "welfare." The funds could be used for capital expenditures, for administrative costs, or for services. Failure to separate these activities as well as to determine the states' actual contributions lessens the sensitivity of the indicator. Again the per capita payment takes no account of what the people in the area needed nor does it tell us how many of the people in need of help received aid. The authors assume a constant need across the forty-eight states.

The use of welfare expenditures by states without taking into account the fiscal capacity of the state to raise revenues results in an inaccurate measure of the state's welfare effort. Data from continuing studies of state fiscal capacity and state tax effort reveal quite a different pattern from that shown by state personal income or composite income statistics. Southern (one-party) states are all clustered at the bottom of an income distribution but are scattered throughout the distribution on fiscal tax effort, when fiscal capacity is taken into account.

However, even if the above complaints were satisfied the crude manner of assessing the relation between variables lends itself to an oversimplified view of the relationships and to spurious findings. Even the authors' own data do not support their conclusions. A brief correlation analysis indicates that when the average 1949 income of families and unrelated persons is correlated with old age assistance payments a Pearson correlation of .69 is found, but income against welfare payments yields a modest .22. It is clear that personal income is related *differently* to these indicators. Now if we make an index to measure the degree of two-party competition by taking the absolute per cent deviation from the maximum competition possible (50 per cent of the two party vote for one party) of the average of the 1944–50 Congressional elections in each state we can correlate this index against old age payments (—.60) and welfare payments (— .29) (the index range is from 0 to 48 per cent). The partial correlation of old age assistance payments with political competition holding income constant is —.11. The partial of welfare payment with party competition holding income constant is —.21. I conclude that the hypothesis that party competition results in more benefits (of this particular kind) for citizens because parties compete for votes seems quite without empirical support. However, it is likely that this finding is the result of badly chosen indicators of political effort; alternative indicators might support the original hypothesis.

Finally the rather explicit belief of the authors that conclusions derived from their study can be extrapolated to cross national data seems hardly justified. I hope that my analysis will assuage the authors' fears that their study demonstrates that "underdeveloped nations cannot afford a competitive party system" because a "competitive party system . . . seems likely to thwart economic growth" (p. 543).

PHILLIPS CUTRIGHT

Social Security Administration

REJOINDER

June 19, 1963

To the Editor:

In our article, "The Context of Democratic Competition in American State Politics" (March, 1963), we concluded that four major hypotheses emerged: (1) Wealth and educational level are related to democratic government as defined by the degree of interparty competition, but the effect of traditions is also important. (2) Democratic competition may flourish without urbanization and industrialization but may have a different character than that found in urban areas. (3) The level of competition that the underdeveloped countries can afford, if their attempts at economic development are to succeed, may be rather low. (4) Apportionment of legislatures is less important than party competition in explaining the level of policy payoffs. Phillips Cutright has criticized only our investigation of the proposition that "competitiveness between parties results in more benefits for citizens because the parties are forced to bid for votes," which led to the third statement above.

With respect to the relationship between our study and the underdeveloped countries, Cutright seems to have misinterpreted our intentions and misunderstood our terminology. He remarks that "the rather explicit belief of the authors that conclusions derived from their study can be extrapolated to cross-national data seems hardly justified. I hope that my analysis will assuage the authors' fears that their study demonstrates that 'underdeveloped nations cannot afford a competitive party system' because a 'competitive party system . . . seems likely to thwart economic growth.' " Cutright has in effect misquoted us by tearing a clause out of a sentence. The key sentence, only part of which is

quoted by Cutright, reads, "Although the relationship between competition and policy payoffs must be treated with caution when it is extrapolated to nations, it suggests the hypothesis that the underdeveloped countries cannot afford a competitive party system." The study "suggests," not "demonstrates," a hypothesis that "must be treated with caution."

Perhaps the most critical charge that Cutright makes is that "Even the authors' own data do not support their conclusions." He attempts to prove this by means of correlation analysis. The attempt at proof is faulty because Cutright changes two of our operational definitions. First, a trivial point, since he used a Pearson correlation, presumably his "average 1949 income of families and unrelated persons" refers to mean income. By contrast, we used median per capita income. Second, an important point, he invents his own measure of competition. Since the standard measures of competition—which we used—are not susceptible to analysis with a Pearson correlation, his results, therefore, do *not* show that "the authors' own data do not support their conclusions." Indeed, our data are stronger than reported because Table 2 contains a typist's error. Under "Key Bloc" the "less competitive" and "more competitive" headings should be switched. The figures should remain as they now are.

In addition to the definitional changes which Cutright's correlations involve, his correlations are worth careful scrutiny, relative to our article, for three reasons. First, his use of the division of the vote in congressional races as the sole index of a state's competitiveness would strike many political scientists as not wholly appropriate for this purpose. The standard caveats and reasons for not doing what Cutright does are: (1) The division of the vote does not reveal wins and losses. Cutright should note how carefully all students of competitive politics have avoided sole dependence on what appears to be a natural and simple measure readily at hand. A little reflection will suggest the reason. For example, in New Hampshire, Democrats for some years have gained a sizable percentage of the gubernatorial vote without, until recently, capturing the governorship or many other offices. (2) Many congressional districts are gerrymandered so they are not competitive and overrepresent rural areas. Such races do not encourage full participation, it may be surmised. (3) The average division of the vote does not indicate the volatility of the vote. (4) Congressional races alone are inadequate as a measure of *state* competitiveness. Cutright will note that we have favored use of gubernatorial figures for this purpose. Finally, Cutright's introduction of his index is so brief that the manner in which the index is computed is not completely clear.

Second, Cutright's correlations are computed with his competition variable running from high to low while the remaining indexes run, as is commonly the case, from low to high. This is potentially misleading. Many readers will be puzzled, as we were until we calculated his correlations, and may assume that his negative correlations are the reverse of our findings. They result, rather, from Cutright's not setting up his measures in the usual fashion. In fact they confirm our findings. For example, when we recovered the correlation between income and competition, using Cutright's data, we obtained a figure of —0.8. This correlation confirms the first conclusion that we advanced in our article—competition is positively related to wealth.

Third, our statement of the relationship between competition and welfare, which we found holding wealth constant, was rather modest. "Thus party competition is associated with higher average policy payoffs, even when wealth is controlled" (p. 543). (Of course we are aware that Mississippi and New York might have different levels of O.A.A. payments irrespective of competitiveness.) In fact, Cutright's own correlations holding income constant are in the predicted direction. The correlations are low, but perhaps if Cutright had selected a more suitable measure of competition they might

have been somewhat higher (we also suggest that they will be highest where income is lowest). Cutright seems to share the same belief because he remarks that "it is likely that this finding is the result of badly chosen indicators of political effort." In short, despite his criticism, Cutright is chary of rejecting our conclusion.

With respect to our use of per capita expenditures on public welfare in 1948 and average December payment of Old Age Assistance in 1948, Cutright remarks, "It is unfortunate that the authors have selected two inappropriate measures of state welfare effort." At the outset, we should mention that our use of the Old Age Assistance payment was particularly deliberate. Old Age Assistance, unlike many other measures that might be devised, has the special value of having been a serious political issue in many states, for example, "ham and eggs," "$30 every Tuesday," the "Townsend Plan," etc. It seems to us especially appropriate in a study of political payoffs resulting from a competitive politics. In addition, Cutright suggests that our data should be adjusted for (1) federal government contributions, (2) fiscal capacity of the state, (3) cost of living, (4) need, (5) extent of coverage, and/or (6) differences among capital expenditures, administrative costs, and the cost of services. The sixth item is not clear, at least to us, either as to what is suggested, that is, which of the three costs or combination of two he proposes for the adjusted index, or as to why it is suggested, that is, the justification of the superiority of the adjusted index over the original. The fourth item appears to be extremely difficult to operationalize, and Cutright does not elucidate this problem. In practice, the first item tends somewhat to cancel out both the second and third items. For example, Cutright apparently recognizes that southern states have a lower cost of living than most northern states. Table 2, in our original article, shows that the southern states have a lower Old Age Assistance payment than northern states. Cutrigh does not mention the fact, although he doe note the variation, that the federal formul for Old Age Assistance was weighted s that the federal government paid a large proportion of the total payment in state with low payments than it did in states wit high payments. The upshot of the fore going is that the first and third items ten to cancel each other out with respect t Old Age Assistance payments, and we sus pect that the same is true for the first an second items. Finally, the fifth item strike us as cogent in principle but unlikely t change our results.

In general, aside from the ambiguity o some of Cutright's suggested adjustments if *all* the adjustments were made, it is no clear that our results would be seriousl affected. This is true for two reasons. First some adjustments tend to cancel out others Second, we were concerned with classes o states rather than with individual states This concern with classes of states stemmec from our use of the classification system fo competition which have had wide currency among students of state politics. We doubt that Cutright's proposed adjustments, i he operationalized them and compiled the relevant data, would lead to substantiall different comparisons among the different classes of competitive states. In short, Cutright engages in an a priori criticism of our welfare measures but does not demonstrate that the criticism is pragmatically valid that is, does not demonstrate that taking account of all of his proposed adjustments leads to different conclusions.

We still believe that our conclusions point in the correct direction. In particular, for the reasons given above, Cutright's criticism does not vitiate our conclusion about competition and welfare. We are indebted to him, however, for intellectual stimulation in general and for causing us to note the error in Table 2 in particular.

Thomas Cassstevens
Charles Press
Michigan State University

GREELEY, "INFLUENCE OF THE 'RELIGIOUS FACTOR' ON CAREER PLANS AND OCCUPATIONAL VALUES OF COLLEGE GRADUATES"

To the Editor: July 3, 1963

Andrew Greeley's article (*AJS*, May, 1963, pp. 658–71) provides a welcome empirical analysis of the question of anti-intellectualism among Catholic college students. The heart of his argument seems to be that there are no real differences, as some Catholic critics have proposed, between the plans to enter graduate school and choice of fields of graduate study of Catholic and Protestant students. The data on occupational values are relevant to the question, but less convincing than the findings on career plans, mainly because of the greater opportunities for "social desirability" and other response sets with this type of question.

The results on religious differences in plans for entering graduate school suffer from two major shortcomings: (1) there are no controls for the type and quality of the graduate school and graduate department chosen, for example, Catholic versus non-Catholic graduate school, productive versus non-productive department; and (2) no attempt is made to specify the amount of graduate training that is being considered, especially whether it is Master's level or doctoral level. According to the present classification system, then, a student who planned to take ten graduate hours of education at a remote Catholic college would be placed in the same category as one who planned to complete a Ph.D. in nuclear physics at Harvard.

The implications of the control for the quality of the graduate school and department are obvious. In present-day academia the reputation of the school and the specific department are prime determinants of both employment opportunities and research productivity within the profession. It is hardly a secret within our own profession, for instance, that certain departments such as those at Chicago, Columbia, and Michigan are overrepresented in their contribution to the field, while all but a few Catholic colleges, such as Notre Dame, are highly underrepresented. The Catholic self-critics to whom Greeley refers would certainly be more impressed by the findings if they demonstrated that as many Catholics as Protestants planned to obtain their Ph.D. degrees in first-rate graduate departments.

The same criticisms apply to the results on the choice of fields by "potential scholars." It seems crucial to know the probable locus of the activities of these future scholars if one wishes to penetrate to the core of the problem of anti-intellectualism. One of the most frequent criticisms made in the past is that there are too few Catholics in the mainstream of the professions—in the better and more productive departments, for instance. It is certainly no sign of intellectualism if a man plans to obtain a graduate degree and then retires to the safety of a denominational college, often without publishing even his doctoral dissertation. As Greeley probably knows from his own experience, one has to travel far and wide in the major sociology departments to uncover even a handful of Catholic scholars. It is this writer's impression that the same condition prevails in other social science fields, especially psychology.

In short, until some additional analyses are carried out it will be difficult to agree with the contention that the findings come as a "rude surprise" or with the conclusion that there is "no substantial evidence of anti-intellectualism among Catholic college graduates." In the event that there is a re-analysis, it would also be desirable to see the full sample of 35,000 used, rather than a 10 per cent subsample. In treating a question of this importance, it would be helpful to have as large an N as possible in the cells of the control categories.

DONALD P. WARWICK

Oberlin College

REJOINDER

July 31, 1963

To the Editor:

Actually, an "attempt is made to specify the amount of graduate training that is being considered"; such data are provided on page 669 (there being no difference in the proportion of each religious group expecting to study for the Ph.D.). It is possible, of course, that the Catholics will drop out after ten hours or not write their dissertations (or return to the safety of a denominational college). Fortunately, the NORC study of the June, 1961, class is a panel study, and this hypothesis can be tested. It ought to be possible to predict when the exodus of Catholics from graduate schools will begin. As of June, 1962, however, there were yet no signs of such defection.

The underachievement of American Catholics in the intellectual life is hardly being questioned. Nor is it claimed that the anti-intellectualism hypothesis must be discarded as an explanation of the behavior of the current Catholic graduate. The June, 1961, Catholics will have to do far more than merely plan to get a Ph.D. As was noted in the article, "considerably more research will be required, however, before one can in fact argue that the values o Catholics with regard to scholarship ar not different from their non-Catholic fel low Americans."

Thus graduate-school performance, com pletion of degree requirements, and es pecially quality of professional work o these students will have to be examined a the years go on. These issues are not fore closed by their plans as undergraduates What is unexpected is that such question could even be asked about Catholic gradu ates.

As to "rude surprises," Warwick is ap parently not surprised that one-third of the Catholics are going to graduate school the fall after graduation and that more than one-fifth are planning an eventual Ph.D.— unlike the prominent Catholic educator whc suggested to the writer before the study began that he "find out why our kids are not going to graduate school." But then some people are more easily surprised than others.

ANDREW M. GREELEY

National Opinion Research Center
University of Chicago

BOOK REVIEWS

Theory of Collective Behavior. By NEIL J. SMELSER. New York: Free Press of Glencoe, 1963. Pp. xi+436. $6.75.

There have been few attempts at comprehensive theory-building in the important but underdeveloped field of collective behavior. Smelser's work is a significant contribution. He proposes a new framework for classifying types of collective behavior and for identifying the determinants of the different types.

Smelser defines collective behavior as "mobilization on the basis of a belief which redefines social action." Such a belief is *generalized,* and akin to magical beliefs positing the existence of extraordinary forces at work in the universe. Two inclusive types of collective behavior are the "outburst" and the "movement." The outburst includes panic, the craze, and the hostile outburst. Movements may be either norm-oriented or value-oriented. These types are distinguished on the basis of the generalized belief that gives rise to each. Panic is based on a hysterical belief about some generalized threat, the craze on a positive wish-fulfilment belief. The hostile outburst is mobilization for action under a hostile belief involving the identification of persons thought to be the agencies of the threatening forces. A norm-oriented movement is an attempt to restore, protect, modify, or create norms in the name of a generalized belief—presumably a belief that action on the norms will reduce structural strain. A value-oriented movement acts on values in the name of a generalized belief. It is more general than a norm-oriented movement in that it involves a basic reconstitution of self and society.

The development of the different types is analyzed within the framework of a theory of social action. Undergraduate students will find this crucial section of the book formidable. There are four components of social action: values, norms, mobilization into organized roles, and situational facilities, values standing highest in the hierachy. From top to bottom, each is less central to the integration of the social order. Thus a basic proposition is that redefinition of any component requires readjustment of all components below it but not of those above it.

But each of the four components has an internal organization involving seven levels of specificity. Thus a table of "the levels of specificity of the components of social action" has four columns and seven rows. Imbalances between the various points in the system constitute the source of structural strain and the relationship between the points suggests the lines along which collective behavioral responses will flow.

Structural strain is the most general condition giving rise to collective behavior. Structural conduciveness is at least equally important and seems even to establish the scope within which strain can develop. Other determinants are: the growth and spread of a generalized belief; precipitating factors; and mobilization of the participants for action. Modifying all of these is a sixth, overriding determinant: social control.

All these determinants operate in any instance of collective behavior. The listing of the determinants in what suggests a temporal sequence does not imply a natural history approach, however, but to what Smelser calls a "value-added approach." While all of the determinants must be present for collective behavior to develop, one does not grow out of another and they need not appear in any set sequence. One may be added after another develops, but it may exist beforehand and simply be activated by the addition of other determinants.

The largest portion of the book, chapters VI–X, consists of an analysis of how the value-added process leads to each of the five types of collective behavior. These chapters abound in systematic, comparative illustrations of the operation of the determinants, drawn from a wide range of historical and sociological studies. The consistent application of a unified system of analysis to the different forms highlights the similarities in

diverse episodes of collective behavior. The author has gone far toward his goal of reducing the residue of indeterminacy in the explanations of collective outbursts.

A useful contribution to the vocabulary of the field is the concept of "real" and "derived" phases of collective behavior. The "real" phase develops from the concatenation of determinants in the value-added process. But the collective behavior itself acts as a new set of determinants leading to the "derived" phase.

This work offers a new way of examining collective behavior. It is to be hoped that this mode of analysis is proposed and accepted as being complementary to other approaches, not as a substitute for them. The approach is rigorously sociological, sometimes to the point that it becomes sociologistic. While the determinants of collective behavior are defined and illustrated clearly, the behavior of the participants seems at times almost incidental to the analysis. Differences in the spatial and temporal dimensions of compact and diffuse collectivities are disregarded, and there is little systematic analysis of the differential participation of the human actors in the collectivity. Individual differences in perception of the situation and in responses to these perceptions are acknowledged but given almost parenthetical treatment. Instead one is left with the impression that responses to the determinants are essentially uniform.

This is an excellent and much-needed analysis of the determinants of collective behavior. But while the determinants appear in sharp focus, the collectivity itself appears as a faceless blur in the background.

LEWIS M. KILLIAN

Florida State University

American Minority Relations. By JAMES W. VANDER ZANDEN. New York: Ronald Press Co., 1963. Pp. ix+470. $7.00.

This is a first-rate text on one of the oldest topics in sociology-intergroup relations. It is organized according to sociological categories rather than by specific minority groups. There is an excellent interweaving of theory and facts. It offers students a wealth of information along with an understanding of current problems; it is well-written and logically organized; and it makes fairly good use of the available literature, although it could not be said to be comprehensive.

Starting with two topics of public interest—the 1960 presidential elections and the Freedom Rides—the book moves to a general consideration of the nature of minority relations, defining race in terms of both fact and myth. Central organizing concepts are prejudice and discrimination, the sources of which are examined in the sociocultural factor; in economic, power, and prestige factors; in ideological factors; and in personality factors. The book then considers a factual description of American intergroup relations in terms of conflict, segregation, stratification, and assimilation. Minority reactions to domination are treated mainly in terms of accommodation, avoidance, and aggression, with lesser consideration given to sensitivity, ego enhancement, self-hatred, and flight from reality. The book closes with a discussion of trends and deliberate efforts toward social change.

There are at least two special difficulties for any textbook in intergroup relations to overcome. One is to make the analysis seem pertinent to the times. The "facts" are changing so fast, under the very eyes of the students, that a textbook is likely to become dated more quickly in this field than in any other in sociology. Vander Zanden apparently did most of his writing in 1961 and speaks of the 1960 election as a significant "current" event; there is little in the book that would prepare the student of 1963 to understand the great unrest stirring among American Negroes and the "crisis of conscience" being forced upon the leadership of the white society. The only way I know to overcome this quality of datedness in a textbook is to analyze trends objectively and then hazard predictions. The author has wanted to be cautious and avoid predictions of great change; the result is that his book is partly out of date as it comes off the press.

The second special difficulty of a textbook in intergroup relations is that the student today, who takes a course in intergroup relations because he wants to "understand" what is going on, is not satisfied with mere facts. He wants to understand the "attitude" of the segregationist, of the Negro, and of others in this great current drama of American society. Case materials to satisfy him are available, al-

though they are "subjective" and biased, but the author has not used them; he has sought to be objective through the facts rather than through presenting the various sides of an issue.

Yet this book is a good text. It is "solid," objective, and sociological. Perhaps we should expect no more of a text in the intergroup-relations field than in any other.

ARNOLD M. ROSE

University of Minnesota

English Landed Society in the Nineteenth Century. By F. M. L. THOMPSON. London: Routledge & Keegan Paul; Toronto, University of Toronto Press, 1963. Pp. xiii+374. $7.50.

A symptom that often indicates a first-rate book is a reviewer's difficulty in relating what the book is about. The parts are interdependent, and each is worth describing. Perhaps the central intellectual problem of this book is to explain the sentence, "In the past the landed aristocracy has done great service as well as enjoyed great wealth, and the most important service has been the peaceful surrender of power" (p. 345). Thompson shows that the criterion of landed wealth in recruitment to the House of Lords did not change much during most of the nineteenth century, that large landowners dominated the House of Commons even after the great reform, and that landlords were the political leaders who responded to agitation for repeal of the Corn Laws. More generally, he makes it clear that the landed rich administered the rising political power of the bourgeoisie, serving as more or less responsible trustees of power ultimately deriving from new social forces. By a detailed analysis of the economic, social, and political life of the landed rich, he tries to locate the structural forces and historical circumstances that account for this.

This involves attempts to answer such factual questions as the following: How rich in terms of land did one have to be before he was eligible for a peerage at different times, and exactly when did the land requirement disappear? What economic, administrative, and investment activities produced landed income, and what accounted for lowered or raised incomes at various periods? How much yearly income was needed to maintain the style of life of landed gentlemen of different degrees? What determined whether constituencies went with their main landlords, how much did a seat in parliament cost under various electoral systems, and exactly what kinds of costs were involved? When did the reform of public schools yield a culturally unified upper class, superseding the aristocratic tradition of family tutors? What role did fox-hunting play in maintaining political solidarity, and who was included?

The degree of approximation to scientific establishment of the relevant facts is very high, though the absence of formal scientific paraphernalia may lead some readers to underestimate the quality of the logic. When rates are the relevant facts, the attempt has apparently always been made to enumerate the relevant universe, the success of the enumeration is evaluated where it is known, and possible sources of systematic bias are clearly set out. Where an idea of the range of variation within a group is relevant, it practically never rests on a single case.

Roughly the answer to the main question is that the aristocracy was generally more urban and more responsive to the necessities of national government than the gentry, and dominated the landed interest. The division of the aristocracy among the two parties, the fact that the army and police were usually not strong enough to control a serious revolution, and the fact that the landed rich were by no means threatened with poverty by the rise of the bourgeoisie, were supplementary factors. The gradual decay of the bases of undisputed country power at its roots in the country, rather than by frontal attack, may have disposed to a peaceful solution.

The research is adequate to the problem, and the problem is interesting. The index is primarily organized by proper names, rather than subject-matter headings, which makes it more useful to historians than to other intellectuals.

ARTHUR L. STINCHCOMBE

Johns Hopkins University

The Feuchtwanger Family: The Descendants of Seligman Feuchtwanger. By a COMMITTEE OF THE FEUCHTWANGER FAMILY. Tel Aviv: Edition Olympia, 1952. Pp. 151. $5.00.

Chronicle of the Lauterbach Family: Descendants of Jacob Bezalel Lauterbach of Drohobycz. Jerusalem and Los Angeles: Lauterbach Family Fund, 1961. Pp. 184.

Genealogy has been of little interest to sociologists thus far, although it is obvious that it could be a potent tool, not only for demographers, but also for the study of social change. This refers to genealogical research *ab initio* as well as to the sociological utilization of studies already done. It would therefore seem to be of some importance to American social scientists to take cognizance of a new kind of genealogical literature which has emerged in Israel in recent years. The histories of the Feuchtwanger, Auerbach, and Lauterbach families (of which only the first and the last are reviewed here) were written because the dispersion of their surviving members as a consequence of the upheaval of the Hitler years in Europe made it desirable to take stock of what had happened and to forge a new bond of family cohesion. But comparable reasons exist in this country, considering the transformations that have come about through international and internal migration and the generally shifting socioeconomic status.

Technically, the Feuchtwanger history blazed the trail. Contrary to earlier usage, each generation has been set out horizontally, with the heading representing the first generation, the first column the second generation, and so forth; the children are numbered chronologically, according to the date of their birth. The tables contain merely names, while the biographies of the descendants and their consorts are listed in alphabetical order in a second chapter. There follow a historical sketch dealing with the origin of the family and, finally, valuable if incomplete comments of a statistical nature. The Lauterbach publication, in addition, contains brief biographical and autobiographical notes, some of them conventional, but others highly personalized, and a list of martyrs. Most of the textual remarks in both publications are in English.

The two family histories are comparable because they span about the same period, comprising the emancipation of the Jews in Central Europe, their rise on the socioeconomic scale, and the Hitler catastrophe. Seligman Feuchtwanger, the ancestor of the F. family, was born in Fuerth, Bavaria, in 1786; Jacob Bezalel Lauterbach, the ancestor of the L. family, was born in Drohobycz, Galizia, in 1800. They had 18 and 10 children, respectively. All in all, 1,395 members of the F. family and 930 members of the L. family are listed.

There are tabulations referring to marital and personal status (including childless marriages and mixed marriages), number of children, occupations, countries of origin and of domicile, and, in the case of the F. family, also cause of death. Some of the tabulations are not comparable, for example, those regarding the losses sustained during the Nazi period. Others could have been more specific, for example, in the case of occupations, where classification according to generation would have made the transformation that occurred during the last 160 years more clearly visible. But the decline in the birth rate—from about five children per family in the first generation to between one and two children in the fourth and fifth generations—is unmistakable, even if one takes into account the fact that family size in the last generation is not yet complete. The increasing professionalization of both families and the decline in their birth rate may be functionally related. Further studies of this kind, using more refined statistical methods as well as more intimate biographical data, should be encouraged.

WERNER J. CAHNMAN
Rutgers University

Education and the Working Class. By BRIAN JACKSON and DENNIS MARSDEN. New York: Monthly Review Press, 1962. Pp. 268. $6.75.

How does the intellectually able English working-class child come to terms with the conflicting demands of his social surroundings and the school system? For the American reader, *Education and the Working Class* provides a suggestive cross-cultural focus for the general problem of social class and public education. The scope, style, and method of the work are stated accurately in the subtitle: "Some general themes raised by a study of 88 working-class children in a northern industrial city."

The authors "were formed by the grammar school world of Marburton, and for some years [their] natural line of interest . . . led to constant discussion around questions of social class

and state education." As social observers, they drew upon their own experiences, official records, statistical reports, and the writings of others. Returning to Marburton, they interviewed the forty-nine men and thirty-nine women (and their parents) who had passed the G.C.E. examination at the advanced level during specified periods. For contrast, seven men and three women (and their parents) selected randomly from those on the pass list with middle-class backgrounds were also interviewed. Detailed statistics on the composition of the samples, and a few crossbreaks, are presented. The interviews provided a wealth of interesting quotations and illustrations. Rather than attempt systematic analysis of the small samples, the authors tried to "go behind the numbers and feel a way into the various human situations they represent."

The strengths of the volume are its sensitive analysis of social mechanisms and pointed discussion of the policy implications of research. The authors investigate social causation by confronting questions raised by data with illustrations in rich yet relevant detail. Wherein lies the advantage of the middle-class child now that fees have been abolished? Why do educationally successful working-class children overselect certain occupations? Why do they lack objectively accessible information on entrance requirements and available scholarships? The approach permits a subtlety in the analysis of such issues beyond the reach of more rigorous and precise procedures.

American sociologists who evaluate the book in terms of its goals rather than their expectations will find it stimulating, insightful and informative, but disorganized.

RICHARD F. CURTIS

Yale University

Scientists: Their Psychological World. By BERNICE T. EIDUSON. New York: Basic Books, 1962. Pp. xiv+299. $6.50.

During the thirties there appeared a book entitled *Scientists Are Human.* Since that time scientists have acquired considerably more social visibility, but much of the popular (and some of the "scientific") treatment of the profession has made them appear as either superhuman or all too human. The study under review seeks to throw light on the validity of the prevailing images and stereotypes of scientists.

Eiduson's methodology and frame of reference follow the pattern set by Anne Roe (cf. her *The Making of a Scientist* [New York: Dodd, Mead, & Co., 1952]). Both authors are psychologists married to biologists. (Is this significant?) Both chose "eminent" scientists for their sample and used projective tests and interviews to investigate the nature of scientific creativity (as well as creativity among artists), the personality patterns of scientists, and their "life-style." It seems unfortunate that Dr. Eiduson, although well aware of the work of her predecessor, did not see fit to make a more systematic comparison of the approaches and results of the two studies and thus failed to maximize the cumulation of knowledge in this area.

The most obvious defect of the present study is the lack of comparisons between scientists and other occupational groups. We are told that nineteen out of the sample of forty natural scientists did not know their fathers very well, that feelings toward their mothers were typically ambivalent, and that isolation from the group at an early age and later was a common factor. But how can we tell that this is not equally true of eminent politicians, funeral directors, or baseball players? The only clues in that direction are provided in the Appendix, where Rorschach and TAT results differentiating between artists, businessmen, and scientists are reproduced.

On the positive side of the ledger one is struck by the fact that the author was able to transcend the limitations of the clinical frame of reference and to adopt instead a "field" or "sociological" approach. In describing the scientists' personality patterns she points up the importance of the institutional setting in controlling and channeling "neurotic" behavior into socially productive activity (pp. 97, 105 ff.) A variety of personality needs are compatible with the requirements of a variety of scientific roles. Potential role conflicts, as for example between the "search for truth" and the necessity to work within a bureaucratic organization, are successfully headed off by the scientists' use of various mechanisms. In fact, their status as "eminent" scientists could be a function of the successful manipulation of such devices.

The book provides insights and hypotheses

for the study of "plain" as well as eminent scientists. The author suggests that recruitment of capable young people into science should capitalize on the fact that science is a house with many mansions, providing room for a range of personality needs, including that of the "gentleman scientist" who should feel no guilt about spending time and energy in pursuing art and politics. Here again a comparative approach is indicated, utilizing data such as those gathered by Edgar Friedenberg and his colleagues at Brooklyn College on students who have left the physical sciences. Similarly, we need to know more about the variety of scientific roles, such as the research administrator. It is to be hoped that Eiduson's work will stimulate the accumulation of knowledge about the sociological as well as the psychological world of scientists.

WALTER HIRSCH

Purdue University

Structures of Custodial Care: An Anthropological Study of a State Mental Hospital. By RICHARD F. SALISBURY. ("University of California Publications in Culture and Society," Vol. VIII.) Berkeley: University of California Press, 1962. Pp. v+138. $3.00.

This study of two wards of a large state mental hospital for chronic patients focuses upon the divergent perspectives of doctors, attendants, and patients, and their interactions. Much of the analysis hinges upon the observation that the patient's hostility is directed toward those who exercise authority over him, and once he has selected this person or role group, the choice of persons to like is made regardless of the content of their behavior, as long as they do not exercise obvious authority. In the closed ward, the attendant controls the patients and thus is disliked, while the distant and ineffectual doctor can be liked. In the open ward, patients can be trusted so the attendant need not exercise much authority and is liked, while the doctor, who does have to make decisions, is disliked.

Salisbury argues that the difference in behavior on the two wards is due to different ward structures rather than characteristics of patients. Thus he can make his central recommendation for increasing the rehabilitative potential for patients with certain types of illnesses: make one doctor responsible for administration on wards but keep him distant from the patients (block some communication channels) and draw hostility toward him. Then "rehabilitation workers" (other doctors without administrative responsibilities, attendants and the various para-medical therapists) will be liked and can get close to the patient. As the patient is gradually rewarded for compliant behavior they will cease to support his hostility toward the administrative doctor and bring the patient to a realistic perception of the therapeutic role of the administrative doctor as well as that of the other workers.

The sketchy model of this revised structure has a suspicious similarity to present relations on the open ward where the patients are already in contact, can be trusted, have behaved well, can work at hospital jobs and so forth. No consideration is given to how the workers are going to handle disturbed patients without exercising the authority that leads to rejection. His analysis of closed ward behavior repeatedly makes it clear that the patients' behavior is due to the individual characteristics of their illness, and that the ward structure, rather than being causal, is really adaptive.

Indeed, this analysis is one of the things that makes this book well worth reading for specialists in mental hospital research and social psychology. For he generally avoids the excesses of attributing much of the disturbed behavior of patients to staff practices or authoritarian structures. He insightfully analyzes how patients' perceptions are functional in view of their illness, but he notes they are distortions nonetheless. Attendants, on the other hand, usually exhibit appropriate and rational behavior under the circumstances. Characteristics that might have produced a highly custodial score on the familiar custodial mental illness scale are here seen in a different light. Compared to much of the literature, which would have the small society sick and the patients well, his observations appear singularly rare and astute. Salisbury also provides us with a number of valuable insights, such as the fact that the incomprehensible behavior of patients with their visitors is because the latter are the only persons the patient is free to order about and exploit. In another example he treats the forcing of candy or cigarettes upon another patient as a degredation ceremony, establishing status and power.

The absence of any really effective summary of the book, and the obscurity and/or generality of those that do appear in chapters or on page 118, undercut the force of his recommendations and his presumed theoretical contribution, modest though his claims are. This is simply not a very clear or powerful analysis. Yet he is very perceptive in many places, and his description of life on these wards is unusually balanced and complete.

CHARLES PERROW

University of Michigan

The Sociology of Crime and Delinquency. Edited by MARVIN E. WOLFGANG, LEONARD SAVITZ, and NORMAN JOHNSTON. New York: John Wiley & Sons, 1962. Pp. xiv+423. $4.45 (paper); $6.75 (cloth).

This book contains a good variety of well-chosen selections from the best sociological literature on crime and delinquency. The selections are sensibly organized into seven major sections which, in turn, are introduced and knit together by brief yet thoughtful editorial comment. The principal divisions of the book have to do with the nature of criminology, problems of the statistics of crime, methods and techniques of analysis, empirical work on crime and social structure, theoretical selections on values and social structure, family setting and delinquency, and selected patterns of criminal activity. For a book of this kind, which makes no pretense of final synthesis, the coherence of the whole is most satisfying.

The conflicting demands of comprehensiveness, on the one hand, and space limitations, on the other, often lead the editors of works such as this to arbitrary and dubious editorial compromises which, by and large, the editors of this book have managed to avoid. There are some notable exceptions to this, however. For example, in an excerpt from a stimulating article by Cressey on the development of differential association theory we are told (p. 89): "As we shall see below, a principle accounting for the distribution of deviancy . . . can be valid even if a presumably coordinate theory specifying the process by which deviancy occurs in individual cases is *incorrect,* let alone untestable." Yet the reader never gets to see "below" where the meaning of this interesting statement is elaborated because the relevant part of the original article was omitted in the editorial process. Or, again, in the section on methods and techniques we find a frustrating editorial compromise. Here the reader is presented with a series of excerpts illustrating the case study, ex post facto, and experimental techniques that are so dissociated from the major findings and conclusions of the original studies as to make exceptionally uninteresting reading. Perhaps this is the place where a single, new review and critique of the relevant literature would have been most welcome.

Prior to the publication of this book it was difficult for most teachers to make readily available to their students the choicest sociological writings on crime and delinquency. Having brought these together under a single cover, the editors of this book have helped to meet a pressing need. The book should prove valuable to graduate students by way of reference and as a general introduction to criminology. It will certainly make an excellent text for undergraduate courses in crime and delinquency and may be used to great advantage as collateral reading in conjunction with other texts. In short, this is potentially a very useful book and a welcome addition to the literature of criminology.

CHARLES R. SNYDER

Southern Illinois University

The Twilight of Cities. By E. A. GUTKIND. New York: Free Press of Glencoe, 1962. Pp. x+201. $5.00.

E. A. Gutkind is professor of urban studies at the University of Pennsylvania, a close student of urban history, and an experienced practitioner in the redevelopment of European cities after the last war. In this book he argues that the urban environment should be planned in terms of a number of centerless regions. Each region would be made up out of many small self-contained cities filled with parks and garden apartments and surrounded by green belts. The small cities would provide their residents with a comprehensive range of facilities and services so that people could work, live, and play there without the need to travel often to another community. Given the

variety of activities and pleasures that contemporary man demands and enjoys, Gutkind recognizes that specialization of functions among settlements may be necessary, especially to support cultural and educational endeavors; but he is emphatic that it should be kept to a minimum.

The general ideas about the proper arrangement of urban settlement discussed in this book belong to the body of planning thought called "regional planning," one of the venerable intellectual traditions of the city planning profession. Important figures in the planning profession in America, such as Clarence Stein, Lewis Mumford, and Henry Wright, all were opposed to the great city and advocated instead the controlled development of communities of moderate size. To most regional planners the centerless region derives its value from its promise to eliminate the worst blight caused by concentrated settlement, including traffic congestion, slums, air pollution, and the need for incessant commutation between cities and suburbs. In the hands of Gutkind, it offers the further prospect of reducing the alienation of man from his work by bringing "the functional life" and "the personal life" closer together and promises to cleanse and restore the human spirit by encouraging the encounter of man with nature and landscape.

Once regarded as incontrovertibly worthwhile, but also thought to be hopelessly utopian, the goal of a centerless region is no longer remote. Gutkind points out that with the exception of a few great cities, of which New York is the prime instance, the ability of densely settled urban areas to hold, or to attract back to their midst, the prosperous population of the nation has proven limited. Many cities in the nation are scarred by large pockets of cleared land intended for urban renewal but which real estate developers will not purchase or on which they refuse to build. Along with the "twilight of cities" we find that many suburban communities are beginning to attract not only industry, but also a number of cultural and educational facilities that once were found exclusively in the great city itself. These suburbs might be the nascent self-contained communities of the centerless regions of tomorrow. However, at the very moment in time that urban history promises to confirm their vision, the ideals of the regional planners, as distinct from their historical generalizations, are widely disputed. For example, critics and students of cities have become sensitive to the importance of urban values for which the convenience of living close to nature and being relieved of many problems of congestion and blight offer no substitute. Those who enjoy and love cities today are attracted by the opportunity to discover within a few blocks a variety of ethnic groups, cultural institutions, shops, theaters, and restaurants. It is often argued that traffic congestion, uncontrolled growth, high rentals, indeed slums themselves, are the necessary price for the diversity, vitality, and individuality that the city traditionally has offered to its residents and users.

In fairness to Gutkind it must be pointed out that he is not unaware of the charge that the urban world of the regional planners offers a dull and unexciting prospect. He answers that a combination of new highways and other advanced modes of transportation will make it possible for the residents of the centerless region to taste diversity by traveling quickly from one settlement to another. Unfortunately, this image of commutation on the grand scale does not quite meet the real criticism directed against the regional city. There is a difference between the planned consumption of variety that the centerless region would require of its residents and the casual encounter with diversity that is available to any pedestrian in New York, Chicago, or San Francisco. Modern life already demands too great a repression of impulse and too much rational calculation in its effort to assure a minimal level of human satisfaction. I worry about the fact that Gutkind's region will only intensify the need to plan the decisions of personal life, and I wonder whether the additional burden will not more than outweigh whatever advantages the centerless region offers for a closer relationship to nature and the ease of living in small communities.

ROBERT GUTMAN

Rutgers University

Mathematical Methods in Small Group Processes. Edited by JOAN H. CRISWELL, HERBERT SOLOMON, and PATRICK SUPPES. Stanford, Calif.: Stanford University Press, 1962. Pp. viii+361. $9.75.

R. R. Bush's own verdict on linear stochastic models of learning as applied to individual humans is that they are no good, or so I gather from his brief sardonic chapter in this symposium, the eighth in the Stanford Mathematical Studies in the Social Sciences. C. J. Burke, however, is ebullient: "Recent successes in systematic learning theory have led to systematic research by a number of investigators whose aim is to represent and predict two-person interactions on the basis of the results of learning experiments" (p. 51). Clearly Burke's is the predominant mood since about half the contributions in this symposium are applications of the familiar linear or Markov models, decked out in a little new finery, to dyadic and small-group situations.

To this bystander Bush's pessimism seems justified by these contributions. The parameters of the models remain mere fitting constants, once again no attempt being made to develop empirical, much less theoretical, explanations of the variation in parameters from one individual, situation, or type of respondent to another. The impressive parsimony of the original models is abandoned as additional arbitrary assumptions and parameters are tacked on (e.g., the Atkinson and the Suppes and Schlag-Rey chapters). The idea that genuine insight into the psychological mechanisms actually at work is needed to develop valid models is nowhere seen, except in Bush's chapter. Quack norms are the obsession, norms that represent neither some rationally optimum solution for efficient learning in the spirit of game theory nor the norms to which given subjects in given situations actually orient themselves. As Bush and Mosteller made clear initially, quack norms would be justified, as an intermediate step to simplify the development of genuine theory from very intricate data, only if rigorous fits to parsimonious models of the simplest situations were achieved.

W. K. Estes proposes a radical reformulation of how a subject in a binary choice experiment perceives his alternatives (p. 142—the verbal description on the preceding page is highly misleading). Reinforcement probabilities (Π's) are contingent upon response in the design assumed. The subject is assumed to predict not the probability of reward for each response but the probability that if *both* responses were made on the next trial both would be rewarded, or neither, or one, or the other. Naturally the setup must be such that the subject does not perceive reward of one response as necessarily exclusive of reward of the other response if he had made that instead, but most experiments modeled on the simple T-maze are not inconsistent with this, not that experimenters usually bother about how the subject *does* perceive the experimental setup. Estes then invokes the probability matching theorem (asymptotic response probabilities tend to match the probabilities of reinforcement), which seems truly extraordinary since the subject can never make both responses simultaneously in fact. Assuming the subject uses an arbitrary decision rule to break ties (there are only two reward magnitudes), Estes derives formulas for asymptotic response probabilities of the (individual) responses solely from the contingent probabilities of reward. Estes is comfortable (p. 144) about the ability of his model to fit data because if they do not fit then he assumes the asymptote has not yet been reached. Since the only condition Estes stipulates for his reformulation to be applicable is that the rewards be clearly discriminable, one can legitimately test his predictions (eq. 13) against the data in Tables 13.4 and 13.5 in Bush and Mosteller whether or not there is a money payoff: in each case where his prediction differs from that of the equal α model the latter is at least as good and usually much better, although of course its parameters are fitted to the data and perhaps the asymptote had not been reached. In my mind Estes' reformulation simply serves to direct attention to how shaky and arbitrary the distinction between experimenter-controlled and contingent-reinforcement experiments is. What good evidence is there that subjects can tell contingent from non-contingent reinforcement when both are random in detail, in each of the very numerous possible experimental setups?

Another type of weakness is apparent in Seymour Rosenberg's chapter, which again is technically proficient and accurate. Once you grant the simple cybernetic model he uses for an individual moving a target point toward a goal line on a screen under the distraction of random noise jerks along the vertical axis, replacing the noise generator with a second subject (who in turn is moving his own point on another screen toward a goal line against the distraction of vertical jerks set off by the first subject's adjustments) he does not introduce any interaction of sufficient psychological

interest to be worth studying. Rosenberg would obtain the same results if he used in place of the second subject a fancier noise generator whose mean output varied with the first subject's responses. The difficulty is much the same as that of the earlier work on communication in small groups—they controlled so many things that there was no possibility of finding out interesting things about human interaction: the subjects were simply relatively inexpensive servomechanisms largely programmed by the experimenters (see sec. 3 of T. B. Roby's chapter and sec. 4 of the contribution by O. K. Moore and A. R. Anderson for similar verdicts).

Unfortunately the chapters not centered on learning theory have little more to offer in the way of substantive insight and, by and large, are not as competent technically. There are also a few largely verbal essays, which while stimulating do not seem to lead in any definite direction and are perhaps out of place in a volume dealing with mathematical methods.

HARRISON WHITE

University of Chicago

Statistical Principles in Experimental Design. By B. J. WINER. New York: McGraw-Hill Book Co., 1962. Pp. x+672. $12.50.

Statistics texts may be roughly categorized into two classes: those that are unintelligible (except to a mathematician) and those that are full of half-truths and occasional downright errors (such as a misstatement of the central-limit theorem in a currently popular social statistics text). Twenty years ago Harold Hotelling provided a caricature of the "interesting literary cycle" followed by textbooks written for the non-statistician, but with few exceptions the statisticians have not met their responsibility of providing texts that communicate in simple language without oversimplifying to the point of implying erroneous interpretations. Statisticians are well aware of the problem, as is illustrated in a remark I once heard that students who have been through a typical introductory statistics course have a harder time learning the subject properly, when exposed for the first time to a competent treatment, than students without prior training. The problem is also illustrated in the remarkably critical review article of Siegel's *Nonparametric Statistics* that appeared in the *Journal of the American Statistical Association*, in spite of which the book continues to be a popular and standard reference for users of statistics.

Because of the reception accorded Siegel's book, I am inclined to hedge my bet that this book, which is part of the same series, will be much better received by the statistical fraternity. The field of experimental design has become a field of considerable complexity in the past twenty years, and I am not enough of a specialist to know what fine points of theory have been distorted by this author. Although it is my impression that the book is an extremely competent piece of work, a potential user of this book should check reviews in the statistical journals. I am not in a position to give the Seal of Statistical Approval.

The book opens with an extremely terse but clear ten-page review of the concepts of contemporary statistical inference, material which is then applied in the following chapter to hypotheses about means and variances. The main discussion begins in chapter iii, which together with chapters v and vi presents the analysis of single-factor and multifactor experiments (i.e., what are sometimes referred to as one-way and multiway analyses of variance). Chapters iv and vii are especially useful in describing procedures to be followed when repeated measures have been made on the same subjects (hence observations are dependent). Chapter viii deals with designs in which some interactions are confounded; chapter ix with various other incomplete-block designs, chapter x with Latin squares and related designs. Finally, chapter xi gives a brief treatment of the analysis of covariance. There are two useful appendixes, the first dealing with ranking and contingency table analogues of the analysis of variance among other topics, and the second containing a full complement of somewhat specialized tables.

One of the virtues of the book is its exposition. The author writes clearly and with neither ambiguity nor superfluity. The notational problems in this field are enormous, and they are handled here with great ingenuity and apparent accuracy. Difficult proofs are sketched out and "talked through" in a fashion that I think does no violence to the theory and makes them intelligible to the hard-working student who lacks the requisite mathematics. In addition to an algebraic statement, a simplified

numerical example is often given, as well as an application from the literature (almost entirely experimental psychology, incidentally). This careful exposition helps to account for the length of the book. Another virtue of the book is the inclusion of much recent work. Nearly half of chapter iii, for example, describes and compares the various methods that have been devised in the past decade for handling comparisons of particular means. This recency is also apparently true of most of the work on correlated measurements in chapters iv and vii. As might be expected in the reporting of recent work, there remain some differences of opinion among theorists, and it is appropriate that these are noted by the author.

The one aspect of this subject not given explicit discussion is perhaps the most important for many readers. That is the logic that motivates the idea of a hypothetical population of which the particular experimental observations are considered a random sample. The standard work on this topic for statisticians, the volume by Cochran and Cox, begins with a very thoughtful introduction to the idea of inherent experimental variability, and the rationale for comparing the "treatment" variability to the error or residual variability in an F-ratio. Oddly, the present work does not contain "randomization" in the index, and although of course the term is used, one feels the need for an explicit discussion. It is too easy for a student to ignore the assumed random elements in his data, carry out an F-test, and decide that he has found something "significant" in his data, where a little reflection might show that hidden biases raise questions even about the direction of the results, to say nothing of the "significance." It seems unwarranted to treat survey data as a natural experiment, and assume, as Kish suggests, that uncontrolled variables have been randomized, without some evidence in favor of such an assumption. While statistical inference is intended to introduce a note of *caution* into an analysis, by reminding the investigator of the omnipresence of random variation, it sometimes seems that the method leads people to make untested assumptions, perhaps unconsciously, on the basis of which the data analysis ends with a note of discovery or affirmation. Because of a failure to discuss such issues in the present book, it might better have been entitled "The Statistical Analysis of Experimental Designs, Simple and Complex," for a discussion of which it can be highly recommended.

ROBERT H. SOMERS

University of Michigan

International Migration and Economic Development: A Trend Report and Bibliography. By BRINLEY THOMAS. Paris: United Nations Educational, Scientific and Cultural Organization, 1961. Pp. 85. $1.50.

Under the auspices of UNESCO, Brinley Thomas has prepared this brief volume that proposes to summarize and criticize our theoretical and empirical knowledge about international migration. The book consists of separate chapters devoted to migration in pre- and post-World War II periods. However, the examination of prewar migration is flimsy indeed, being restricted to eight pages of text and one table from Carr-Saunders. A fair part of the prewar survey is essentially a statement of Thomas' thesis in his *Migration and Economic Growth.* Actually, there are some pre-World War II subjects found in the larger postwar section of the work.

The economic analysis section of the chapter covering post-World War II international migration was the most useful part of the book for this reader. It consists of a number of short statements of problems, theories, and counterarguments that have been developed by economists and others on the effects of migration for the receiving and sending countries. The volume also includes a bibliography of 397 publications and this is always of value. Unfortunately it is not annotated and, as far as I could tell, many of the works are not referred to in the text.

A surprisingly large part of both chapters is devoted to migration to the United States and within the British Commonwealth. Although this is perhaps due to the author's previous interests, one is disappointed to find virtually no attention paid to major non-white migration flows such as that of the Nan-yang Chinese in past decades or the considerable intra-African movements. Perhaps this is too much of an assignment for a book of less than 100 pages. However, the net effect is a shoddy product. The section devoted to demographic and social aspects of international migration since World War II is meager and is actually weighted by issues covering earlier periods, for

example, Walker's replacement theory. Parts of the survey consist of references made in a most vague fashion with hardly any illumination of their contents. In some instances, this amounts to no more than the titles of studies, authors, sources of financial support, and the like. Also, much of the discussion is quite scanty. For example, the examination of intermarriage is based upon one article.

I was rather surprised to learn in the section on "Problems of Assimilation" that the term "assimilation" is now passé since the majority of writers find "integration" superior. I trust this will surprise more than a minority of readers. Curiously, I noticed that Thomas utilizes "assimilation" in several instances in succeeding discussions.

In view of Thomas' earlier contributions to our knowledge about international migration, it would appear the author has been burdened here by either too broad a topic or too short a book.

STANLEY LIEBERSON

University of Wisconsin

Man Alone: Alienation in Modern Society. Edited by ERIC and MARY JOSEPHSON. New York: Dell Publishing Co., 1962. Pp. 592. $0.95 (paper).

And still another reader. This one—organized around the term "alienation"—joins an already impressive list of readers dealing with the problems of mass society and mass culture. There is something ironic about this growing list of readers; nothing so clearly epitomizes the scholarly life in a mass society as the reader itself: what is it, if not an attempt at instant scholarship?

The editors preface their collection with a lengthy and needlessly pretentious introduction on the concept of alienation. (By an endless string of "as ——— observes," "as ——— writes," "as ——— states"—in almost mechanical alternation—they manage to create the impression that alienation as a problem and concept is the single culmination of the disparate streams of social thought of the last century.) While it is generally agreed that this concept has become a hopelessly complex metaphor that lends itself to multiple uses and endless ambiguities, it is important to recognize that intellectual discourse is rarely conducted with a high regard for terminological clarity. The concept will obviously continue its wide currency. And there is no reason why responsible sociologists cannot add or provisionally create some specificity for the concept as part of a contribution to this discourse. Unfortunately, in linking this concept to an amazing diversity of social behavior, the editors have only added to this complexity, at the expense of its potential power. With their encouragement—though it is hardly needed—alienation can now freely float with its near-relative anomie, providing at no expense instant profundity, instant sophistication.

Despite these very general objections, *Man Alone* has a number of virtues. Not the least of these is that it is remarkably inexpensive. (One shudders at contemplating its cost were it to have been produced by a publisher more self-consciously dedicated to enriching the academic life.) The selection of contributors is delightfully varied. Ranging from Marx, Simmel, and Merton to James Baldwin, William Burroughs, and Donald Corey, touching on themes of work, culture, politics, sex, mental illness, and contemporary "experiments" in social organization, there is hardly a conventional sociology course offering for which the volume does not contain a relevant selection. Here is a source book that a teacher may freely assign without burdening his students financially and, at the same time, providing an opportunity for some highly worthwhile browsing. Much of the writing is very "literary," but this very excess is a refreshing contrast to the conventional "academic baroque" or "academic caution" to which we are more accustomed.

Despite the dubious value of its conceptual framework, the book provides at very little cost many rich and stimulating selections. It does, however, pose some embarrassment for the profession. Were one to re-create this volume with a greater emphasis upon systematic and empirical work, what would one want to include? Where are the empirical analogues to Baldwin, Corey, and Swados? For that matter, one would have difficulty finding such analogues for Marx, Simmel, and Merton. Given the currency of the term "alienation" in social science circles, it is rather strange that so little empirical work has been done in this area.

WILLIAM SIMON

National Opinion Research Center

The Truth about the Mafia. By GIOVANNI SCHIAVO. New York: Vigo Press, 1962. Pp. 318. $6.95.

In this interesting, well-documented, occasionally journalistic but usually convincing book, this proudly Sicilian author of many books about Italy tells the brief history of the Mafia in Sicily, describes its decline, the many misconceptions of the term, and the development and present status of organized crime in the United States.

According to Schiavo, the Mafia was a kind of invisible government or state of mind, a political phenomenon limited mostly to rural areas of western Sicily. For all practical purposes the Mafia disappeared in 1927 when Mussolini forcefully disbanded it or forced it to go underground by bureaucratically placing it in a position of ridicule and reducing its image of invincibility and courage. Crushing blows by the labor union movement and the loss of control over local elections brought an end, according to this author, to a brotherhood of corruption that grew from the Sicilian seeds of a long history of opposition to established government and that emerged under the name of Mafia around 1868. But the functions of the social structure preceded the nomenclature.

We are told that not only has there never been an American Mafia or an international Mafia but also that there has never been a Mafia even in eastern Sicily. The author does not deny that there are crime syndicates in America; he describes their rise out of "America's heritage of violence and corruption." He admits the past existence of a fraternal organization known as Unione Siciliana, one of the many respectable mutual benefit societies, and of the Black Hand, which consisted of small criminal gangs and was born in America, not Sicily, and which lasted about fifteen years until 1910. Neither of these groups had anything to do with the Sicilian Mafia. Nor did Al Capone, Luciano, and the members of the famous Appalachian meeting in 1957.

Schiavo is not writing within a sociological framework of social systems, organization, or theory. He has his own obvious defensive perspective and vitriolic writing style. But there is sociology in his book and there are sociologists as references. Persons who have written about the alleged Mafia in this country are clearly identified and are placed among those who know and those who do not know the facts. For example, Mortimer and Lait, Frederick Sondern, Danilo Dolci, Ed Reid, and Harry Anslinger are false prophets or were deluded into believing there was an American Mafia; and they are irresponsible proponents of their ideas. They are the "bad guys." The "good guys" who speak the truth are represented by Turkus and Feder, Daniel Bell, and Virgil Peterson. The footnotes are very informative and should not be missed. The Bibliography on the history and allegations about the Mafia is excellent.

The book is obviously written by a well-informed student of Italy and the Italians in America. With only trivial errors he writes with knowledge of related criminal data as well. Anyone interested in these topics will certainly want to read a book that declares it is telling the truth about the Mafia.

MARVIN E. WOLFGANG

University of Pennsylvania

The Northern Ireland Problem: A Study in Group Relations. By DENIS P. BARRITT and CHARLES F. CARTER. New York: Oxford University Press, 1962. Pp. 163. 25*s*.

Frank O'Connor, from the secure impartiality of agnosticism, once commented that, "In the South of Ireland, you have the best Catholics in the world and in the North of Ireland, you have the best Protestants. Of course there's not a Christian in the whole bloodly lot." Carter and Barritt attempt to understand what are the social effects of 300 years of religious and ethnic strife and what opportunities there are for attempting to begin a new and somewhat more constructive chapter in the history of Protestant-Catholic relations in the north of Ireland.

While the book is hardly a rigorously scientific analysis of the exceedingly complex Northern Ireland phenomenon, it represents a good beginning to fuller study of one of the oldest conflict situations in the Western World. Sociologists will be interested in noting how religion has become more of a symbol of the differences between two peoples than a system of belief and practice. They will also be intrigued by the accommodations that are necessary when fully one-third of a society does not accept the legitimacy of the society's government yet is not prepared to engage in direct

activity to change the government. (Political parties that deal with problems other than the religious division are, for example, a luxury neither side can afford.)

It seems to the present reviewer, whose ancestral prejudices must certainly place him on the side of the Irish Nationalists, that the two Unionist authors have been more scrupulously fair than anyone could have reasonably expected. Only in their defense of the practice of arrest and detention without trial do they seem to engage in special pleading. However, their hope for an improvement of the situation does not seem well founded. Despite progress made at the university and in the unions, anything more than an informal *modus vivendi* would involve the Nationalists (Catholics) conceding the legitimacy of the Unionist (Protestant) rule. Much history—and not all of it remote—will have to be forgotten on both sides before this can happen. In the meantime, this outpost of the "free world" will still see newspaper ads that read, "Cook wanted: Protestant only (Christian preferred)" and will hear children maintain that the children next door are "not neighbors but Protestants." Ecumenism's final battle will surely be on the plains of Ulster and in the streets of Belfast.

ANDREW M. GREELEY

National Opinion Research Center
University of Chicago

Almost White: A Study of Certain Racial Hybrids in the Eastern United States. By BREWTON BERRY. New York: Macmillan & Co., 1963. Pp. viii+212. $5.95.

Living in isolated settlements along the eastern seaboard of the United States are thousands of people who are part Negro, part Indian, and part white. Their origins are obscure (although tales of their beginnings are legion) and they are known in different areas by many different names—Brass Ankles, Melungeons, Red Bones, Lumbees, and dozens of others. They share in common an assiduous denial of Negro ancestry and an obsessive wish to be accepted as whites. Beyond this their status varies with the mores of the communities in which they happen to live and with the whims of those whose job it is to classify people socially. At the local level, particularly in the Deep South, the fact that they are "neither white, nor black, nor red, nor brown" poses a real problem for those with whom they interact; one often doesn't know "whether to 'Mr.' them or not." They are marginal men.

Almost White is their story. It is the product of twenty years of study during which time author Brewton Berry corresponded with hundreds of persons who shared his interest and talked with thousands of whites, Negroes, and "mestizos" (his generic label for this social category). The book is narratively written, and the author draws freely on a seemingly endless lode of anecdotes he has collected over the years. For those who know little of the mestizos of the eastern United States, *Almost White* is a potpourri of fascinating information.

Yet despite its easy style, its factual content, and its excellent bibliography, there is a lack of conceptual clarity. Most noteworthy is the absence of clear distinction between the problems of self-identity and of social status.

For example, after discussing the widespread phenomenon of miscegenation, Berry says, "Most of us, however, have the good fortune to be identified with, and accepted by, one of the major racial groups. Not so with these 'marginal' folk." The implication of this statement is that it is worse to be in the no man's land of the mestizos ("neither fish nor fowl") than in the fixed position of Negroes.

At several points this hypothesis is supported by evidence that indicates the high degree of anxiety felt by mestizos who are constantly trying to be what others say they are not, in contrast to both Negroes and whites who know *who* they are and live accordingly.

Throughout most of his book, however, Berry counters this Stonequist-type argument by demonstrating that, irrespective of their identity problem, the *status* of the mestizos is invariably better than that of Negroes. Most of the mixed bloods have not only succeeded in removing "the taint of the tar brush" but "have doggedly asserted their claim to being white, and have simply redoubled their efforts to achieve that status." And, says Berry, "believe it or not, they have had a great deal of success."

Many Negroes would surely envy the advantages accruing to those marginal people who frequently call themselves "Indians" and are "almost white."

PETER I. ROSE

Smith College

The Scotch-Irish. A Social History. By James G. Leyburn. Chapel Hill: University of North Carolina Press, 1962. Pp. xx+377. $7.00.

The subjects of Leyburn's study are interesting because of the unusual pattern of their migration. They made two significant moves in a relatively short period, and they thus present the scholar with an opportunity to examine a particular culture in three different environments.

Before 1600, the lowland Scots occupied the rough borderland north of the River Tweed where perpetual involvement in raids across the English frontier prevented them from raising their standard of life above that of brutish and savage existence. In the century that followed, they drifted in substantial numbers across to Ulster County in Ireland where they established a Presbyterian enclave that still maintains its distinctive character. In the eighteenth century many of the Ulster Scots sought opportunity across the Atlantic, and in America they helped to occupy a frontier that reached down the great valleys from Pennsylvania to South Carolina. In each setting they maintained distinctive cultural characteristics.

It is the virtue of Leyburn's volume to have seen the movement as a whole. His discussion focuses on each of the three environments within which these people lived. His material is drawn largely from secondary sources, but he has succeeded in putting together a useful analysis of the experiences of the Scotch-Irish. He has avoided the most obvious pitfalls. There is no easy ascription of national characteristics to these people, nor is there an acceptance of the myths that have been woven about them. The account is generally sensible and well balanced.

Yet for all its merits, the book is not wholly satisfying. It does not come to grips with the significant general problems on which it touches. Estimating the influence of frontier life, for example, Leyburn concludes that the Scotch-Irish "never felt themselves to be a 'minority group,'" that "anomie was an experience unknown" to them and that they "needed no Immigrant Aid society to tide them over a period of maladjustment so that they might become assimilated in the American melting pot." These propositions dispose too readily of significant problems. If the Scotch-Irish were not a minority group, why did many of them persistently attempt to preserve their identity on to the fourth generation? If they felt no sense of anomie, how does one account for the personal and social disorganization of back-country life in the areas in which they settled? If they were so readily assimilated, why did their communities often become apathetic and stagnant? Such questions remain yet to be answered.

Oscar Handlin

Harvard University

The Human Factor in Changing Africa. By Melville J. Herskovits. New York: Alfred A. Knopf, Inc., 1962. Pp. 500. $6.95.

The anthropological tradition has bequeathed two main orientations to modern social science. One is the holistic approach, the desire to comprehend all the aspects of a given social structure in their interrelations. The other is the predisposition to view alien cultures with a sympathetic and appreciative eye, a willingness to discover the rational meaning behind strange behavior.

The critics of anthropology have charged that these virtues brought in their wake some unhappy consequences. One was presumably an antihistorical bias. If one looked, so to speak, vertically at *all* aspects of a given culture, it was perhaps too much to ask that at the same time, one trace each of these aspects through time. The second vice was even worse, for it made virtue of the first vice. It was the tendency to romanticize the past, the "static" past, to oppose change. Yet these criticisms, if they once had some merit, have long since been belied by the large number of anthropologists who accept with a whole heart both history and change, while still serving as the tribunes of cultural heterogeneity.

If proof were needed, it may be found in this final work by the late Professor Herskovits, who has written himself an admirable epitaph. Herskovits was in many ways the first of America's "Africanists." The very act of entering some thirty-five years ago this *chasse gardée* (not only politically but also intellectually) of the European colonial powers was an act of political courage, the kind he continued to show up to his recent death. He may have started more than he would have wished, for the United States today boasts more Africanists than any other country.

This book is neither a monograph nor a

survey. It is an attempt to look at modern Africa with the eyes of a scientist and a humanist, two categories that Herskovits would probably have considered identical. Starting with the base lines of change, Herskovits proceeds to give us a picture of the major outlines of "continuity and change" in the land system, the religious, educational, and occupational arenas. Nor does he neglect the development of modern nationalism and new ideologies. And he ends with two chapters on "rediscovery and integration."

He abhors what had been the pervasive "tendency to write off aboriginal beliefs, moral codes and other regulatory devices," but he also inveighs against "the fallacies in the assumptions of the static society, the custom-bound automatons that presumably made up its membership, and the supposed fragility of its traditions in the face of outside pressures." He believes that Africans "are quite ready to accept change when it is demonstrated that this is to their advantage" and urges that the concept of "change be carefully dissociated from demoralization."

If this sounds intelligent and sensible, it is because it reflects the book, which gives a sober and sensitive evaluation of African hopes to change but also to preserve their society.

IMMANUEL WALLERSTEIN

Columbia University

Cycles of Conquest: The Impact of Spain, Mexico and the United States on the Indians of the Southwest, 1533–1960. By EDWARD H. SPICER. Tucson: University of Arizona Press, 1962. Pp. xii+609. $12.50.

Here for the first time is a full account of the impact of Western civilization on the Indians of northwestern Mexico and the southwestern United States. Spicer utilizes the work of historians to provide a framework within which to view not only the events of contact but also the programs for civilization that were developed by Spain, Mexico, and the United States. But his main focus is upon the Indian groups in the region and their reactions to the various plans to change their lives in the directions their conquerors deemed desirable.

The Spaniards inaugurated a program for civilization that was guided by a religious orientation. They attempted to create a type of community—the Mission community—in which the missionaries could bring about the desired types of changes that would lead to eventual assimilation. The Mexican program discarded the religious orientation of the Spaniards and emphasized the political incorporation of the Indians, on the assumption that they were already Hispanicized. But this plan met considerable resistance on the part of the Indians, and the most important factor in bringing about change was the steady infiltration of settlers. In the American Southwest there was no attempt at political integration until relatively late. The reservation system was under the control of the federal government, and education was used as the major means to cultural assimilation.

Spicer finds that after 400 years of contact, despite all the influences brought to bear, most of the conquered peoples still retain their own sense of identity. The forces for assimilation were balanced by resistance to submergence in the dominant societies. Spicer asks the question: "Why did Indians not progressively reorganize their communities in such a way as to participate in the higher level of political and economic organizations made available to them and, parallel with such change, adopt the European type of world view?" One answer, he believes, lies in the fact that Indian land, labor, and resources were never brought into "a fully functioning role in the developing market economy." But he also notes that we need to take another factor into account: social structure. Where there was greatest continuity of social institutions and organization there was least change in the direction of the dominant cultures.

This brief summary does not do justice to the detailed accounts of the processes of social and cultural change among the Yaquis, Mayos, Tarahumaras, Pimas, and Seris of northwestern Mexico and the Navahos, Apaches, Yumans, Papagos, and Pueblos of our own Southwest. The new interpretations of social and cultural change recently presented by Spicer in *Perspectives in American Indian Culture Change* (Chicago: University of Chicago Press, 1961) are here broadened and further exemplified. The historian will find a demonstration of the significance of the cultural dimension in the interpretation of events. The administrator of overseas programs will find case studies relevant to the problems he is engaged in. And the sociologist should be pleased with the em-

phasis on the social system and its role in continuity and change.

The volume is written in non-technical language and is well illustrated with maps and drawings. Detailed bibliographic notes are given for each chapter and there is an excellent index. Both the author and the University of Arizona Press are to be congratulated on a job well done.

FRED EGGAN

University of Chicago

Entrepreneurship in Argentine Culture: Torcuato di Tella and S.I.A.M. By THOMAS C. COCHRAN and RUBEN E. REINA with SUE NUTTAL. Philadelphia: University of Pennsylvania Press, 1962. Pp. xii+338. $7.50.

As an account of an extremely successful transfer of technical aid from Italy to Argentina—a continuing aid which enabled Argentina to produce a wide variety of complex machinery—this book is of special interest. There are also here brief accounts of successful technical missions from Argentina to Chile, Uruguay, and Brazil. Torcuato di Tella, by organizing such missions, became a very rich and powerful man. This biography of him and his company can be read either as entrepreneurial history or as a history of successful cultural transmission.

The most fundamental factor in this technical aid mission seems to have been career commitment to the country aided and to the enterprises and projects aided. Such commitment increases the probability of success for several reasons. First, when failure of the enterprise is failure of a man, he is much more likely to shoulder the responsibility of finding the source of failure, whether or not it lies within his defined "technical" role. Second, he becomes intimately acquainted with the culture of the receiving country, especially with those crucial parts that affect large-scale enterprise intimately: law and politics, labor relations, sources of trained talent, size and characteristics of the market, the manners of the upper classes, the types of relations of trust and confidence provided by the culture and the conditions that have to be met to enter them for business purposes, the amount of accomplishment considered a reasonable day's work, sources of material supply and their reliability, etc. Third, he has time to adapt technical systems to such local requirements, to learn about technical systems of other countries that would be appropriate for transfer to the enterprise, and to experiment on a small scale before making a heavy commitment. Fourth, he has time to build a team of experts and executives competent to deal with the range of problems presented, committed to the enterprise, and capable of arriving at firm, more or less unanimous, decisions. Fifth, as a consequence of the foregoing, he is able to create an administrative tradition of technical and managerial competence that will continue after he dies or retires. All of these accomplishments are extremely difficult with one- to three-year commitments as technical consultants. Argentina has paid a good deal to di Tella for his type of technical aid. But unlike some cheaper aid programs, it worked.

This suggests that, in general, United States technical aid might better be channeled through technicians from countries that might supply more career personnel than through American nationals who generally refuse to emigrate. Career commitment to foreign countries is probably easier to obtain from technicians who do not have to leave such a rich country to start with.

Another interesting facet of this biography is its account of the growth and dynamics of the inner circle, the "court" that surrounded the "king." The authors might have improved their work by making the analogy with the rapidly growing literature on royal administration in various historical periods. The formal structure of the enterprise was governed by an interpersonal milieu structured by relations of trust, confidence in the competence of others, and various kinds of intimate ties. Kinship and ethnicity, competence, friendships and dependency relations growing from the problems and rewards of work, and career commitments to the enterprise, formed fascinating combinations which changed as the enterprise grew. More analysis of the incidents of tension between these forces, such as the case of the oldest son who refused to take on a career commitment to the enterprise (he became a sociologist), would have illuminated the nature of the "court culture."

The authors are apparently inclined to believe everything they were told by the managerial group. From the internal evidence, this seems to distort the account of labor relations particularly. It may or may not have been

true that Perón was responsible for most of the major labor relations difficulties of the enterprise. The casual mention of mass firings as a labor relations technique indicates that the alienation of the workers might not be entirely due to Eva Perón's agitation.

ARTHUR L. STINCHCOMBE

Johns Hopkins University

Social Control in an African Society. By PHILIP H. GULLIVER. Boston: Boston University Press, 1963. Pp. 306. $6.50.

Philip Gulliver presents a lucidly written and closely illustrated account of the modes of dispute settlement of the sedentary agricultural Arusha who inhabit the fertile and now densely populated slopes of Mount Meru in northern Tanganyika.

The book's first half is devoted to the statics of dispute settlement. It contains a description of the three institutions through which disputes are heard; these are age, grade, and residence; patrilineage; and the recently imposed system of Colonial Courts. Disputes are handled in the two indigenous systems through "spokesmen" elected from among their peers. These men wield no authority but work through "political" processes to bring about mutually acceptable agreements, primarily in an effort to maintain social cohesion even at the expense of justice.

The system of courts pays close attention to tribal law but concentrates on its *letter* in an attempt to seek justice through authoritative decision by a third party. Despite their great differences, Gulliver shows that there are formal parallels among these three systems in that each provides mechanisms for settlement "out of court," in private conclave as well as in formal public assemblies.

The book's second half is devoted to the dynamics of dispute settlement. Although there are many complexities in the way the three systems are used, Gulliver presents seven principles which together express the normal flow of litigation. No account is taken of instances in which indigenous and imposed laws are in marked conflict, such as the case of murder.

The study is based on two years of field work among the Arusha. Not only are the "ideal" kin and age-grade systems shown but the degree of empirical fit is numerically documented. Unfortunately, however, there are no data on the frequency of disputes. Gulliver comments that he was in the field too short a time to generalize but, presumably, Colonial Court records would have been available to him as government anthropologist. Gulliver notes the narrow time span of the "ethnographic present" but does not take advantage of this fact to relate rates and kinds of disputes to the tensions in a rapidly evolving society.

Gulliver's use of the term "social control" is quite limited. He deals neither with internalization nor with the consequences of disputes other than those consequences flowing directly from the agreed settlement. But perhaps the most serious defect of the monograph is the ethnographer's emphasis on description at the expense of attention to theoretical issues raised in the study of other societies. Many comparisons and contrasts might have been suggested by references to a work such as Malinowski's *Crime and Custom in Savage Society*.

RICHARD A. PETERSON

University of Wisconsin

Ego and Milieu: Theory and Practice of Environmental Therapy. By JOHN and ELAINE CUMMING. New York: Atherton Press, 1962. Pp. xv+292. $7.50.

The Cummings have attempted to develop in this book a theoretical synthesis that embraces concepts of ego functioning and of the interpersonal and social environments in which individuals act. The authors use their theoretical propositions as a framework for justifying proposals and prescriptions they make about how to do milieu therapy. The theory is formulated in terms too general to demonstrate how specific hypotheses can be derived for empirical research on the relation between treatment environments and the character of mental illness, but then the book is conceived primarily as a guide to practitioners rather than to researchers.

In Part I, the authors draw upon the work of Hartmann, Erikson, C. H. Mead, Parsons, Lewin, and Federn to formulate a series of

concepts about ego organization, ego damage, and ego growth and to articulate these with the general characteristics of mental patients as described by conventional psychiatric categories.

Part II presents specifications for the ideal therapeutic milieu, constructed, as the authors state, from sociological theory and their own practical experience. These specifications for the practice of milieu therapy are presented in the context of a description of the structure of the milieu, which includes the physical setting of the hospital, the structure of authority and control, roles and role relationships among staff and between staff and patients, cultural beliefs and norms of staff and patients, and processes of interpersonal communication.

Part III is concerned with actual problems involved in the treatment of acute illness, in work programs as they are related to chronicity and rehabilitation, and in the provision of support and control in natural and structured milieus in the community after hospitalization.

The Cummings have written a how-to-do-it book which is considerably more sophisticated than others of its genre thus far available. They provide a logical basis for most of their therapeutic recommendations, place strategic emphasis on the acquisition of instrumental and social skills by former patients if they are to be rehabilitated, and stress the need for manipulation of posthospital settings if the benefits of milieu therapy are to be effective. Their prescriptions often verge on the pontifical ("should" and "must" are used frequently), but they specifically disclaim curative efficacy for their prescriptions in milieu therapy. Their more modest goal is to provide the patient with instrumental and social skills "sufficient to render his basic temperament less painful to himself and to others." Moreover, they are aware that the efficacy of milieu therapy is as "unproven" as that of psychotherapy, and acknowledge the need for undertaking evaluation studies which will require the development of standard criteria for measurement.

Ozzie G. Simmons

University of Colorado

Delinquents, Their Families, and the Community. By C. Downing Tait, Jr., M.D., and Emory F. Hodges, Jr., M.D. Springfield, Ill.: Charles C Thomas, 1962. Pp. xvii+199. $6.75.

The authors report the very interesting failure of an attempt to prevent juvenile delinquency. Half the families of problem children referred by teachers in two elementary schools located in a deprived area of Washington, D.C., received an experimental treatment consisting mainly of social casework; the other half did not. After four years the delinquency rates of the two groups of children were compared and found to be the same. The reader's first reaction is that this finding is not surprising, considering the modest amount of treatment dispensed—an average of only four hours to each child and eleven hours to an adult in each family. However, the authors contend that this small amount of casework reflects the lack of receptivity of the families, who often regarded the casework as an intrusion into their privacy. Since the authors' action proposals depend heavily on the impossibility of helping such families with conventional techniques, it would have been valuable to have had this resistance and non-cooperation documented more thoroughly, even though most readers with experience in this area would not doubt the report.

A review of past studies shows similar treatment programs to have had, at best, only limited success. Therefore, the authors propose that therapeutic subcommunities be established, wherein the entire family would be the patient. Lest this appear utopian, they indicate that such projects have been under way for some time in Holland. Serious consideration is given to methods of bringing families into such communities while at the same time safeguarding their rights.

Clearly, this is an intelligent and realistic proposal. It reflects growing recognition of the family as the primary locus of the causes of delinquency, of the persistence of these causes over generations within a small percentage of hard-core families, and of the economic absurdity of piecemeal efforts aimed mainly at relieving isolated symptoms. A rigorous effort to determine the relative cost effectiveness of the various techniques for handling delinquency, taking into account their generational transmissibility, would contribute greatly to our ability to evaluate proposals such as this, in that it would establish the level of effi-

ciency a new technique must realistically attain in order to represent an improvement.

The authors' somewhat uncritical enthusiasm for the Gluecks' prediction tables (see the review by O. D. Duncan, *American Journal of Sociology*, LXV [March, 1960], 537–39) fortunately does not detract from their important argument.

ROBERT A. GORDON

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The Churches and Mental Health. By RICHARD V. MCCANN. New York: Basic Books, 1962. Pp. 278. $6.00.

This volume in the Mental Illness and Health Series gives a summary of the use of mental health methods in the pastorate along with several empirical studies of the pastoral role as perceived by patients, communities, pastors, and psychiatrists. The development of psychological technique in pastoral work has been treated extensively in several journals, notably *Pastoral Psychology* and the *Journal of Pastoral Care;* hence, the significant aspects for this review are the empirical findings reported.

One empirical investigation examines the reactions of mental patients to the work of chaplains; it provides a rather ambiguous picture. Half the patients felt that they received no help from the chaplains, and only 2.5 per cent saw the chaplain as an important resource in getting well. Although these are interesting clues for further research, in themselves they convey very little; they suggest that contacts between chaplains and patients deserve careful investigation—occasions, amounts of time spent, kinds of interchange, etc. Patient reactions without such observational data are rather meaningless.

In summary, findings of the empirical studies were that: there is ambivalence among the clergy about the relevance of religious resources in mental illness and uneasiness about psychiatric approaches; twenty-four psychiatrists indicated positive feelings for a clergyman's role in mental health where he exercised his traditional functions; samples of lower- and middle-class communities indicate remoteness from psychiatric perspectives and a tendency to feel that they should solve their own problems.

These findings provide many clues for further research but little that is representative or adequate enough for significant generalization. The pay-dirt of pastoral work lies in its operational effects and we know practically nothing in a scientific way about such effects in coping with mental illness. Until studies are done on a representative basis, we shall continue to know rather little about the churches and mental illness.

GIBSON WINTER

University of Chicago

Application of Methods of Evaluation. By HERBERT H. HYMAN, CHARLES R. WRIGHT, and TERENCE K. HOPKINS. ("University of California Publications in Culture and Society," Vol. VII.) Berkeley: University of California Press, 1962. Pp. xiii+396. $6.50.

This book is a detailed report on an extensive research evaluation of the Encampment for Citizenship, a residential summer program for high-school and college students whose aim is to "increase skills in democratic living and inculcate values apposite to an ideal world."

Despite the fact that, as is typical in evaluation studies, the self-selected campers start from rather favorable positions on the criterion scales, the authors show statistically significant favorable changes, both absolutely and in comparison with (*a*) changes in a subsample of campers measured six weeks prior to the opening of camp and on opening day, and (*b*) a sample of college students reasonably comparable to the campers and measured in the spring and fall. Follow-up waves suggest that the changes were maintained six weeks after the close of camp, although backsliding began to appear in the four-year followup. Of the many scales used, the strongest effects appeared in favorable ("liberal") changes on opinions regarding race relations and civil liberties.

No convincing correlations were turned up regarding intrasample differentials in change, although the authors draw the inference that reference-group processes are the key mechanism. Similarly, considerable attention is given to the idea that harsh environments (e.g., Negro campers returning to the South) are associated with post-camp erosion of the effects. Since the strongest evidence for the

latter hypothesis comes from extensive tabulations on eight American Indians (the fourteen Negroes show little difference from whites), the evidence is not sufficient to validate the hypothesis.

Such results would not ordinarily require 392 pages of text for their presentation, but book length is achieved by endless repetition and by presenting the materials as a case study in evaluation research. The latter amounts to an extremely detailed presentation of possible methodological and technical booby traps which surround each aspect of the research, and the student who reads this book as a case study will find almost every conceivable issue in evaluation research raised and stated in a clear and simple fashion. The student should be warned, however, that all doubts are resolved "in favor" of the study and that the decision process for the resolutions would not always be replicated by another set of research workers.

Thus, after carefully stating the issues involved in using small samples, the authors then proceed to present extensive tabulations on eight Indians, sixteen Negroes, and fourteen southerners (an unknown number of whom are Negroes). Although the findings on these minuscule groups are considered "tentative" on page 219, by page 287 they have become hard enough to have to be reconciled with equally unreliable results from a subsequent wave. Many a researcher would have resolved the issue by not tabulating such small samples to begin with. Similarly, the authors correctly argue that they must show that friends were chosen democratically before concluding that democratic friendships were part of the camp experience. However, their table (p. 156) shows 18 and 25 per cent differences in race of friends by race of camper, differences interpreted as "close to the expected values if race were not a desideratum," in interesting contrast to the description of similar percentage differences in the effect analysis as "dramatic" (p. 142). Or again, after a straightforward discussion of replications, the authors note that they got the same results in 1955 and 1957 and that "if we had merely been fortumate that one rare time in 1955, luck would not have broken that way again" (p. 46), a correct statement of the issue but a rather idiosyncratic calculation of the probabilities involved.

JAMES A. DAVIS

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American Cities in Perspective: With Special Reference to the Development of Their Fringe Areas. By G. A. WISSINK. Assen, The Netherlands: Royal Vangorcum, Ltd., 1962. (Distributed in the United States by The Humanities Press, New York.) Pp. iii +320.

As the subtitle indicates, the primary emphasis of this book is on the densely settled areas outside the central cities, that is, on the territory roughly defined as the "urban fringe." Initially the concept of "city" is examined. Then, following ecological theory, the functional interdependence of city and fringe and the effect of this relation on the changes in the internal structure of the city are indicated. Unfortunately, the consequences for urban structure of the position of particular cities in a field of cities or of the regional connections among cities receive only barest mention.

The major part of the book describes the fringe and its significance in the total urban agglomeration. The author examines the historical development of these varied areas, their functions, their internal differentiation and their social and cultural characteristics. He attempts in conclusion to identify and explain some of the factors, both general and local, that account for the development of fringe areas in their present form. The study is principally of American cities with limited comparative materials mostly from London, Paris, and two or three other European centers.

This volume is one of a series of Social Geographic Studies, and these terms describe the basic orientation. The city is viewed as fundamentally a geographical phenomenon. Still, the structure of cities and of their fringes is seen as closely dependent on the national cultural system. New urban development is held to be mainly a fringe occurrence. And here "human values are more or less 'dramatically' at play" (p. 57) to effect a community life that reflects the conscious

and subconscious objectives of a people. Fringe development is thus seen as partly, but most importantly, the reflection of "the search for better living conditions" (p. 77). In the process the growth and new elements of the city are held to center in the suburban areas.

The idea behind this book is a good one. Existing research studies on fringe areas are scattered, based upon variable definitions of the units to be treated, and to some extent contradictory in their findings. A review of this work, supplemented by new and intensive research and leading to a critical evaluation of the concept of "fringe area" and of the current knowledge about actual fringe areas, would be a valuable contribution to comparative urban sociology.

It is disappointing, but not entirely the fault of the author, that this study falls short of such an attainment. Clearly, there is not yet a sound basis of research studies for a comparative evaluation. Nevertheless, unfulfilled hopes are raised by the statement that this study is based on field work in six cities in the United States and on "additional investigations" in Paris, London and a few Dutch cities, as well as on the "literature concerning cities in several countries"; there is no subsequent indication of original research beyond the unearthing of a few local-area studies completed by others. Further, the studies of other scholars that are presented do not for the most part receive the critical assessment that this eclectic approach requires. The recent study of the megalopolis by Jean Gottman appears not to have been available to the author and its notion of an area of mixed urban and rural land uses covering a broad territory receives only incidental attention on the descriptive level. The last major section of the book, which attempts to explain why the fringe assumes its specific form in particular areas, is superficial and tends to concentrate on simplified statements about the effects of such familiar phenomena as the automobile or the desire for open spaces. Ideas and research findings are not always clearly distinguishable.

In sum, this study presents little that is new to the urban sociologist but has the merit of bringing together a variety of scattered materials on the urban fringe. The points covered and the principal conclusions can be satisfactorily gauged by reading the brief section which presents conclusions and implications. There is an appendix on U. S. Census Bureau definitions of Urbanized Areas and Standard Metropolitan Areas and a useful bibliography but no index.

VINCENT HEATH WHITNEY

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Population and Society: Introduction to Social Morphology. By MAURICE HALBWACHS. Translated by O. DUDLEY DUNCAN and HAROLD W. PFAUTZ. Glencoe, Ill.: Free Press, 1960. Pp. 207.

This is a translation of a book published in French in 1938, under the title (more fitting than that selected) of "Morphologie Sociale." To supplement the author's original text, the translators have provided a preface, two technical notes, and a brief list of references in English.

The book proper is less than sixty thousand words in length. Its contents are divided into two parts, the first dealing with the morphology of groups and activities internal to a society and the second treating the morphology of total populations. In the former the author does little more than exemplify an approach toward research into religious, political, and economic structures. His general proposition is that these institutional complexes have demographic foundations and characteristics and that inquiries into them can profit from the truism that the functioning of such institutions is necessarily assured by the existence of groups of men in motion on the earth. Halbwachs asserts that this "nature of things" is most evident and influential in the economic area, less so in the political area, and least persuasive in the religious area, where beliefs are less responsive to the sanctions of physical experience.

The second part of the book, making up the bulk of the monograph, is an elementary, incomplete, and outdated presentation of the contents of a standard undergraduate course in population problems. The style is frequently rhetorical, and the allocation of space to particular topics is old-fashioned. Errors in fact and logic are frequent enough to be annoying.

If there is a justification for this publica-

tion today, it lies in the presentation of a research viewpoint which deserves more than the handful of followers it now has. Halbwachs presents population as the most general framework within which all social facts must be placed. Population as a mass has the two essential properties of being juxtaposed in space and persistent through time (in contrast to its transitory components). Demographic change is a necessary but insufficient determinant of social behavior, restraining some types of development and inducing (but not compelling) others. Although social understanding cannot be reduced to a set of determinisms, it is equally true that no social structure or function is disembodied. The great merit of the viewpoint, then, is the persistent emphasis on the interdependencies of the normative and the physical morphology.

Maurice Halbwachs (1877–1945) was the greatest sociologist of his generation in France. He was one of the very few who have tried to bring together sociology and demography, in the tradition of his predecessor and mentor, Émile Durkheim, for whom social morphology represented the most immediately accessible type of phenomenon for sociological research, preliminary to and underlying the investigation of the normative structure environing the individual. Regrettably, the links that Halbwachs attempted to forge between the demographic and the normative aspects of society turned out to be mostly metaphors in collective psychology or anthropomorphic allusions to the instincts of the population, which might be testable by faith but certainly not by research.

The translators' Preface is a mild polemic on behalf of the directions in which Halbwachs might have developed his work. Sometimes Halbwachs disappears from sight somewhere between the much more substantial figure of Durkheim and the much more immediate present of modern human ecology. In particular there is a disjunctve shift into an examination of the work of Charles Booth in relation to contemporary urban studies, which might deserve publication in a less inappropriate context.

In the reviewer's judgment the translation of this book represented a misallocation of scarce resources. The book is clearly not a classic; indeed, its contents are here and there embarrassing, as happens with most merely good works in the course of time. The cogency of the message for current research strategies will prove compelling only to those who are already convinced. A modern treatise on social morphology is a worthy task. Perhaps the translators will write their own book next time.

NORMAN B. RYDER
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The Technological Order: The Proceedings of the "Encyclopedia Britannica" Conference. Edited by KARL STOVER. Detroit: Wayne State University Press, 1963. Pp. 280. $6.95.

When Diderot, together with a group of eminent French scholars, compiled the first great encyclopedia in 1751, it was in an effort to codify the rapidly expanding fund of knowledge. One of the prime sources of this expanding fund of knowledge was the rapid development of material and scientific technology. It is quite apropos, then that the editors of the *Encyclopedia Britannica*, two hundred years later, should call together a group of savants to talk about the nature of technology and its continuing impact on society.

Unfortunately, the group assembled for this purpose included only one technologist, Sir Robert Watson-Watt, the "father of radar," and no professional students of technology or its implications (biologists, geographers, sociologists, or psychologists). In his keynote address, Sir Robert pointed to the need for socially responsible direction of technological development. His only specific proposal, however, was for a "Hippocratic oath for the technologists" (p. 5).

The article following, by Bordeaux professor Jacques Ellul, points to *"la technique"* as a self-determining system in which man is increasingly dominated by the tools he has created, so that "the further technological progress advances, the more the social problem of mastering this progress becomes one of an ethical and spiritual kind" (p. 24). The next paper, by W. Norris Clarke, S.J., is interesting in its exposition of a "Protestant" ethic based upon biblical and papal texts. As Clarke moves from Genesis to Pope John XXIII, A. Zvorikine, who follows, moves from Marx through Lenin to Khrushchev. His paper begins with the promising postulate that the implications of technology are a

function of the socioeconomic context in which it develops. Unfortunately, the paper degenerates into an unqualified eulogy of Soviet Russia. These three papers comprise the section entitled "Ideas of Technology."

The section on "The Technological Act," which follows, includes papers by Lynn White, Jr., and A. Rupert Hill. The former traces the complicated history of several inventions to conclude that "'creativity' may well be a lot of things and not just one thing" (p. 113). The latter suggests that the historically recent rapid rate of technological advance can be linked to the deliberate quest for innovation through science and the application of science to mass production.

In the ensuing section on "Nature, Science and Technology," Scott Buchanan and Willy Hartner lament the loss of human values. The next section, entitled "Technology in Focus—the Emerging Nations," finds Richie Calder arguing that primitive countries need primitive technology in order to advance, while Arthur Goldschmidt and Robert Theobald argue that such nations can bypass change through capital-intensive technology and thus avoid the rigors of the Industrial Revolution. The book concludes with brief, uninspired commentaries by Aldous Huxley, Robert M. Hutchins, and Warren E. Preece.

To judge by the written record, at least, this must have been a dull conference. In the Introduction Stover states: "The modern scientific technology promises to be both the hope of man's future and the instrument of his enslavement or his destruction . . . we must put it in the control of reason. To do so, we must understand what modern technology is, what it means, and what must be done with it if it is to serve man well" (p. xi). While there are a number of interesting tidbits scattered throughout the 280 pages of the book, the conference went wide of the mark in its stated goal of showing how technology can be put in the control of *reason.*

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THE RATE OF TECHNOLOGICAL DIFFUSION AMONG LOCALITY GROUPS[1]

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ABSTRACT

The rate of diffusion for single innovations has been found to vary among communities and other types of sociogeographic areas, but whether an area manifests a characteristic rate of diffusion, and, if so, to what factors it may be related, has not been systematically investigated. Data obtained in personal interviews with farm operators in twelve Kentucky localities in 1950, 1955, and 1960 provide insight into these questions. It is found that the relative pace of diffusion among localities for each of five recommended farm practices is quite similar, indicating the existence of an underlying rate of diffusion for each locality. A measure of relative locality diffusion rate is found to be most closely related to the median educational level of farmers, median level of contact with communication media, and an index for localities of the integration of communication structures. The locality median scale of farming and measures of attitudes toward scientific farming, family visiting, and social participation are less closely related to average diffusion rate.

Much of the sociological research on cultural diffusion has been stimulated by the obvious relative "backwardness," technologically, of one sociogeographic area or locality compared to another. Only a few of the many studies of technological diffusion, however, have dealt directly with the problem of differential area diffusion rates. One of the earliest that did so found that the Pacific Coast region led all others in the United States in the rate of adoption of amateur radio sets.[2] And in a study of the acceptance of hybrid corn seed Ryan found that the speed of acceptance varied among type-of-farming areas in Iowa;[3] in a more recent study similar differences were found by Griliches among U.S.D.A. crop reporting districts and among states in the Midwest and South.[4]

Although it is thus apparent that sociogeographic areas on occasion do differ in the rate of diffusion of new technology, evidence from these studies does not permit us to say how generally this may occur or to determine to what extent, if any, there are typical diffusion rates for areas. Additional studies of locality differences in the technology in use at a particular time[5]

[1] Revision of a paper presented at the American Sociological Association meetings, September, 1962. The research on which this report is based has been conducted under the auspices of the Kentucky Agricultural Experiment Station, and is published by permission of the director.

[2] Raymond V. Bowers, "Differential Intensity of Intra-Societal Diffusion," *American Sociological Review*, III (February, 1938), 21–31.

[3] Bryce Ryan, "A Study of Technological Diffusion," *Rural Sociology*, XIII (September, 1948), 273–85.

[4] Zvi Griliches, "Hybrid Corn: An Exploration in the Economics of Technological Change," *Econometrica*, XXV (October, 1957), 501–22.

[5] See, e.g., Harold A. Pedersen, "Cultural Differences in the Acceptance of Recommended Practices," *Rural Sociology*, XVI (March, 1951), 37–49; E. A. Wilkening, *Acceptance of Improved Farm Practices in Three Coastal Plain Counties* (Techni-

strengthen the supposition that there are differences in typical locality diffusion rates. But this evidence is inconclusive, since the use of an item at any particular time is a function of both the time at which it became available and the rate of its diffusion.[6]

Information pertaining to this problem was obtained in a series of surveys of farmers in twelve Kentucky localities.[7] A complete survey of all farmers in 1950 was followed by a second survey in 1955; in 1960 a 50 per cent sample was surveyed. In 1950 the localities ranged in size from fourteen to seventy farm operators, but by 1960 the range was from about ten to forty-six operators.

The farmers studied are homogeneous ethnically[8] and in dominant type of farming;[9] their farms differ, however, in topography and soil characteristics.[10] To satisfy most of the routine needs of daily life, the farm operators[11] use the same agricultural agencies, markets, services, etc., as a consequence of living in a single trade center area of one county.

Stated most concretely, the research questions with which this paper is concerned are: (1) Do these locality groups of farmers typically differ in rate of diffusion? (2) To what factors are locality group differences in rate (or rates, if a typical rate does not exist) of diffusion most closely related?

THE RATE OF DIFFUSION

Over twenty-five years ago Pemberton discovered that in cases of "completed" diffusion the frequency distribution of adopters, when plotted against time, tended to be bell-shaped and that the distribution of the accumulated proportions of adopters resembled an ogive.[12] Although diffusion occurred more rapidly in some cases than in others, Pemberton made no attempt to assess these differences. In fact, only recently has attention been focused on the problem of measuring the diffusion rate itself.

Two types of diffusion rates have been used in recent work:[13] (*a*) the "average" diffusion rate for the entire diffusion cycle, and (*b*) the diffusion rate at "any" point of the time series. The latter rate, which

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cal Bulletin 98 [Raleigh: North Carolina Agricultural Experiment Station, May, 1952]); C. Paul Marsh and A. Lee Coleman, "The Relation of Neighborhood of Residence to Adoption of Recommended Farm Practices," *Rural Sociology,* XIX (December, 1954), 385–89; A. W. van den Ban, "Locality Group Differences in the Adoption of New Farm Practices," *Rural Sociology,* XXV (September, 1960), 308–20; Norman Hilton, "Technology, Economy, and Society in a Long-settled Area," *Rural Sociology,* XXVI (December, 1961), 381–94.

[6] Griliches, *op. cit.*

[7] In previous reports of these studies the term "neighborhood" has been used to refer to these areas. But to avoid prejudgment of their sociological properties, they are herein referred to simply as "localities" and additional description is provided by data on the social relationship systems in the localities.

[8] Nearly all residents are native-born white Kentuckians.

[9] The dominant type of farming is Burley tobacco and livestock, principally beef and dairy cattle.

[10] The localities vary from areas suited mainly to cultivated crops to areas suited mainly to pasture and occasional cultivation (cf. *Land Areas of Kentucky and Their Potential for Use* [Frankfort: Agricultural and Industrial Development Board of Kentucky with the cooperation of the Soil Conservation Service, U.S.D.A. and the Agricultural Experiment Station, University of Kentucky, 1953]).

[11] It should be noted that for study purposes the population is an occupational "group" distributed in twelve localities rather than all locality residents. However, this distinction is not particularly important to this analysis since the farm operator group is by far the largest single occupational group.

[12] H. Earl Pemberton, "The Curve of Culture Diffusion Rate," *American Sociological Review,* I (August, 1936), 547–56.

[13] Cf. Jarvis M. Babcock, "Adoption of Hybrid Corn: A Comment," *Rural Sociology,* XXVII (September, 1962), 332–38; Griliches, *op. cit.,* and his "Profitability versus Interaction: Another False Dichotomy," *Rural Sociology,* XXVII (September, 1962), 327–30; A. E. Havens and E. M. Rogers, "Rejoinder to Griliches' 'Another False Dichotomy'" *Rural Sociology,* XXVII (September, 1962), 330–32; and Edwin Mansfield, "Technical Change and the Rate of Imitation," *Econometrica,* XXIX (October, 1961), 741–66.

varies throughout the diffusion period, can be determined either directly from the actual data or from the slope of a mathematical function that describes the time series. Regardless of how it is determined, the diffusion rate is usually considered to be the time rate of adoption, expressed as a ratio of the number (or proportion) adopting to the number (or proportion) who might possibly have adopted the innovation.

For many analytical purposes a single rate for the entire diffusion cycle is more useful (where it can be justified) than a variable one. To obtain a constant diffusion rate, Fliegel and Kivlin used the slope of a straight line fitted to the eight-year period of the most rapid diffusion.[14] Doubtless, in many instances a straight line provides a reasonably close fit for the part of the diffusion cycle where "bandwagoning" effects occur, but this rate is hardly satisfactory in dealing with the entire diffusion cycle. A more satisfactory measure is the slope of the logistic fitted to the logarithmic transform of the diffusion curve data.[15] This measure of "average" diffusion rate[16] is satisfactory, however, only in those cases where the logistic can be justified theoretically[17] and it provides a satisfactory description of the diffusion data.

In the present case, since only three points on the diffusion curve of each practice are known—for 1950, 1955, and 1960—a mathematical function for the data cannot be determined. Fortunately, *relative* differences in "average" locality diffusion rates can be determined by directly comparing the diffusion curves for localities, assuming that all localities have a common starting point and approximately the same equilibrium level. For instance, it is easily seen in Figure 1 that the "average" diffusion rate for locality *A* is greater than that for locality *B*, which is greater than that for locality *C*. For many purposes, including the present one, a measure of relative difference in "average" diffusion rate is all that is required.

It is possible, moreover, to introduce a degree of precision in the measurement of the relative "average" diffusion rate (hereinafter referred to as "ADR"). Where the origins and equilibrium levels are essentially the same for all localities, the relative ADR of a locality is quite highly (usually perfectly) related to the relative size of the area under the diffusion curve, bounded by the origin and the time when the survey is made. ADR is also monotonically related to the elapsed time between the diffusion origin and attainment of equilibrium. Thus, either the elapsed time from origin (t_0) to equilibrium ($t_{1,2,3,\ldots,n}$) or the area under the curve ($t_0\, t_{n+1,2,3,\ldots,m}$) provides a satisfactory basis for indicating relative ADR (Fig. 1). The area under the diffusion curve is less sensitive to minor variations in t_0 and $t_{n+1,2,3,\ldots,m}$ and is thus a more useful indicator of the relative ADR where the dates of t_0 and t_n may not be known precisely.

[14] Frederick C. Fliegel and Joseph E. Kivlin, "Farm Practice Attributes and Adoption Rates," *Social Forces*, XL (May, 1962), 365.

[15] Griliches, "Hybrid Corn: An Exploration . . . ," *op. cit.*, pp. 504 ff.; and Mansfield, *op. cit.*

[16] It "is a measure of the slope of the growth curve after adjustment is made for the fact that it is a growth curve, i.e., is S-shaped" (Griliches, "Profitability versus Interaction . . . ," *op. cit.* p. 328, n. 7).

[17] As Pemberton, *op. cit.*, recognized, the use of a particular mathematical function to describe empirical data is justified only if there is a correspondence between the a priori conditions of the mathematical equation and the theoretical conditions of the process being described. Too frequently this dictum escapes attention, e.g., use of the normal curve and normal ogive in diffusion studies is continued despite the fact that Pemberton's justification for its use is no longer satisfactory. Mansfield, *op. cit.*, has developed a strong theoretical justification for the use of the logistic, however, and the theoretical basis for two other mathematical models has been developed by James S. Coleman, Elihu Katz, and Herbert Menzel ("The Diffusion of an Innovation among Physicians," *Sociometry*, XX [December, 1957], 253–70). Everett M. Rogers (*Diffusion of Innovations* [New York: Free Press of Glencoe, 1962], pp. 152–59) attempts to supply a rationale for use of the normal ogive.

Where ADR is determined directly from the mathematical function, or the relative ADR's for localities are obtained by comparing areas under the diffusion curves, the "average" rate is not an arithmetic average of the set of diffusion rates for the time series. In an exact sense, the meaning of ADR depends on the operations by which it is determined. ADR as herein defined, however, bears a similarity to Griliches' *b* in that both relate to the average speed with which equilibrium is attained.[18]

FIG. 1.—Typical diffusion curves

In each survey of the twelve localities, two types of questions were asked: (1) Have you *ever* tried practice *Y* in your farming operations? (2) Have you used practice *Y* in your farming operations during the past two (or, in certain cases, three) years? The questions were asked only of farmers having the type of enterprise to which the practice in each case applied. All practices were recommended by the Kentucky Cooperative Extension Service for general use in the county. The aggregate response of farmers in each locality to the first question indicates the current level or extent of diffusion, while responses to the second question indicate the current level of utilization of the practice. The two are identical only in case diffusion (adoption) implies a "final" commitment to the use of the technique for an extended period of time. While this often occurs for innovations requiring relatively large financial outlays, such is not the case for these practices, and in every case diffusion of the practice exceeds the level of current utilization.[19]

The six recommended farm practices for which the progress of diffusion during the decade can be determined are (1) terracing, which was not promoted in this county until the middle of the 1930's

[18] Griliches, "Hybrid Corn: An Exploration . . . ," *op. cit.*

[19] C. Milton Coughenour, "The Practice Use-Tree and the Adoption, Drop-out, and Non-Adoption of Recommended Farm Practices: A Progress Report" (paper presented at the meetings of the Rural Sociological Society, Ames, Iowa, August, 1961).

owing to the lack of technical personnel to lay out contour lines; (2) soil-testing, for which the necessary laboratory facilities became available about 1938; (3) calfhood vaccination for brucellosis (available in 1938); (4) artificial breeding of dairy stock (available in 1945); (5) bluestone lime to prevent wildfire and angular leaf spot in tobacco (available in 1940); and (6) methyl bromide gas to kill weed seed in tobacco plant beds (available in 1948). These practices are important in the major farming enterprises in the county, although none is crucial in the sense that its omission would of necessity prevent the farmer from making a profit.

Inasmuch as each recommended practice became available to farmers in all localities at the same time and the equilibrium level in each locality is nearly equal,[20] differences in the ADR for each locality will be reflected in differences in the areas under the diffusion curve. For illustrative purposes the diffusion curves for soil testing are presented in Figure 2. It is apparent that the localities differ in vertical displacement of the ten-year segment of the diffusion curve and hence in the area bounded by the curve, the X-axis, and the dates of the initial and final surveys.[21] In nearly every case this situation is repeated for the other practices. For every one, in fact, some localities had attained levels of diffusion by 1950 that were not attained by others until about 1955, or by still others by 1960. Some localities thus lagged as much as ten years (or more) behind others in the pace of diffusion.[22]

By convention, the diffusion curve is described by the cumulative proportions of a population who have adopted an idea, but in the present study it is described by the percentage having the relevant farm enterprise in each survey who reported that they had *ever* used the recommended practice. The diffusion curve determined in the latter way should not differ from the conventional curve if (*a*) the respondents reliably report prior use, (*b*) diffusion is considered to have occurred when an innovation was first used, and (*c*) there is no change in the population. In either case, the diffusion curve may level off short of complete diffusion, but it cannot decline. Yet in locality *C* (Fig. 2) there was an apparent decline in diffusion from 1950 to 1955, and similarly for locality *H* from 1955 to 1960. This phenomenon is not confined to soil-testing. Indeed, it is most prevalent in the 1955–60 period for calfhood vaccination for brucellosis and use of bluestone lime, occurring in eight and seven localities, respectively. Evidently, the foregoing assumptions have not been fulfilled.

Like many rural areas the twelve localities lost population during the decade. Population loss by locality ranged from 8 to 46 per cent, and the assumption of a stable population hardly fits the facts for any locality. Careful analysis (see Appendix) indicates, however, that population change is not the most likely cause of decline in the diffusion curves; instead

[20] The first assumption seems reasonable since behavioral adoption, rather than cognitive identification and psychological acceptance, is the crucial datum and since, as noted earlier, the localities are all in the same county and the needed information services and technical facilities are similarly available to all. There may be some doubt, on the other hand, about the assumption of similar equilibrium levels in view of the wide variation among localities in land-use suitability and scale of farming. However, it should be remembered that diffusion (per cent of adopters) is based on the population having the enterprise to which the practice pertains rather than on that of all farmers. In this respect, the enterprise status of the base population to which the equilibrium level in each case pertains is equal. Moreover, with the possible exception of terracing, none of the practices involves the outlay of large sums of money. These considerations thus strengthen the assumption either that there are no differences in the potential equilibrium levels for each practice or that they are small in relation to differences in ADR.

[21] It matters little in this case whether the area under the diffusion curve is computed from the assumed point of origin (1938) or the initial survey in 1950.

[22] Cf., e.g., *K* and *L* with *A, B, C, D, E,* and *H* in Fig. 2.
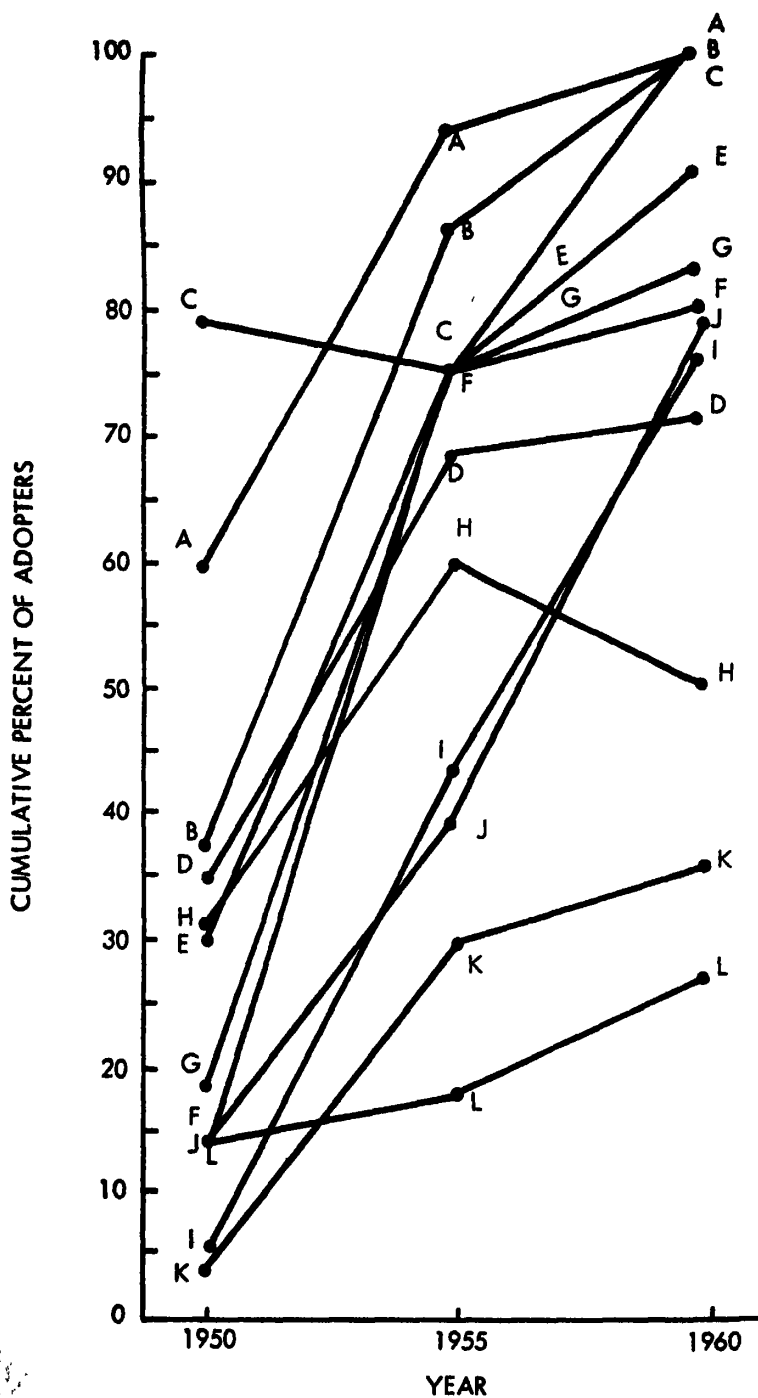
FIG. 2.—The diffusion of soil-testing in twelve localities

it seems due to unreliable reporting of *ever* having used the particular practices in question. While this emphasizes a weakness in the use of recall in obtaining data for the diffusion curve,[23] the crucial issue in the present case is the extent to which relative differences in areas under the diffusion curves have been affected thereby.

Fortunately, although the decline in the diffusion curve does affect the circumscribed area on which the ADR in each case is based, this has only a minimal effect on the relative rank order measure of differences in this respect, only the rank order measure of locality ADR for each practice need be considered. As indicated in Table 1 the localities generally differ in relative ADR. However, in some cases, for example, the use of methyl bromide gas, there are tied ranks indicating that there was no difference in relative ADR. This occurs most often among the high ranks where there was relatively little or no diffusion of the practice during the decade. Even so, the ADR ranks of a locality for different practices are quite similar.

TABLE 1

LOCALITY RANK OF AVERAGE DIFFUSION RATE FOR SIX RECOMMENDED PRACTICES AND THE CHARACTERISTIC AVERAGE DIFFUSION RATE, TWELVE LOCALITIES, 1950–60

Locality	Soil Test	Brucellosis Vaccination	Bluestone Lime	Terraces	Methyl Bromide Gas[a]	Artificial Breeding	Average Diffusion
A. . . .	1	1	1	2	5	2	1
B.	3	2	3	1	10.5	5	2
C	2	7	4	4	1	1	4
D	4	3	7	3	3	3	3
E.	5	8	5	7	4	4	6
F.	7	4	2	5	10.5	11.5	7
G	6	6	6	6	6.5	6	5
H. . . .	8	5	10	8	10.5	7	8
I . . .	9	10	9	11.5	6.5	9	10
J	10	12	8	11.5	10.5	8	9
K.	11	11	11	9.5	2	10	11
L.	12	9	12	9.5	10.5	11.5	12

[a] Omitted in computing *ADR*.

locality ADR for each practice (see the Appendix for this analysis.) It is thus possible to use the original data in the analysis of both the differences among localities in ADR for each practice and the similarities in the relative ADR of a locality for several practices.

Since in itself the computed area under each diffusion curve has little meaning except to show that there are in fact locality differences... This raises a question of the extent to which the ADR's for each practice reflect an underlying *ADR*[24] that is characteristic of each locality.

Intercorrelations of the ADR's (Kendall's τ) range in size from —.05 to .75 (Table 2). Ten of the fifteen are above .48, however, and thus would be significant at the .02 level or below if the localities had been randomly selected. Four of the five remaining coefficients involve a single practice—methyl bromide gas. The low correlations in this case occur because in five localities there was no diffusion of the practice, and ADR is thus

[23] For recent studies of this particular problem see E. M. Rogers and L. E. Rogers, "A Methodological Analysis of Adoption Scales," *Rural Sociology*, XXVI (December, 1961), 325–36, and, N. B. Patel and C. M. Coughenour, "Speed of Initial Use and Persistence in Use of Recommended Farm Practices" (paper presented at the meetings of the Rural Sociological Society, Washington, D.C., August 29, 1962).

[24] Hereafter, italics will be used to distinguish references to the underlying or characteristic *ADR* of a locality from that for an individual practice.

indeterminate. With the omission of methyl bromide gas, the evidence for an underlying *ADR* is thus rather strong considering the crudity of the measurements. The numbers of practices and localities are too small, however, to reach a firm conclusion on the matter.

For the purpose of further analysis, a general index of locality diffusion rate, *ADR*, was computed. It is based on the unweighted average of the areas under the diffusion curves for all practices except use of methyl bromide gas. The relative *ADR* for each locality is shown in the last column of Table 1, and its correlation with the ADR for each practice is in the last column of Table 2, the latter range from .64 to .88 indicating high intercorrelation.

TABLE 2

INTERCORRELATIONS (KENDALL'S τ) OF SIX FARM PRACTICE DIFFUSION RATES AND CHARACTERISTIC AVERAGE DIFFUSION RATE

	Terraces	Brucellosis Vaccination	Bluestone Lime	Methyl Bromide Gas	Artificial Breeding	Average Diffusion Rate
Soil test	.68*	.58*	.67*	.32	.75**	.88**
Terraces	....	.74**	.58*	.10	.48*	.77**
Brucellosis vaccination	....	....	.48*	−.05	.32	.65*
Bluestone lime	....	....	....	.10	.51*	.67*
Methyl bromide gas	....	....	....	....	.52*	. . .
Artificial breeding	....	....	....	....	....	69**

* Significantly different from zero below the 0.02 level.
** Significantly different from zero below the 0.001 level.

These six practices are only a few of the larger number of recommended practices in each survey for which the diffusion and/or utilization was determined. The lists of practices for each survey were not identical, however, and whether the farmer had "ever used" each practice was not always determined. Relative diffusion rates thus can be determined for only these six practices.

In previous analyses, the longer lists of recommended practices were used to develop a mean adoption score for each locality at the time of each survey.[25] While diffusion is taking place, both its extent and the extent of current utilization of the practice at a particular time should be related to the underlying average diffusion rate, if one exists.

To the extent that *ADR* based on the five practices in an estimate of the underlying average diffusion rate for each locality, it should be closely related to the mean locality diffusion and utilization score for each survey. This is indeed the case as indicated by rank correlations of .75, .88, and .62[26] for 1950, 1955, and 1960, respectively. This further bolsters the conclusion that for the farmers in each locality there is a characteristic underlying average rate of diffusion, which is manifested in the diffusion rates for a broad range of technology. Indeed, once one begins thinking in this way, the hypothesis from general sociological theory that localities as social systems differ in the speed of response to the introduction of new ideas is so compelling as to occasion doubt that the contrary might be so.

[25] Cf. C. Paul Marsh and A. Lee Coleman, "The Relation of Neighborhood of Residence to Adoption of Recommended Farm Practices," *Rural Sociology*, XIX (December, 1954), 385–89, and James N. Young, "The Influence of Neighborhood Norms on the Diffusion of Recommended Farm Practices" (unpublished Ph.D. dissertation, University of Kentucky, 1957), chap. v.

[26] The probability that a rank correlation (τ) this large would occur by chance is less than .0025

AVERAGE DIFFUSION RATES AND CHARACTERISTICS OF LOCALITIES

In developing an explanatory model for average diffusion rates, one must keep clearly in mind that the adoption unit in this case is the farmer and that the context of the decision, in the most immediate sense, is the farm enterprise.[27] The diffusion curve for a locality thus describes the aggregate response process of the farmer-enterprise units, which are imbedded in a system of social relationships, to the introduction of a technological innovation. The greater the speed of the social system's response, the greater is the average diffusion rate.

Where the unit of study is a locality consisting of a number of farmer-enterprise units and their interrelationships, previous studies indicate the importance in diffusion of three main types of variables. First, there is the attitude, knowledge, decision-making ability, interpersonal competence, etc., of the farmer and of his enterprise (including scale of farming, capital, and labor resources, etc.).[28] These are quality and performance attributes of the farmer-enterprise units in the locality which may be derived from, but are otherwise largely independent of, the social relationship systems. Second, there are the characteristics of the structure of social relationships in which farmer-enterprise units are imbedded. On the one hand, these include characteristics of farmers as users of information[29] and of the communicators and media of information.[30] On the other hand, there are characteristics of the specific structural relationships among particular farmer-enterprise units; the type of communication and influence structures[31] and the norms pertaining to new technology, media, independence of decision, etc.,[32] are examples. Third, the average diffusion rate has been shown to be related to certain characteristics of the innovation itself. There are, on the one hand, certain properties of the innovation that are inherent, so to speak, such as divisibility, concreteness, cost, communicability, and, on the other hand, properties that accrue to the innovation in view of its relationship to other practices in the farm enterprise, for example, compatibility, risks, size of return in money or labor-saving, increased skill required in its use, etc.[33] In some-

[27] For these farm practices the decision unit is not a group (cf. Elihu Katz, "Notes on the Unit of Adoption in Diffusion Research," *Sociological Inquiry*, XXXII [Winter, 1962], 4–5).

[28] See above n. 4. For a summary see Herbert F. Lionberger, *Adoption of New Ideas and Practices* (Ames: Iowa State University Press, 1960), chap. vii.

[29] See esp. Elihu Katz and Paul F. Lazarsfeld, *Personal Influence* (Glencoe, Ill.: Free Press, 1955), pp. 219–335; James S. Coleman *et al.*, *op. cit.*; Herbert F. Lionberger, *Information Seeking Habits and Characteristics of Farm Operators* (Research Bulletin 631 [Columbia: Missouri Agricultural Experiment Station, 1957]); Elihu Katz, "The Social Itinerary of Technical Change: Two Studies on the Diffusion of Innovation," *Human Organization*, XX (Summer, 1961), 70–82.

[30] E. A. Wilkening, "Informal Leaders and Innovators in Farm Practices," *Rural Sociology*, XVII (September, 1952), 272–75; C. Paul Marsh and A. Lee Coleman, "Farmers' Practice-Adoption Rates in Relation to Adoption Rates of 'Leaders,'" *Rural Sociology*, XIX (June, 1954), 180–81; Herbert F. Lionberger, "Some Characteristics of Farm Operators Sought as Sources of Farm Information in a Missouri Community," *Rural Sociology*, XVIII (December, 1953), 327–38; and Everett M. Rogers, *Characteristics of Agricultural Innovators and Other Adopter Categories* (Research Bulletin 882 [Wooster: Ohio Agricultural Experiment Station, 1961]).

[31] Elihu Katz, "The Two-Step Flow of Communication: An Up-to-Date Report on an Hypothesis," *Public Opinion Quarterly*, XXI (Spring, 1957), 61–78; Lionberger, *Information Seeking Habits* . . . , pp. 30–31; Elihu Katz, "The Social Itinerary of Technical Change . . . ," *op. cit.*

[32] James N. Young and A. Lee Coleman, "Neighborhood Norms and the Adoption of Farm Practices," *Rural Sociology*, XXIV (December, 1959), 372–80; Pedersen, *op. cit.*; van den Ban, *op. cit.*; related studies of practice adoption and values are summarized in Lionberger, *Adoption of New Ideas and Practices*, chap. vii.

[33] Homer G. Barnett, *Innovation* (New York: McGraw-Hill Book Co., 1953), chap. xii; Fliegel and Kivlin, *op. cit.*; Herbert Menzel, "Innovation, Integration, and Marginality; A Survey of Phy-

what more general respects the average diffusion rate is thus a function of the social, economic, and "emotional" resources of the farmer-enterprise systems, of the structural mechanisms of the social system for providing and distributing essential resources, of the values governing the desirability of relevant resources, and of the intrinsic and extrinsic properties of the innovation.

Rank-order measurement of some of these properties is possible for the twelve localities. Relative locality differences in farmer-enterprise resources in 1950 are reflected in the median education of farmers, which ranged from 4.4 to 8.8 years of school completed, and median value of farm products produce (a measure of scale of farming). In 1950, the localities ranged in value of crops produced from a low of $1,120 to a high of $3,678. However, the level of education and scale of farming, even in the top localities, were rather low compared with those in localities in other parts of the country. The strength of the scientific farming value in each locality is indexed by the extent to which farmers in 1955 held highly favorable attitudes toward new practices and the media foremost in promoting them.[34]

Several measures of participation in formal and informal social structures are available. Locality medians in social participation were computed from scores on the *Chapin Social Participation Scale* administered in 1950. Median participation score varied by locality from 5.8 to 13.8. Both the extent of informal visiting in localities and the solidarity of locality visiting can be determined from responses in 1950 to questions on visiting relationships.

More directly related to diffusion, however, is the farmer's participation in the communication structure. An index of use of printed media (*I-pm*),[35] and an index of use of institutionalized media (*I-im*),[36] were constructed from responses in the 1955 survey to questions concerning the general use (not the specific use for the six recommended practices) of personal and impersonal sources of farm information. The extent of contact with information media is indexed by the percentage of farmers in each locality who had contacts with two or more media of both types. This varied from 7 to 82 per cent.

In earlier reports of the 1955 survey, farmers' adoption of recommended practices has been shown to be closely related to the extensiveness of contacts with printed and institutionalized media.[37] Moreover, the adoption level of a locality was shown to be closely related to the extent to which the farmers tried farm practices after discussing them with other farmers whom they regarded as "successful."[38] Using the hypothesis of the two-step flow of influence as an analogy, the author hypothesized that locality *ADR* is directly related to the degree to which farmers talked about farming matters with those named as "successful" who had more extensive contacts with media than themselves. With this in mind an index of the integration of communication structure was constructed.[39] It reflects locality differences ranging from those in which farmers actually tended to name and talk

sicians," *American Sociological Review*, XXV (October, 1960), 704–13; Griliches, "Hybrid Corn: An Exploration . . . ," *op. cit.*

[34] Young and Coleman, *op. cit.*

[35] This index is the number of the following media used by a farmer: farm bulletin, newspaper, and farm magazine.

[36] This index is the number of the following media used by a farmer: farm meeting, representatives of agricultural agencies, letters from the county agricultural agent.

[37] C. Milton Coughenour, "The Functioning of Farmers' Characteristics in Relation to Contact with Media and Practice Adoption," *Rural Sociology*, XXV (September, 1960), 283–97.

[38] Young and Coleman, *op. cit.*

[39] This index is developed from the cross-tabulation of farmers in a locality and those named by them as "successful farmers," according to the number of institutionalized media used in each case. The index expresses the extent to which the farmers in each locality talk to "successful farmers" having contacted more media than themselves relative to the potential extent to which this might occur.

to as "successful farmers" persons who had fewer contacts than themselves with information media, to localities in which there was maximum integration of communication structures in the sense that all farmers named and talked to as "successful farmers" persons who had the highest possible scores on media contacted.

With the exception of the measures of visiting, a direct relationship is expected between *ADR* and each of the independent variables. Visiting is commonly regarded as an indicator of the strength of social ties and of locality ties to the extent that social ties are locality bound. While visiting solidarity thus indicates the extent to which, in this sense, the localities may be regarded as social groups, a relationship to *ADR* is expected only in case visiting relationships are related to patterns of influence. Since this is not specified in the index of visiting solidarity, little or no relationship with *ADR* is expected.

The hypothesized relationships between the average diffusion rate for each practice and *ADR* and the measures of locality-system characteristics are clearly supported by analysis of data for the twelve Kentucky localities (Table 3). Except for the extent and solidarity of visiting relationships, the correlations between the measures of diffusion rates and of resources and communication structures are strongly positive. Most closely correlated with the average diffusion rates are median level of education, use of media of farm information, and the index of the integration of communication structure. One exception is brucellosis vaccination, for which ADR is more closely correlated with median social-participation score than with the two measures of communication structure. The communication structures, of course, govern the input of cognitive materials in locality systems. Educational level, on the other hand, presumably relates more di-

TABLE 3

CORRELATIONS (τ) BETWEEN DIFFUSION RATES AND MEASURES OF LOCALITY CHARACTERISTICS

Neighborhood Characteristic	Average Diffusion Rate					
	Average of Five Practices	Soil Test	Brucellosis Vaccination	Bluestone Lime	Terracing	Artificial Breeding
1. Median education, 1950	.81**	.81**	.66*	.69**	.79**	.75**
2. Median social participation, 1950	.68*	.62*	.62*	.58*	.75**	.60*
3. Median value of products produced, 1950	.61*	.61*	.55*	.45*	.69**	.48*
4. Scientific attitude, 1955	.70**	.58*	.58*	.48*	.77**	.38
5. Index of use of media, 1955	.80**	.80**	.49*	.58*	.73**	.72**
6. Integration of communication structure, 1955[a]	.82**	.82**	.53*	.56*	.83**	.63*
7. Mean families visited, 1950	.09	.15	−.03	.12	.19	.11
8. Visiting solidarity, 1950	−.12	−.06	−.15	.03	−.14	−.17

* Significant below the 0.02 level.
** Significant below the 0.001 level.
[a] Based on eleven localities, since for one locality there were insufficient data to compute the index.

rectly to the cognitive functioning of the farmers.

For the most part, correlations between locality characteristics and *ADR* are equal to or greater than for the individual practices. This suggests, perhaps, that in *ADR* some of the idiosyncratic factors affecting the ADR's of individual practices have been averaged out, thereby providing a more accurate indicator of the underlying average diffusion rate (*ADR*) of each locality system.

case, since in all localities the persons named as "successful farmers" varied widely in the extent of their use of media, and, in any case, the index of communication integration is standardized for differences in the range of media used by "successful farmers."[40]

DISCUSSION

The level of media contact, the index of integration of communication structure, and median years of school completed are as

TABLE 4

INTERCORRELATIONS (KENDALL'S τ) OF MEASURES OF PERSONAL AND SOCIOECONOMIC RESOURCES OF LOCALITIES, AND INDEXES OF COMMUNICATION STRUCTURES

Neighborhood Characteristic	2	3	4	5	6	7	8
1. Education, 1950	.81**	.84**	.56*	.84**	.81**	.11	– 24
2. Social participation score, 1950	....	.58*	.57*	.75**	.71**	.05	– 09
3. Value of products produced, 1950	....	....	.55*	.69**	.78**	.09	– 20
4. Attitude, 1955[a]	....	....	......	.69**	.71*	.35	.08
5. Index of use of media, 1955[b]	....	....	....	....	.93**	.25	– 08
6. Index of integration of communication structure, 1955[c]	....	....	......	....	....	.45	.04
7. Mean families visited, 1950	....	....	....	....	....	....	.52*
8. Visiting solidarity, 1950	....	....	....	....	....	....	......

* Significant below 0.02 level.

** Significant below 0.001 level.

[a] Prevalence of favorable attitudes toward scientific farming, 1955.

[b] Prevalence of extensive use of media in each locality.

[c] All intercorrelations of this measure are based on 11 localities since the data for the remaining locality were inadequate

The relationships of the average diffusion rates to each of the locality-system characteristics are, of course, affected by the relationships among the latter. In fact, several of the "independent" variables are more highly correlated with each other (Table 4) than with the average diffusion rates. The correlation (τ) between levels of media contact and the index of communication integration is so high (.93) that one immediately suspects a "built-in" correlation in the construction of the index of integration of communication structure itself. This might occur, for instance, if a high integration of communication structure in the sense described were possible only where the extent of contact with [illegible] high. However, this is not the

strongly related to each other as any one of the three is to *ADR*. Since the educational attainments of a population are not dependent on the first two of these factors, it seems likely that the level of formal education makes a most important contribution both to the communication structures that develop among farmers and to the *ADR* that ensues from the three factors operating together.

It is well to remember in this respect that the diffusion curve describes the process of behavioral adoption or initial practicing of recommended technology. It thus does not describe the process either of the cognitive identification of a new idea or of its psychological acceptance.

[40] See n. 39.

Where initial application of a recommended practice is a voluntary act, it is reasonable to assume that cognitive identification precedes adoption and that the curve of identification or awareness resembles the adoption curve but rises more steeply, that is, the average rate is greater.[41] However, psychological acceptance of a new idea may either precede or follow behavioral adoption depending on the nature of the practice and how acceptance is defined. If adoption by degree is possible, as in the case of fertilizer or new seed varieties, acceptance may be withheld until after several trials, although trial by itself implies a measure of acceptance. On the other hand, if the practice must be adopted completely or not at all, as in the case of the purchase of farm machinery or a major new farm enterprise, acceptance ordinarily precedes adoption since the farmer is unwilling to adopt the innovation (if he is free to decide) until he is relatively certain that it will be successful.[42]

Since cognitive identification of an innovation is primarily a consequence of contact with media, especially mass media, and cognitive processes, it is reasonable to assume that the educational background of an individual is relatively more important in identification than in behavioral adoption, in which the supporting situational and social factors presumably perform more important functions.[43] It may be, therefore, that the strong relationship found between *ADR* and the educational and media contact levels of localities is largely due to the relationship of the latter to the process of cognitive identification of a recommended practice which precedes its initial adoption. On the same grounds, it is likely that the observed relationship between *ADR* and the measures of the integration of communication structures and of the scale of farming operations are stronger than would be the case if cognitive awareness alone were the criterion.

The value of crops and products produced is a measure of the scale of a farm operation. One expects, and ordinarily finds, some relationship for individuals between education and scale of farming, but hardly the degree of relationship that obtains among the localities in this case (.84).[44] It is, therefore, likely in this case that both mean locality value of farm production and median educational attainment are functions of the level of farming opportunity and their intercorrelation is spuriously high. In any event, *ADR* is clearly more closely related to cognitive functioning than to general economic indexes of the farm enterprises in which the practices are to be used.[45]

Particularly interesting are the correlations involving the strength of favorable attitudes toward scientific farming. Locality differences in this respect are almost equally related (about .70) to *ADR* and the extent of media contact and the integration of communication structure, but they are less strongly related to the remaining factors. Inasmuch as the scientific farming index involves attitudes toward these media and new farming ideas, and since *ADR* is more closely related to measures of the communication structure than to

[41] See, e.g., data reported in Bryce Ryan and Neal Gross, "The Diffusion of Hybrid Seed Corn in Two Iowa Communities," *Rural Sociology*, VIII (March, 1943), 15–24, Fig. 1.

[42] For an analysis of the adoption process using a similar conceptualization see R. G. Mason, "Information Source Use in the Adoption Process" (unpublished Ph.D. dissertation, Stanford University, 1962).

[43] Consider esp. J. H. Copp *et al.*, "The Function of Information Sources in the Farm Practice Adoption Process," *Rural Sociology*, XXIII (June, 1958), 103–11; and F. E. Emery and O. A. Oeser, *Information Decision, and Action* (New York: Cambridge University Press, 1958), Parts II, III, and IV.

[44] For the 393 farmers in the 12 localities in 1950 the Pearsonian correlation between educational attainment and the value of crops and products produced was 0.36.

[45] It is noteworthy that other measures of farm scale—mean cropland acreage and mean productive man-work units—not shown in this analysis behave similarly to the mean value of crops and products.

attitude, one suspects that this attitude is primarily a product of the functioning of the communication structure in the diffusion process. More than anything else, in other words, it may be an expression of collective farmer satisfaction with the general applicability and adoption of new ideas and the structures that have transmitted them. In turn, the attitude may tend to predispose farmers to retain contact with, and to accept new ideas transmitted by, these media.

The level of participation by farmers in formal organizations is less closely related to *ADR* than to educational attainment and to the two measures of communication structure. Since some of the organizations included in the social-participation score are directly involved in transmitting new ideas to farmers, the observed correlation seems largely due to the relationship between educational attainment and social participation and its overlap with the more specific measures of the communication structure.

SUMMARY

Data on the diffusion of five farm practices in twelve Kentucky localities were analyzed in regard to the speed of diffusion and the factors related to it. Average diffusion rates for the practices in one locality, relative to the rates in the other localities, were found to be highly correlated, indicating the presence of an underlying average diffusion rate characteristic of each locality. This characteristic average locality diffusion rate (*ADR*) was found to be closely correlated with locality differences in mean diffusion and utilization score (based on a list of more than eighteen farming practices). This both strengthens the conclusion that there is an underlying *ADR* in each locality and permits a more adequate interpretation of previous findings.

It was hypothesized that locality differences in *ADR* are related to (1) the socioeconomic and attitudinal resources of the locality systems, and (2) the nature of social relationships with information sources and media, including the extent of contacts with media and the integration of media and interpersonal communication structures. Among measures of these factors for which data were available, the most closely correlated with locality differences in *ADR* were: the educational level of farmers in the locality, the extent of their use of information media, and the integration of communication structure.

UNIVERSITY OF KENTUCKY

APPENDIX

In the typical case an increase from one time to the next in the absolute number of units (whether individuals or organizations) adopting an idea is attributed to diffusion. Clearly, this is so only when there has been no change in the relevant population, however this may be defined. But during the typical diffusion cycle of an agricultural practice, which may extend from one to several decades, there is ordinarily considerable change in the farmers engaged in the enterprise to which the practice applies. Thus, if the initial population in a locality is used as the basis for determining cumulative adoption and if there is correlation between adoption (or non-adoption) and population loss, the diffusion curve may appear to attain stability at less than complete adoption even though every one of the relevant units remaining in the population has adopted the idea. This difficulty can be avoided, of course, by basing the diffusion curve on the proportion of adopters in the population during successive time periods, which is the procedure used in the present study.

Nevertheless, where adoption (or non-adoption) is correlated with change in the decision units to which the practice applies, the proportion "ever adopting" may increase or decrease from one time period to the next simply because of a disproportionate increase or decrease in the decision units that formerly had (or had not) adopted the practice.[46] Moreover,

[46] For this reason, however, the percentage of those in a locality "ever adopting" a practice tends to mask certain aspects of the over-all process of technological change. This is a general problem which arises in the use of sociogeographic areas in the study of diffusion.

if the diffusion curve is reconstructed on the basis of data obtained from the population living in an area after diffusion has occurred, the curve will be misleading to the extent that the population at the time of the survey is different from that existing at the time diffusion began.

A general methodological solution to the problem of population change in field studies of diffusion is difficult, if not impossible, to attain; in this case, however, the decline (or increase) in diffusion, which is reflected in the proportion who reported ever using a practice, is related to population change in only slight degree, if at all. This is so at least for the diffusion trends after 1955 when fluctuation in the percentage ever using a practice was most pronounced.

This is evident from the following considerations. First, since the diffusion index used is based on those having the enterprise to which the practice applies, the percentage ever adopting a practice is insensitive to change in either the number or the percentage of farmers having a particular enterprise where there is no correlation between population change and adoption. In the latter regard, no association was found between increase or decrease in the diffusion curve after 1955 and differences in either (*a*) the percentage loss of farmers in each locality in the 1955 to 1960 period,[47] or (*b*) the percentage of adopters in 1955 who were interviewed in 1960. In fact, no general factor of population change or size can account for these fluctuations in the diffusion curves since the localities in which the diffusion of one practice declined are not generally the same as those in which the second practice declined.

The direction of the diffusion curve after 1955, in particular, thus seems to be a response primarily of farmers in certain localities to particular practices. With respect to brucellosis vaccination and the use of bluestone lime, the direction of the diffusion curve in each locality is most closely associated with the direction of the curve of the actual utilization of these practices during the same time period. That is, where the actual utilization of the practice declined, there was a tendency also for the diffusion curve based on the percentage reporting that they had ever used the practice to decline. In fact, at the end of the decade some farmers failed to report that they had once used one or the other of these practices even though they had reported using them on earlier surveys. This suggests that the farmer's present attitude regarding the utility of a practice is a factor in his reporting its prior use.

Assuming that the measure of diffusion is to some extent unreliable, the crucial question in the present analysis is the extent to which the area under the diffusion curve, which indexes relative ADR, is affected by the apparent decline in diffusion. By assuming that there is unreliability in the data only where the direction of the diffusion curve is downward,[48] and that in these instances (i.e., where utilization is declining) there has been no advance in diffusion, an adjusted diffusion curve can be drawn. A comparison of the areas under the original and adjusted diffusion curves provides an indication of the extent to which the unreliable responses may affect the original index of relative ADR.

Despite the differences that occur between the original and adjusted diffusion curves, there is little difference in the relative locality ADR for each practice. A difference in this respect occurs only for brucellosis vaccination and the use of bluestone lime, and Kendall's τ for the original and adjusted ranks is .93 for the former and .97 for the latter. Since these are quite high and since the intercorrelations among ADR's for the adjusted data are lower than for the original data, the latter are used in the analysis.

[47] For brucellosis vaccination, however, there is a significant *inverse* association between differences in percentage loss of farmers, 1950–60, and decrease or increase in the diffusion curve after 1955. Thus decrease in the diffusion curve tended to occur in those localities that over the decade were the most stable!

[48] There may be some question about the validity of this assumption since those who tend to deny ever having used a practice when the practice does not have utility may be equally inclined to erroneously report its use during periods of widespread utilization. In such cases, however, the effect of the assumption is to exaggerate the true difference in the areas under the original and adjusted diffusion curves, and the conclusion of no important difference in the measure of relative ADR is thus conservative.

COMMUNITY STRUCTURE AND ACCEPTANCE OF PSYCHIATRIC AID

EDNA E. RAPHAEL

ABSTRACT

Ecological distribution of cases examined at a child guidance clinic is atypical when compared to distribution of indexes of personal and social disorganization. The hypothesis that this distribution is related to diffusion of psychological and psychiatric explanations of conduct is sustained when variation in rates among areas is observed to correspond closely with indicators of community acceptance of psychological and psychiatric explanations. The defining role of professional workers is important in the diffusion.

The data of the present study consist of the 3,017 cases examined during 1951–58 at a large, free, and publicly supported child guidance clinic located in Chicago.[1] These cases were first examinations, and all of the children were residents of the city. Preliminary study indicated that the ecological distribution of these data was highly atypical: rates of examined cases by community areas of the city did not correlate with rates of juvenile delinquents, total first admissons to the state's mental hospitals, or the incidence of such chronic diseases as tuberculosis (Table 1).[2] Although the ecological distribution of these data did not follow the traditional pattern, it was not random but instead appeared to have a distinctive consistency. Children seemed to be coming to the clinic from both upper and lower socioeconomic areas of the city. But within both socioeconomic levels the children came somewhat more frequently from areas where large proportions of the residences were multiple-dwelling-unit structures.[3] It seemed ad-

[1] The Institute for Juvenile Research not only was the first child guidance clinic established in Western society (1909), but also is reputed to be the largest of its kind. At the time of the study between 600 and 900 children under 18 years of age were being examined annually by its large professional staff of psychiatrists, psychiatric social workers, and clinical psychologists. Although not equally distant from all parts of the city, the clinic is located just west of the city's central business district and is easily accessible by means of public transportation. It is located within a large medical center complex lying just west of Chicago's central business district.

Although distance of residence from the clinic may have had some bearing upon continuation in treatment, it appeared to have had little effect on the ecological distribution of the examined cases. During the latter part of 1959 and throughout 1960 the clinic, quite independently of this investigation, kept a record of every request made for service or information, including telephone conversations, by address of the inquiring persons. A 50 per cent sample of these 1959–60 service requests was compared with the clinic's examined population under study (1951–58). Rates of service requests and of examined cases, by community areas of the city, correlated at a significant level ($r=0.545$, $p<.01$). Thus, not only did it appear that examinations were not appreciably affected by place of residence, but also that the distinctive but atypical distribution was relatively stable and reflected throughout the city. The author wishes to express her warm appreciation for the critical assistance and counsel of H. D. McKay, S. Kobrin, K. I. Howard, and the clinic personnel.

[2] For several decades a persistent ecological pattern of distribution of rates of indicators of personal and social disorganization, including juvenile delinquents and cases of mental disorders requiring hospitalization, has been recognized for Chicago and for other American commercial and industrial cities. Following the growth pattern of these cities, high rates of juvenile delinquents and hospitalized mental illness tend to be concentrated toward the center of the city and to decline as one approaches the city's periphery (see Clifford R. Shaw and Henry D. McKay, *Juvenile Delinquents in Urban Areas* [Chicago: University of Chicago Press, 1942], and Robert E. Faris and Henry W. Dunham, *Mental Disorders in Urban Areas* [Chicago: University of Chicago Press, 1939]). Juvenile delinquents and cases of mental disorders requiring hospitalization were selected as comparative ecological indicators of social and personal disorganization because they were the most relevant to the topic under consideration. There are no epidemiological studies available which are similarly concerned with the distribution of mental illness in child populations.

[3] Not unrelated is Gruenberg's observation that rates of first admissions of senile psychosis and cerebral arteriosclerosis among persons over 65

visable to explore the possibility that the complexity of this distribution at the ecological level referred to an underlying differential distribution of acceptance of the social innovation of seeking psychiatric help.

The study proceeded systematically to explore the proposition that current use being made of such psychiatric facilities as out-patient clinics reflected an underlying differential diffusion in the city's population of a social innovation, where the social innovation referred to the interpretation of conduct in psychological and psychiatric terms. Substantive literature in the fields of mass communication and diffusion of innovation provided the conceptual framework for the study of this proposition. While other perspectives might well have been applied to them, examination of the data in these terms indicated that their distribution might very plausibly be explained as representing a consequence of the differential diffusion of innovation. Children were brought to the clinic more frequently from those areas of the city where the theory predicted that the innovation would diffuse rapidly, and less frequently from those areas where the theory predicted resistance to the innovation. At the same time voluntary parental referrals, representing acceptance of the innovation, were more frequent in areas with high examined-case rates and less frequent in areas with low examined-case rates. Finally, the severity of the disturbance exhibited by the children was relatively independent of the frequency with which they came to the clinic from the widely differing areas of the city.

TABLE 1

CORRELATIONS AMONG RATES OF CLINIC-EXAMINED CASES, JUVENILE DELINQUENTS, FIRST ADMISSIONS TO STATE MENTAL HOSPITALS, AND TUBERCULOSIS: 73 COMMUNITY AREAS, CHICAGO*

	Juvenile Delinquents	First Admissions to Mental Hospitals	Tuberculosis
Clinic-examined cases (1951–58)†	0.033	0.009	−0.036
Juvenile delinquents (1954–57)‡	....	0.777§	0.775§
First admissions to state mental hospitals (1949–51)‖	....	....	0.917§
Tuberculosis (1949–51)#	....	....	....

* A community area in Chicago is a combination of adjacent Census tracts "having in the main, a history of its own as a community, a name, and an awareness on the part of its inhabitants of some common interests" (Philip M. Hauser and Evelyn M. Kitagawa, *Local Community Fact Book for Chicago, 1950* [Chicago: Chicago Community Inventory, University of Chicago, 1953], p. xi). At the time of the study there were 75 community areas designated for Chicago. In this preliminary phase of the study because of their atypical residential populations the two community areas constituting the central business district were not included. As will be noted for other tables in this text later in the study a third area was randomly dropped in order to facilitate statistical operations and the number of areas studied thus was 72.

† Rates per 10,000 persons under 18 years of age (1950–60 average populations).

‡ Rates per 100 males 12–16 years of age, interpolation of 1950–60 populations (Henry D. McKay, *Rates of Delinquents by Communities in Chicago, 1954–1957* [rev. ed.; Chicago: Institute for Juvenile Research, 1961]).

§ Correlations significant at 0.01 per cent level or better.

‖ Rates per 10,000 total population in 1950. Data made available through the courtesy of the statistical office of the Department of Mental Health, State of Illinois.

\# Rates per 10,000 persons 15 years of age and older, 1950 (three-year average) (Hauser and Kitagawa, *op. cit.*, p. 5).

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years old were higher in areas of Syracuse with multiple dwelling structures, independent of average monthly rental (Ernest M. Gruenberg, "New York State Mental Health Commission Research Unit, Syracuse, New York," in *Interrelations between the Social Environment and Psychiatric Disorders* [New York: Milbank Memorial Fund, 1953], pp. 186–94).

Insofar as it can be safely assumed that the decision to bring children to this clinic was made by adults and not by the children themselves, it seems likely that observations reported in this paper about the examined population of this particular child guidance clinic may be generalizable to psychiatric clinics that serve adults as well as to other clinics that serve children. Since the present study's view of the population of the psychiatric clinic in the urban setting is rather novel, we will begin first by indicating in some detail the development of the conceptual framework for the analysis. A later section of the paper will consider the results of tests of specific hypotheses.

THE MENTAL HEALTH MOVEMENT—A SOCIAL INNOVATION

Some years ago John R. Seeley proposed that the mental health movement and its following could be viewed as a social innovation directed at the "remedy or alleviation of felt psychological stress."[4] He ascribed the appearance of this innovation to the revolution in social values that accompanied the Industrial Revolution, to the decline of the dominance of the church as the arbiter of conduct, and to the appearance at the same time of widespread personal distress accompanying radical social change. More recently Kadushin proposed that the followers and supporters of psychiatry and psychotherapy be viewed as constituting an interstitial community or sect of believers.[5]

Kingsley Davis' now classic paper focused attention upon the ideological content rather than upon the institutional context out of which the movement had emerged.[6] He demonstrated that the movement had taken over as its own underlying assumptions the dominant Protestant and mobile middle-class ethic that accompanied the Industrial Revolution. He placed considerable stress upon the possible invidious consequences of the fact that these assumptions provided the basis both for conceptions of mental health and scientific analysis. In this context the reduction of conduct to psychologisms became to him a means whereby the middle-class ethic might be propagated rather than a means of adequate therapeutic intervention and scientific analysis. He forecast as a consequence that this innovation would find its adherents and its effectiveness confined to the middle class. Studies such as those of Hollingshead and Redlich appear amply to have demonstrated the validity of Davis' analysis.[7]

Seeley forecast a rather different outcome for the mental health movement, one that has not as yet been explored. He predicted that the very existence of the movement would have the consequence of tending increasingly to focus attention upon alleviation of problems of felt psychological stress independent of any change in their incidence or prevalence.[8] And unlike Davis, Seeley made no reference to a possible class containment of this innovation. The present study, focusing upon an examined population, rather than the more customary subsequently selected treated population, of a psychiatric clinic, tends to support Seeley's prediction that the diffusion of the acceptance of such services would become both independent of the incidence or prevalence of mental illness, and more

[4] "Social Values, the Mental Health Movement, and Mental Health," *Annals of the American Academy of Political and Social Sciences*, CCLXXXVI (March, 1953), 15–24.

[5] Charles Kadushin, "Social Distance between Client and Professional," *American Journal of Sociology*, LXVII (March, 1962), 517–31.

[6] "Mental Hygiene and the Class Structure," *Psychiatry*, I (February, 1938), 55–65. Lemert and Szasz refer to this movement as one aimed at substituting the more acceptable concept of "illness" for the stigmatic concepts of "insanity" and "malingering" (Edwin M. Lemert, *Social Pathology* [New York: McGraw-Hill Book Co., 1951], p. 412, and Thomas S. Szasz, *The Myth of Mental Illness* [New York: Harper & Bros., 1961], pp. 21–72).

[7] August B. Hollingshead and Frederick Redlich, *Social Class and Mental Illness* (New York: John Wiley & Sons, 1958).

[8] Seeley, *op. cit.*

general than class confined. The same data also suggest that the followers of the mental health movement do indeed serve as its "mediators,"[9] as the instrumentality for diffusion of the innovation of seeking psychiatric assistance, through other than middle-class segments of the urban community. Although this potentially profitable perspective on mental illness has been suggested repeatedly in the work of the above and other writers, perhaps the novelty of the present paper is that it provides some initial substantive support for their theories. Following the lead provided by the work of these students of the mental health movement, the literature on the diffusion of innovation, reviewed in the following paragraphs, provided a conceptual framework for operationalizing and then testing specific hypotheses that accounted for the atypical ecological distribution of the clinic's examined population.

DIFFUSION OF THE INNOVATION

Recent studies have reported that within communities the diffusion of information and innovation proceeds in phases. In studies of urban communities this phasing of communication and change has recently been described by the two-step flow of communications hypothesis.[10] Studies of rural and non-industrial communities refer instead to distinctions between early and later adopters of innovation as social types and to the differential permeability to change exhibited by communities as wholes. The phasing theories described by these hypotheses are an outcome of differing research strategies.

The data of the two-step flow hypothesis are essentially sociometric and refer to the diffusion of information and innovation stemming from the mass media.[11] A distinction is made between "influentials," those persons who are more subject to influences stemming from the mass media (step 1), and "influencees," whose decisions are affected by the influentials (step 2). By contrast with the influentials, the influencees are less subject to influences stemming from outside their communities and more dependent upon interpersonal communications, especially those coming from the influentials, for information and decisions. According to the studies which have thus far supported the two-step flow hypothesis, diffusion of information and innovation occurs largely within rather than across differentiated segments of community structures, for example, social classes. The research strategy, being essentially cross-sectional rather than longitudinal, tends to stress the fact that diffusion of innovation remains confined within differentiated segments of community social structures and only very limitedly moves across them. However, it is recognized that innovations may be adopted simultaneously by influentials located in different segments of community structures and from them flow to the corresponding influences. The present paper will be able to present evidence suggesting possible conditions for the diffusion of innovation across differentiated segments of the urban community.

The phasing theory emerging from studies of rural and non-industrial communities has referred largely, although not completely, to the diffusion of innovation stemming from "expertness" located outside of the community. Among other things, this literature is illuminating because it indicates that the diffusion of innovation sometimes does or can potentially proceed across differentiated segments of community structures. Here, instead of influentials and influencees, early and late adopters of innovation are seen

[9] *Ibid.*

[10] Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting: A Study of Opinion Formation during a Presidential Campaign* (Chicago: University of Chicago Press, 1954).

[11] Elihu Katz and Paul F. Lazarsfeld, *Personal Influence* (Glencoe, Ill.: Free Press, 1955); Elihu Katz, "The Two-Step Flow of Information: An Up-to-Date Report on an Hypothesis," *Public Opinion Quarterly*, XXI (Spring, 1957), 61–78.

as social types.[12] Like influentials, early adopters of innovation appear to be more susceptible than late adopters to influences stemming from outside their communities. Like influencees, late adopters appear to be more dependent than early adopters upon primary-group interpersonal contacts for their decision-making.

The bridging mechanism for the flow of information and innovation from early to late adopters has not been elaborated in the literature on the diffusion of innovation within rural or non-industrial settings. The possibility of such a bridging mechanism has been suggested by Merton's reference to the presence, at least within urban communities, of two distinctly different but intersecting influence systems and two corresponding types of influentials, the cosmopolitans and the locals.[13] The influence of cosmopolitans within communities arises from expertness in rational-scientific spheres, while the influence of locals appears to be associated with extended interpersonal contacts. For example, Merton cites and contrasts the cosmopolitan expertness of the social worker with the localistic influence of the precinct captain, both of whom attempt to work on similar problems of constituents.[14]

Rural and non-industrial studies also have reported that, independent of distinctions between early and late adopters of innovation as social types, communities as wholes differ in their permeability to innovation. A number of hypotheses have been proffered as possible explanations of such community differences.

Since the data of the present report did not permit analysis in terms of the flow of influence, the study was dependent upon a review of the literature describing tl characteristics that distinguish early fro late adopters as persons, and upon tl literature on differential community perm ability to innovation.

EARLY ADOPTERS OF INNOVATION

In a number of studies early adopte have been variously described as margina secular, or cosmopolitan.[15] They have al been distinguished by such specific soci characteristics, among others, as hig levels of educational attainment, young age, higher socioeconomic status, and greater propensity to migrate. Dependir upon the community context of the stud (i.e., rural or non-industrial versus urba industrial) differing combination of the characteristics appear somewhat like clu ters distinguishing early adopters froı their late-adopting community counte parts. Thus, while a propensity to migra does not appear to be a consistently rel vant attribute of early adopters of inn vation in rural or non-industrial setting it appears to be an important descripti variable with reference to early adopte in the urban industrial setting.[16] An while marginality and secular orientatior appear to characterize early adopters i rural or non-industrial settings inde pendently of any propensity to migrate,[17] in the urban industrial environmen marginality, secular orientations, and th propensity to migrate appear to be inte correlated.[18] Similarly, indexes of socic economic status do not consistently diffe entiate early from late adopters of innc vation.[19] In the non-sociometric studie the one attribute that appears most con

[12] Bryce Ryan and Neal Gross, *Acceptance and Diffusion of Hybrid Corn Seed in Two Iowa Communities* (Research Bulletin No. 372 [Ames: Agricultural Experiment Station, Iowa State College of Agriculture and Mechanic Arts, January, 1950]).

[13] Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1957), pp. 387–420.

[14] *Ibid.*

[15] Edward Wellin, "Boiling Water in a Peruvia Village," in *Health, Culture and Community*, e Ben D. Paul (New York: Russell Sage Foundatio 1955), pp. 71–103; Merton, *op. cit.*; Ryan an Gross, *op. cit.*; and Bryce Ryan, L. D. Jayasen and D. C. R. Wickremesinghe, *Sinhalese Villag* (Coral Gables, Fla.: University of Miami Pres 1958).

[16] Merton, *op. cit.*

[17] Wellin, *op. cit.*

[18] Merton, *op. cit.*

[19] Ryan and Gross, *op. cit*

sistently to differentiate early from late adopters of innovation is a high level of formal educational attainment often associated with younger adult age groupings. That is, wherever communities are internally differentiated in terms of the formal educational attainments of their members, it appears that larger proportions of persons with higher educational attainments (often in young adult age groups) will be found among the early than among the late adopters. The exact level of educational attainment appears to be inconsequential when compared with the presence of some differentiation along this axis. The association between early adoption of innovation and higher levels of educational attainment appears in studies of urban as well as rural and non-industrial communities.[20]

In connection with the item viewed as a social innovation in the present report—the psychological or psychiatric interpretation of conduct—both in community and national surveys in the United States—it has repeatedly been reported that better educated and younger persons are more disposed than other population subgroups toward evaluating their own or others' problems in psychiatric terms. Furthermore, it is these better educated persons who not only are most likely to avail themselves of existing psychiatric services but also to recommend such services for others. Thus, Gurin and his co-workers reported that in a national survey the subjective definition of a need for psychiatric assistance and the utilization of available psychiatric treatment resources was "highest among the higher educated and younger age groups."[21] In the 1950 Roper survey of Louisville, again it was the more educated persons in the sample who exhibited the strongest disposition to define a set of objective descriptions of conduct presented to them as reflecting mental illness.[22] Woodward, identifying better educated and younger persons, reported a similar finding for a survey conducted in St. Louis.[23] Recently controlled experimental studies in mental-health education have indicated that modification in attitudes toward the mentally ill occurs with greater frequency among persons with high levels of educational attainment.[24] Similarly, in the present study the correlation between rates of clinic-examined cases and mean educational attainment of the adult populations in the community areas where the children resided was significant although not very high ($r = 0.35$, $p \gtreqless 0.01$, $N = 73$).[25]

While no attempt has been made to determine why better educated persons are consistently more responsive to innovation, at least in the case of the psychiatric definition of conduct it can be

[20] Ryan and Gross, *op. cit.*; Paul C. Marsh and A. Lee Coleman, "Group Influences and Agricultural Innovations: Some Tentative Findings and Hypotheses," *American Journal of Sociology*, LXI (May, 1956), 588–94; Clifford R. Shaw and Henry D. McKay in a personal communication noted that in developing neighborhood delinquency-control programs in each case their initial following consisted of young men who were a little better educated than their neighbors.

[21] Gerald Gurin, Joseph Veroff, and Sheila Feld, *Americans View Their Mental Health* (New York: Basic Books, 1960), pp. 295–96, 298–300.

[22] Elmo Roper, *People's Attitudes concerning Mental Health* (The City of Louisville and Collier's Magazine, September, 1950).

[23] Julian L. Woodward, "Changing Ideas on Mental Illness and Its Treatment," *American Sociological Review*, XVI (August, 1951), 443–54. Other studies which direct attention to the discriminating power of educational attainment also may be cited: Jum C. Nunnally, Jr., *Popular Conceptions of Mental Health* (New York: Holt-Rinehart & Winston, 1961), p. 47; Howard E. Freeman, "Attitudes toward Mental Illness among Relatives of Former Patients," *American Sociological Review*, XXVI (February, 1961), 59–66; Kenneth I. Howard and David T. A. Vernon, "Conceptions of Mental Health Phenomena among Psychiatric Patients" (unpublished manuscript [Chicago: Hines Memorial Hospital, Veteran's Administration, 1961]).

[24] Elaine Cumming and John Cumming, *Closed Ranks* (Cambridge, Mass.: Harvard University Press, 1957), pp. 57–60; John R. Scott and Howard E. Freeman, "The One Night Stand," *Social Problems*, X (Winter, 1963), 277–84.

[25] These measures are discussed later in this paper.

inferred that the association with education reflects not only an underlying middle-class ethic, but also a greater exposure to the social and psychological sciences and a rather sophisticated acquired ability to abstract and reconceptualize action in other than traditional or customary normative frames of reference.[26] In view of the literature and of the correlation between examined-case rates and educational attainment in the present study, educational attainment was selected as one variable that might contribute to an explanation of the atypical ecological distribution of the clinic's examined population.

COMMUNITY PERMEABILITY TO INNOVATION

As indicated earlier, in addition to differences between early and late adopters as persons, some studies also suggest the presence of differential community permeability to innovation. Marsh and Coleman, for example, found that if they held education and socioeconomic status constant some neighborhoods in the agricultural county they studied adopted innovations more readily than did others.[27] As a possible explanation the authors suggested neighborhood variability in group norms. Thus, in one rural neighborhood the norms might support and accelerate change while in another the norms might retard and inhibit change, Ryan and co-workers, observing change in a non-industrial setting, suggested that the axis along which communities might be differentiated in terms of their response to innovation might be the specificity of their normative structures.[28] Ryan referred here to the tightness or looseness of culture and social organization. According to his formulation structural looseness offers wide latitude for change as long as it is consistent with non-specific normative principles. On the other hand structural tightness allows of little behavioral variation and thus creates a barrier to change.

Eliot Freidson, going a step beyond Ryan, separated the cultural and social organizational components of social structure into two separate axes. His formulation suggests that variation in community response to innovation can occur independently along both axes.[29] On Freidson's organizational axis tightness refers to the fact that persons are the more compelled to conform with prescribed norms the more they are bound up in networks of interpersonal relations. Thus tightness or looseness of the structure of interpersonal relations can vary independently of normative or cultural content. On the cultural axis, if the introduced innovation is compatible with the culture of the community, it may diffuse rapidly throughout the social system independently of the tightness or looseness of the structure. However, if the content of the innovation is incompatible with the culture, a tight structure of interpersonal relations will present a barrier to change while a loose structure will permit diffusion of the innovation, at least among such persons as early adopters who are responsive to external influences. Going a step beyond this formulation, given a setting incompatible with some innovation. extreme looseness would be accompanied by community inability to take collective action to repel invasion. Under these circumstances it could be anticipated that innovation might even be imposed. On the other hand, the imposition of a culturally incompatible innovation upon a tightly structured community might be followed by collective action to repel invasion. The latter situation is exemplified in the Cummings' report of an abortive effort by mental health edu-

[26] This difference is suggested by Schatzman and Strauss's description of the differing modes of communication noted among persons with high and low educational attainments and income (Leonard Schatzman and Anselm Strauss, "Social Class and Modes of Communication," *American Journal of Sociology*, LX [January, 1955], 329–38).

[27] Marsh and Coleman, *op. cit.*

[28] Ryan, Jayasena, and Wickremesinghe, *op. cit.*, pp. 188–95.

[29] *Patients' Views of Medical Practice* (New York: Russell Sage Foundation, 1961), pp. 198–200.

cators to introduce enlightened views of the mentally ill into a tightly knit rural community.[30]

Following a suggestion made by Merton, urban communities may be described as tight or localistic in structure if residence is fairly permanent, or as structurally loose or cosmopolitan if the residents are relatively transient.[31] The assumption underlying this distinction between residentially stable and unstable communities refers not to the widely recognized social fact that social structures with extended networks of interpersonal relationships are more likely to appeal under conditions of some permanency or stability of persons and their interactions, but rather to the fact that Merton's recent research, and other studies of migration and migrants, indicate that persons who do or do not migrate constitute different social types: the non-migrant type tends to be localistic, sacred, or traditional, oriented toward the local community rather than toward the greater society outside; the migrant social type appears to be cosmopolitan, secular, marginal or urbane, oriented toward the greater society rather than toward the local community. The distinction between migrants and non-migrants as social types refers to urban industrial rather than to rural or non-industrial communities and societies. According to the local-cosmopolitan dichotomy persons with localistic orientations tend to be bound up in extended networks of local interpersonal relationships, while those with cosmopolitan orientations tend to limit their associations to persons with like interests and skills.[32]

The distinction between migrants and non-migrants as persons enables a corresponding assumption at a more general level about communities with larger or smaller proportions of inter- or intra-city migrants in their populations. According to this assumption communities with large proportions of permanent residents may be viewed as probably exhibiting characteristics more closely associated with tight and locally oriented social structures. Communities with large proportions of inter- or intra-city migrants may be viewed as possibly exhibiting characteristics more closely associated with loose and cosmopolitan-oriented social structures.

In the present study the examined-case rates by community areas correlated inversely ($r = -0.33$, $p < .01$, $N = 73$) with the proportion of occupied dwelling units in the areas in 1960 into which the occupants had moved prior to 1954.[33] That is, the more permanent the residents in an area the lower was the area's examined-case rate. On the basis of this correlation and of the literature, migration (or rather permanence of residence) was selected as the indicator of variation along the structural axis of study. Educational attainment then became the most proximate index of possible variation along the cultural normative axis of study.

THE PARADIGM

Considered jointly, the structural and cultural axes provided a predictive model of response to the particular innovation under consideration, the interpretation of conduct in psychological or psychiatric terms, and the consequent differential use of available psychiatric aid as a response to acceptance of the innovation. It was anticipated that community areas ranking high on both axes would have the highest examined-case rates while areas ranking high on only one of the two axes would have intermediate rates and areas ranking low on both axes would have the lowest rates. Freidson's hypothetical communities thus were adapted to the purposes of the present study, as follows:

1. *Tight structure–incompatible culture.*—The structure and culture here combine to produce a social system in

[30] Cumming and Cumming, *op. cit.*

[31] Merton, *op. cit.*

[32] *Ibid.*

[33] This measure and its implications for the study are discussed later in this paper.

which persons are bound within extended networks of interpersonal relations with a traditionally oriented normative content of interpretations of conduct. Such a system is likely to be highly impermeable to any social innovation that introduces alternative psychological explanations of behavior. As a consequence, persons in these social systems are likely to make only limited voluntary use of available psychiatric-clinic treatment facilities. Such social systems may be operationally inferred from low levels of migration and low levels of educational attainment among their residents.

2. *Loose structure–incompatible culture.*—The structure and culture here combine to produce a social system in which interpersonal networks are "truncated"[34] and in which personal traditional orientations rather than cultural norms tend to exclude psychological explanations of conduct. Lacking extended networks of interpersonal influence that would reinforce personal traditional interpretations of conduct, individuals are susceptible to outside influences and thus may be introduced to a social innovation that gives novel psychological explanations for conduct. Persons in such social systems are likely to be found making more extensive voluntary use of available psychiatric-clinic treatment facilities than traditionally oriented persons in tight social structures. These social systems may be operationally inferred from the presence of high levels of migration and low levels of educational attainment among their residents.

3. *Loose structure–compatible culture.*—The structure and culture here combine to produce a social system in which interpersonal networks again are truncated but in which personal orientations are secular and not bound to community norms. Here persons are likely not only to be susceptible to outside influences but even to look to the cosmopolitan expert for counsel, and they may be relatively easily introduced to psychiatric [illegible]

innovations. Consequently, persons in such social systems are likely to make extensive use of available psychiatric-clinic treatment facilities on a voluntary basis. These systems may be operationally inferred from the presence of high levels of migration and high levels of educational attainment among their residents.

4. *Tight structure–compatible culture.*—Here the structure and culture combine to produce a social system consisting of extended networks of interpersonal relationships with cultural norms that tend to include psychological explanations of conduct. Persons in such social systems are likely to support and encourage each other in the voluntary use of available psychiatric treatment facilities. However, while persons in loosely structured culturally compatible communities are somewhat more immune to stigma and thus freer to take the step of seeking psychiatric help, the stigma attached to being designated as "mentally ill" may prevent persons in tightly structured culturally compatible communities from taking the important step of actually seeking psychiatric help. These social systems may be operationally inferred from the presence of low levels of migration and high levels of educational attainment.

The above-described typology provided a comparative predictive model within which the widely varying use being made of the examination services offered by this particular psychiatric institution could be viewed. On the basis of available information about migration and education the community areas of the city were assigned to one or another of the four community categories in the model. If the rank ordering of their examined-case rates corresponded with those predicted by the model, the hypothesis that current use of such psychiatric facilities represents a consequence of the differential diffusion of a social innovation rather than either the incidence or prevalence of mental illness would be plausible. Such plausibility would not imply that persons who sought out

these services did not need psychiatric help. Rather, the underlying implication of the hypothesis is that it will be easier for persons to arrive at the decision to seek psychiatric assistance if such a decision is supported by community norms, or if the social structure does not interfere with the suggestion that they try psychiatric help for their problems. Also, the present study's reliance upon ecological analysis rather than close accounting of community influence systems results in observations that are suggestive and plausible but far from definitive. The four types of communities with the expected outcomes are presented schematically in Table 2.

It was predicted above that the proportion of examined cases appearing at the clinic as voluntary (parental) referrals would be higher in those communities where the rates of examined cases were high. This hypothesis implied that the proportion of cases seen involuntarily would be higher in communities with low rates of examined cases than in communities with high rates. Some elaboration of the assumptions underlying this hypothesis is required.

Since the literature indicates that definitions of problems as reflecting mental illness depend upon a high level of educational attainment, larger proportions of voluntary referrals could be anticipated from communities with high levels of educational attainment, that is, tight-compatible and loose-compatible communities. But the followers of the mental-health movement and the supporters of psychotherapy could also be expected to be found more frequently among persons with higher levels of educational attainments, as residents in tight-compatible and loose-compatible communities. These followers could be expected to serve as the "definers" (Seeley's mediators) of mental illness for others, whether they are or are not members of the same communities. If the community of believers served as "definers" of mental illness in other than their own communities they could be seen as a bridging mechanism for diffusing the interpretation of conduct in psychiatric terms and the use of psychotherapy among persons who would not otherwise even potentially be members of the community of believers.

The general category of psychotherapists, their clients and friends, includes a large body of lay practitioners, such as teachers and non-psychiatric social workers, whose training in child development includes a heavy emphasis upon the social sciences, the psychoanalytic model of personality development, and psychiatry and abnormal psychology. It appears to be widely accepted that as part of their expected role behavior these lay or non-psychiatric professionals will judiciously apply the behavioral frame of reference in their practice; but it is also expected that, if they encounter people whose problems appear too severe, they will refer them to appropriate psychiatric sources of assistance. By contrast with non-psychiatric physicians who also refer persons to the psychiatric specialist, professionals such as teachers and social workers often are in positions of sufficient power to [illegible]

TABLE 2

TYPES OF COMMUNITIES AND THEIR PREDICTED RESPONSES TO PSYCHIATRIC INNOVATION

CULTURE	SOCIAL STRUCTURE	
	Tight	Loose
Compatible (psychologically oriented)	Type 4: Migration low, Education high, High*	Type 3: Migration high, Education high, High high
Incompatible (not psychologically oriented)	Type 1: Migration low, Education low, Low	Type 2: Migration high, Education low, High

* "High" and "low" indicate predicted order of ranks of rates of examined cases.

pose psychiatric "definitions" upon others. With a child population, as in the present study, teachers could be expected to serve as a rather crucial bridge for diffusion of the psychiatric innovation.

However, schools and teachers are necessarily responsive to the communities in which they are located. Thus, given communities where the culture is amenable to psychiatric interpretations of conduct, in addition to large proportions of voluntary referrals, teachers also could be expected to be found "defining" mental health problems and referring children for psychiatric examination. Teachers similarly might be found "defining" and referring children with mental health problems in communities with loose social structures and incompatible cultures, while voluntary (parental) referrals from such communities would be relatively few in number. By contrast, given communities with tight social structures and incompatible cultures teachers would not be free to impose mental health "definitions" of problems observed in children, or families would resist the imposition of such definitions. Consequently, both teacher and voluntary referrals would be relatively low in tight incompatible communities.

As for the total examined-case rates, preliminary analysis again indicated the presence of considerable variation in source of referral separately along the cultural and then along the structural axis. Along the cultural axis there were more voluntary (parental) and medical referrals from areas with compatible cultures, but more school and social agency referrals from areas with incompatible cultures. Along the structural axis there were larger proportions of voluntary (parental) and social agency referrals from areas with loose social structures, and more medical referrals from areas with tight social structures. There was no variation along this axis, however, in the proportion of school referrals. At this preliminary level of analysis, then, variation in the parental, medical, and social agency referrals appeared to be associated with both the cultural and structural axes. Variation in school referrals appeared to be associated only with the cultural axis. The two axes appeared to be independently associated both with the total number of cases appearing for examination at the clinic and with the processes involved in "defining" and referring children for psychiatric help.

If the ecological distribution of the cases examined at the clinic represents the diffusion of a social innovation rather than the incidence or prevalence of mental illness in the child population of the city, then the severity of the disorders exhibited by the examined children should be independent of types of communities constituting the paradigm. Preliminary single-classification analysis indicated that there was relatively little variation along either the cultural or the structural axis in the severity of the disorders exhibited by these children.

In summary, following a review of the literature on the diffusion of innovation, level of educational attainment (culture) and migration (social structure) were selected as variables for possible explanation of the distribution of the clinic's examined cases. Preliminary single-classification analysis indicated the presence of significant variation in the rates of examined cases among the city's community areas along both the cultural and the structural axes. Rates of examined cases were higher in community areas with compatible cultures (high educational attainments) as well as in community areas with loose social structures (high migration). The "definers" of mental illness in these children also varied with culture and with social structure. Most pertinent to any discussion of the acceptance of innovation, voluntary (parental) referrals appeared at the clinic more frequently from areas with either compatible cultures or loose social structures. Finally, there was relatively little variation in the severity of the disorders exhibited by these children that was associated with either culture or social structure.

To determine the extent to which the comparative model (see Table 2) predicted variation in rates of examined cases, "definers," and severity of children's disturbances, the following section of this paper presents the results of considering the interactive effect of culture and social structure. Rates of examined cases, sources of referral, and severity of disorder are compared among the four types of community areas composing the paradigm.

THE HYPOTHESES

The central proposition of this paper is that the distribution of cases examined at a child guidance clinic represents differential diffusion of a social innovation in which conduct is viewed in psychological or psychiatric terms, rather than the incidence or prevalence of mental disorders in the child population of the city in which the clinic is located. If the distribution of this clinic population is a consequence of the diffusion of social innovation, than three hypotheses following from the proposition can be tested:

1. *Rates of cases examined at the clinic will vary jointly with the culture and social structure of the community areas in which the children reside.*
2. *The "definers" of mental disorders requiring psychiatric attention in the child population of the city will vary jointly with the social structure and culture of the community areas in which the children reside.*
3. *Variation in the severity of the disorders exhibited by the children will be independent of the joint effect of the social structure and culture of the community areas in which the children reside.*

Before presenting the results of the tests of these hypotheses some detailed discussion of the variables to be introduced is required.

THE VARIABLES

The dependent variables are the examined-case rates, computed separately for each of the city's community areas; the "definer" or source of referral for each case; and the examining psychiatrist's evaluation of the severity of the disturbance exhibited by each child. For the independent variable, which for the sake of simplicity will be referred to as "community structure," a measure of educational attainment (culture) and migration (social structure) was required for each community area.

Rates of examined cases.—The data for this variable consist of materials abstracted and coded from the records of 3,017 children under eighteen years of age for whom applications for examination were completed between July 1, 1951, and July 1, 1958, and who were subsequently examined by the clinic's professional staff. At the time of application all the children were residents of the city. These children constituted the clinic's total psychiatric population with residences within the city limits for the period under study; another large population seen at the clinic, consisting of children living in the suburbs, was not included in the present study. The examined child population was allocated by place of residence to the city's seventy-five community areas and a rate of examined cases computed as follows for each community area. Since persons under eighteen years of age constituted the clinic's eligible population, the base for computing the rates was each area's average 1950–60 population of persons under eighteen years of age. The rates were converted to a common base of 10,000 persons under eighteen years of age. For the seventy-two community areas included in this last stage of the study the range of these rates was from 7.4 to 85.6. The mean rate was 28.4.

Source of referral.—Although greater refinement of the categories would have been preferable, the clinic's coding scheme was employed. It consisted of the following referral sources as indicated by the family of the child on the application: (1) parental (family, relatives, friends, etc.); (2) medical (without distinguishing persons referred by clinics from those referred by physicians in private practice); (3) social

agency (public or privately supported social agencies and the juvenile court); (4) school (teachers and other public or private school officials); (5) other (miscellaneous sources). The source of referral had been coded by the clinic for only 2,696 (89 per cent) of the cases. A check of the uncoded cases revealed that they were randomly distributed and thus their omission did not appear to introduce any bias. Voluntary (parental) referrals constituted 13.6 per cent of the cases; 39.7 per cent were medical referrals; 27.4 per cent were school referrals; and 17.3 per cent were social agency referrals. "Other" referral sources constituted only 2 per cent of the examined cases.

Severity of the child's disorder.—The examining psychiatrist's evaluation of the child, during the period under consideration, was independent of the evaluations already available and in the records of examining social workers and psychologists. The scale has three intervals: (1) not disturbed through mildly disturbed; (2) moderately disturbed; (3) severely disturbed. The reliability and validity of this scale have not been determined. However, for purposes of the present study it was considered preferable to diagnostic categories. Of the 2,696 children for whom information on source of referral had been coded, 12.4 per cent were considered not or mildly disturbed, 57.2 per cent moderately disturbed, and 29.2 per cent severely disturbed. For 13 per cent of the cases there was no information available on the degree of disturbance.

Culture.—Materials on educational attainment, employed as the index of community culture, were easily accessible through the records of the U.S. Census of Population and the *Local Community Fact Book for Chicago.*[35] The measure selected was the median number of school years completed by persons twenty-five years of age and older. Since the period under study covered a large part of the intercensal period, any population base or characteristic had to be adjusted to take into account possible changes in distribution that might have occurred in the city during the ten-year period. For this purpose the simplest adjustment, the average of the 1950 and 1960 Census reports, was assumed to be the most closely representative. The median number of school years completed (1950–60 average) among the seventy-two community areas included in the study was 9.6 years. Community areas with educational attainments below this median were viewed as having cultures incompatible with the acceptance of the definition of conduct in psychiatric terms. Community areas with scores of 9.6 years or higher on educational attainment were viewed as having cultures compatible with this innovation. On this basis thirty-seven of the community areas were classified as having compatible cultures and thirty-five areas as having incompatible cultures.

Social structure.—The measure of migration selected as an indicator of social structure was the proportion of an area's occupied dwelling units in 1960 into which the residents had moved prior to 1953.[36] The seventy-two community areas were

[35] See U.S. Bureau of the Census, *U.S. Census of Population and Housing: 1960* (Washington, D.C.: Government Printing Office, 1962), Final Report PHC (1)-26 for Chicago, Ill.; and Hauser and Kitagawa, *op. cit.* Special tabulations by community areas made available through the courtesy of the Community Renewal Program, City of Chicago, Table P-1, "General Characteristics of the Population, by Census Tracts: 1960."

[36] U.S. Bureau of the Census, *op. cit.*, Table H-2. Special tabulations by community areas were made available through the Community Renewal Program, City of Chicago. Since this measure largely covered the intercensal period it could be employed directly from the 1960 Census reports without adjustments. Following Freedman's findings about migration into Chicago, no attempt was made to distinguish the numerous migration streams comprehended by this measure. Perhaps most crucial for this study was Freedman's finding that migrants in most streams to Chicago could be characterized as having minimal family ties. The exceptions were suburban, rural-farm male and southern migrants (Ronald Freedman, "Migration Differentials in the City as a Whole," in *Reader in Urban Sociology*, ed. Paul K. Hatt and Albert J. Reiss, Jr. [Glencoe, Ill.: Free Press, 1951], pp. 205–19. The comprehensive measure of migration (or mobility) employed

dichotomized at the median percentage (45.3) of residents who had moved in prior to 1953. The thirty-seven areas with scores at or above this median were viewed as having characteristics associated with tight social structures, the remaining thirty-five areas as having characteristics associated with loose social structures.

On the basis of their separate scores on culture (education) and social structure (migration), the seventy-two areas were divided into four groups:

1. Tight-incompatible communities (19)
2. Loose-incompatible communities (16)
3. Loose-compatible communities (19)
4. Tight-compatible communities (18)

Having discussed the variables in some detail, the following sections can proceed to present the results of the tests of the three hypotheses.

HYPOTHESIS 1

The first hypothesis states that the rates of cases of children examined at the clinic will vary jointly with the culture and social structure of the community areas in which the children reside. Table 3 shows that this hypothesis is confirmed. As predicted by the paradigm, the rates of examined cases were lowest in the tight-incompatible community areas. At the opposite extreme and as predicted, the rates were highest in the loose-compatible community areas. Just as the rates in the tight-incompatible community areas were distinctly lower than those in any other type of area, rates in loose-compatible community areas were distinctly higher than those in other types of areas. As predicted, the examined case rates in the loose-incompatible and tight-compatible community areas ranked intermediately between the low rates of the tight-incompatible areas and the high rates of the loose-compatible areas.

Because of the racially segregated pattern of residence for many of the community areas it was not possible to compare the distributions of Negro and white case rates except by combining populations and cases, by race, within each of the four types of areas. Even here Negro and white rates were not exactly comparable since the base for the Negro examined cases referred to the total non-white child population rather than to the Negro child population. Nevertheless, the pattern of variation of examined-case rates among the four types of community areas appeared to hold for Negro and for white children (Table 3).

There was greater variation in rates along the social structural (migration) axis than along the cultural (education) axis. Holding social structure constant, among community areas with tight social structures rates for areas with compatible and incompatible cultures did not differ significantly. Among community areas with loose social structures the difference in rates between areas with compatible and incompatible cultures again was not large. But when culture rather than social structure was held constant, among community areas with either compatible or incompatible cultures those areas with loose social structures had significantly higher examined-case rates than those with tight social structures. It thus appeared possible that some of the association between education and informed mental health attitudes reported in the literature in turn reflected the greater propensity to migrate which also has been noted as common to persons of high educational attainment.

The distribution of the examined-case rates approximated the rank order predicted by the comparative model. Type of social structure and culture when considered jointly did appear to provide a plausible explanation of the differences observed among rates of cases of children appearing

here may be viewed alternatively as a measure of tenure. However, a measure of tenure alone would not have taken into account the presence of non-home-owning long-term residents in the community areas. The present measure, of course, is highly correlated with tenure (1950–60: $r = 0.736$, $p < .01$) Also, both tenure and the present measure of permanence of residence correlated inversely with migration to the city from outside of the Standard Metropolitan Statistical Area since 1955 ($r = -0.736$ and $r = -0.638$).

at the clinic for examinations from widely differing community areas of the city. Community areas with tight social structures (low migration) and incompatible cultures (low education) contributed significantly fewer cases to the clinic, and those with loose social structures and compatible cultures contributed the largest proportion.

HYPOTHESIS 2

As a second hypothesis it was proposed that the "definers" of mental disorders among children would vary jointly with the social structure and culture of the areas in which the children resided. The χ^2 test indicated that the "definers" (source of referral) did vary with the structure and culture of the community areas (Table 4). A notably smaller proportion of the children appeared as voluntary (parental) referrals from the tight-incompatible community areas (those with lowest examined-case rates). The largest proportions of

TABLE 3

EXAMINED-CASE RATES, BY COMMUNITY STRUCTURE AND CULTURE*

Tight Incompatible		Loose Incompatible		Loose Compatible		Tight Compatible	
Rank in Total	Rate	Rank in Total	Rate	Rank in Total	Rate	Rank in Total	Rate
2	7.7	21	20.4	5	9.1	1	7.4
3.5	9.0	23	20.9	25	21.4	3.5	9.0
6	9.2	27	22.2	28	22.3	10	12.6
7	10.7	32	23.2	30	22.8	12	13.5
8	11.0	36	25.8	38	26.5	13.5	14.7
9	12.5	37	26.4	39.5	27.6	16	18.1
11	12.9	39.5	27.6	41	28.2	17.5	18.8
13.5	14.7	42	28.4	46	30.6	19	18.9
15	18.0	43	29.4	51	31.6	20	20.0
17.5	18.8	44	29.5	53	32.2	26	21.8
22	20.5	45	30.4	55	33.4	33	23.9
24	21.1	47	30.7	61	36.1	48	31.0
29	22.7	56	34.6	63	37.8	49	31.1
31	22.9	57	34.8	65	44.6	50	31.2
34	24.4	58	35.0	66	50.2	52	31.9
35	25.1	60	35.4	68	54.4	59	35.3
54	32.7			69	66.0	67	51.8
62	37.6			71	85.3	70	74.7
64	38.1			72	85.6		
Mean							
23.6	19.5	41.7	28.4	50.1	39.2	31.6	25.9
Mean Rate, White (W) and Negro (N)†							
....	21.1 (W) 6.6 (N)	....	31.0 (W) 27.8 (N)	....	42.2 (W) 27.3 (N)	....	24.3 (W) 33.9 (N)

* Rates are per 10,000 persons under 18 years of age. Because of the asymmetry of the distributions, non-parametric tests, estimates of variance, were substituted for single classification parametric analysis of variance and t-tests. All tests are two-tailed. H is the symbol for Wallis and Kruskal's test here involving four samples. The symbol z is the standard score for the Mann-Whitney U test employed here when only two samples are involved. (Helen M. Walker and Joseph Lev, *Statistical Inference* [New York: Rinehart & Winston, 1953], pp. 434–38).

† Computed by combining examined cases and population base for all of the community areas within each of the four types constituting the paradigm. The base population for the Negro rates is the non-white population under 18 years of age.

$H = 20.4$; degrees of freedom $= 3$, $p < .005$

z_{1+2} vs. $z_{3+4} = 1.92$, $p < .05$ z_1 vs. $z_2 = 3.01$, $p < .002$ z_2 vs. $z_3 = 1.67$, $p \cong .10$

z_{1+4} vs. $z_{2+3} = 3.86$, $p < .005$ z_1 vs. $z_3 = 3.52$, $p < .002$ z_3 vs. $z_4 = 2.49$, $p < .012$

z_1 vs. $z_4 = 1.05$, $p > .317$ z_2 vs. $z_4 = 1.48$, $p > .134$

voluntary referrals came from the areas with compatible cultures (high educational attainment). However, among the loose-incompatible areas there was a marked shift away from the proportion of voluntary referrals associated with tight-incompatible areas toward similarity with the proportions of voluntary referrals noted for areas with compatible cultures (high education). Larger proportions of voluntary referrals clearly were associated independently with compatible cultures (high education) and with loose social structures (high migration).

School and social agency referrals, which may be viewed as somewhat less voluntary, appeared to be more closely associated with culture than with social structure. Larger proportions of these referrals came from areas with incompatible cultures. If diffusion of innovation is strictly defined as the *acceptance* of a value, belief, or practice, then the criterion of acceptance is necessarily the presence of voluntary action rather than that which may be described as involuntary. Viewed from this perspective, it seems clear that diffusion of the psychiatric view of conduct into community areas of the city with incompatible cultures (low education) appears notably when the social structure is loose and only minimally when the social structure is tight.

Because of very small numbers of examined Negro cases in community areas with tight social structures no attempt was made to compare the "defining" process for Negro and white children within the more elaborate framework of the interaction between culture and social structure. However, Negro cases were sufficient in number to permit comparisons separately along the cultural and structural axis. The "definers" of mental illness for Negro and white children differed in community areas with compatible cultures (high education), in those with incompatible cultures (low education), and in those with loose social structures (high migration). But the "definers" for Negro and white children did not differ in community areas with tight social structures. If one may take the liberty of including medical referrals with parental referrals as among those that are voluntary, while school and social agency referrals are viewed as somewhat involuntary, then in each instance larger proportions of white children came to the clinic for examinations as voluntary referrals, while larger proportions of Negro children came to the clinic as involuntary referrals (Table 5). The observed difference in the "defining" process for Negro and white

TABLE 4

"DEFINERS" OF CHILDHOOD MENTAL ILLNESS BY COMMUNITY CULTURE AND SOCIAL STRUCTURE

(Per Cent)

Definers	Tight Incompatible	Loose Incompatible	Loose Compatible	Tight Compatible	Total
Voluntary (parental)	6.1	12.5	15.8	16.2	13.5
Medical	42.1	34.5	42.0	44.8	39.9
School	29.5	32.1	23.4	25.5	27.4
Social agency	19.6	19.5	16.3	11.7	17.2
Other	2.7	1.4	2.5	1.8	2.0
Total	100.0	100.0	100.0	100.0	100.0
N	(342)	(910)	(1,037)	(377)	(2,666)*

* Reduced from 2,696 when number of community areas was reduced from 75 to 72.

$\chi^2 = 60.3$; degrees of freedom = 12; $p < .005$

children does not imply that the diffusion (acceptance) of the psychiatric view of conduct differs in the two population subgroups. Independently of the magnitude of Negro-white differentials in proportions of voluntary and involuntary referrals, more Negro children came to the clinic as voluntary referrals from areas with compatible cultures (47.5 per cent) than from areas with incompatible cultures (40.9 per cent). But a larger proportion of the Negro children came to the clinic as voluntary referrals from areas with tight social structures (51.2 per cent) than from areas with loose social structures (42.1 per cent). However, of the forty-one cases of Negro children coming to the clinic from areas with tight social structures, thirty-one came from areas with compatible cultures. Thus for Negro children differences in proportion of voluntary referrals on the social structural axis appeared to reflect high levels of educational attainment in the tightly structured areas from which these Negro children came. However, it was clear that consistently larger proportions of Negro than of white children came to the clinic as involuntary referrals.[37]

[37] The differences in "definers" of mental illness among Negro and white children are considered in another paper currently in preparation.

TABLE 5

COMPARISONS: DEFINERS OF MENTAL ILLNESS FOR NEGRO AND WHITE CHILDREN

Source of Referral*	Incompatible Culture		Compatible Culture		Tight Social Structure		Loose Social Structure	
	White	Negro	White	Negro	White	Negro	White	Negro
Voluntary........	51.9	40.9	61.7	47.5	56.5	51.2	58.4	42.1
Involuntary......	48.1	59.1	38.3	52.5	43.5	48.8	41.6	57.9
Total..........	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
N.............	(813)	(418)	(1,224)	(158)	(662)	(41)	(1,375)	(535)

* Voluntary includes parental and medical referrals. Involuntary includes school and social agency referrals. "Other" referrals were excluded from this analysis.

χ^2 incompatible = 13.4, degrees of freedom = 1, $p < .005$
χ^2 compatible = 11.8, degrees of freedom = 1, $p < .005$
χ^2 tight = 0.4, degrees of freedom = 1, $p > .05$
χ^2 loose = 41.3, degrees of freedom = 1, $p < .005$

HYPOTHESIS 3

The third hypothesis stated that variation in the severity of the psychiatric disorders exhibited by these children would be independent of the joint effect of the social structure and culture of the community areas in which they resided. As indicated by the χ^2 test for Table 6, there was relatively little variation in the degree of disturbance among these children that was associated with the type of social structure and culture of the community areas in which they resided.

Although among the four types of areas differences in the severity of disturbance exhibited by these children were small, the direction of these differences was consistent with the hypothesis that the distribution of cases represented a consequence of differential diffusion of an innovation. Combining the not or mildly disturbed and the moderately disturbed classes into a single category of children who were not severely disturbed, in Table 6 within the loose-compatible areas the proportion of children who are not severely disturbed (72.3 per cent) was higher than the proportion observed within each of the three other types of areas. Consequently, the smallest proportion of severely disturbed children (26.5 per cent) also was noted within loose-com-

patible areas. Within tight-incompatible areas, those which had exhibited the lowest total examined case rates (see Table 3), the proportion of not severely disturbed children (66.4 per cent) was lower and the proportion of severely disturbed children was higher (32.5 per cent) when compared with the proportions within each of the other types of areas. Finally, corresponding with intermediate ranks on total examined case rates the proportions of not severely disturbed and of severely disturbed children within loose-incompatible and tight-compatible areas again ranked intermediately. In other words, it appeared possible that resistance to the interpretation of conduct in psychiatric terms might be accompanied by some restriction of the designation "mental illness" to specifically and clearly aberrant conduct, while acceptance of the innovation was accompanied by the designation of mental illness to a wider range of behavior problems.

Comparisons of the degree of disturbance exhibited by Negro and white children with social structure and culture considered separately indicated that, unlike the sources of referral, there was relatively little difference in the degree of disturbance between Negro and white children along either axis.

TABLE 6

SEVERITY OF CHILD'S DISTURBANCE, BY COMMUNITY STRUCTURE AND CULTURE

Severity of Disturbance	Tight Incompatible	Loose Incompatible	Loose Compatible	Tight Compatible	Total
Not or mildly	12.3	11.0	13.1	13.8	12.4
Moderately	54.1	57.6	59.2	54.1	57.3
Severely	32.5	30.0	26.5	30.5	29.0
Undetermined	1.1	1.4	1.2	1.6	1.3
Total	100.0	100.0	100.0	100.0	100.0
N	(342)	(910)	(1,037)	(377)	(2,666)

$\chi^2 = 9.22$, degrees of freedom $= 9$, $p > .25$

SUMMARY

Current differential use being made of the examination services offered by a psychiatric out-patient clinic serving children and located in a large city may plausibly be interpreted as representing a consequence of diffusion of a social innovation rather than the incidence or prevalence of mental illness in the city's child population. The social innovation is the interpretation of conduct in psychological and psychiatric terms. It was predicted that differential community permeability to this innovation occurred along two independent but intersecting axes: the structural and the cultural. The structural axis referred to looseness or tightness of community networks of interpersonal relations. The cultural axis referred to the compatibility or incompatibility of psychological and psychiatric interpretations of conduct with prevailing community orientations. The structural axis was measured by migration, while the cultural axis was measured by level of educational attainment. It was hypothesized that if the use being made of a large, free, and publicly supported psychiatric clinic represented a consequence of diffusion of the particular innovation under consideration, then high examined-case rates should appear in areas of the city which were permeable to innovation: areas with compatible cultures and either loose or tight social structures, and areas with incompatible cultures but loose social structures. On the other hand, low examined-case rates should appear in areas with incompatible cultures but tight social structures.

The examined-case rates did vary with the culture and social structure of the different areas of the city in the manner predicted. The highest examined-case rates appeared in areas with compatible cultures and loose social structures. The lowest examined-case rates appeared in areas with incompatible cultures and tight social structures. The examined-case rates also were high in areas with compatible cultures and tight social structures, and in areas with incompatible cultures and loose social structures. Voluntary referrals, reflecting acceptance of innovation, were higher in those areas where the total examined-case rates were high and lower in those areas where the total examined-cases rates were low. Finally, the severity of the disorders exhibited by the examined persons appeared to be largely independent of the culture and social structure of the areas of the city in which they resided. Lay professional followers of the psychiatric innovation, such as teachers and social workers, serve as "definers" of mental illness more frequently in areas with incompatible cultures than in areas with compatible cultures. Thus, lay professionals appear to serve as the mediators or bridging mechanism for diffusion of the psychological and psychiatric interpretation of conduct into areas with cultures which otherwise are incompatible with acceptance of this innovation.

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ASCETIC PROTESTANTISM AND POLITICAL PREFERENCE IN THE DEEP SOUTH[1]

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ABSTRACT

Using survey data from a Pacific Northwest city, the author demonstrated in a previous paper that even when occupational class is controlled, Protestants who attend fundamentalist churches are more likely to be Republicans than are Protestants who attend liberal churches. The present paper reports an attempt to replicate these findings in a sample from a city in the Deep South. The southern findings duplicate the previously reported relationships in almost every respect. Owing, however, to differences in the distribution of liberalism and fundamentalism in these two cities, the over-all political consequences of these relationships are probably quite distinct in the two cities and in their surrounding regions.

In an earlier paper[2] the author advanced the hypothesis that religious influences emanating from the tradition known as ascetic Protestantism[3] have an effect on individuals' political opinions and behavior. It was argued that the theological and social doctrinal disagreements involved in the long-standing division of ascetic Protestantism into the liberal and the fundamentalist factions parallel to a striking extent the value orientational disagreements reflected in the political division between Democrats and Republicans.

It was specifically argued that fundamentalism, representing an older orthodoxy that embodied those orientations that Max Weber has called the "Protestant ethic," has tended toward value consonance with conservative, or Republican, political views. Liberalism, on the other hand, represents a positive adaptation to recent trends in science and secular thought. It has been strongly identified with the social gospel movement and with political liberalism in general. Liberal Protestantism should therefore be relatively well disposed toward the Democratic party.

For these reasons it was predicted that among laymen exposed to fundamentalist teachings religious involvement would vary directly with Republican political preference, whereas among laymen exposed to liberal teachings religious involvement would vary inversely with Republican political preference. It was also predicted that when religiously involved liberals and fundamentalists were compared, fundamentalists would be more inclined toward Republicanism than would liberals. Finally, it was predicted that these relationships would hold up when occupational class was controlled.

These hypotheses were tested by means of interview data gathered in a community of 50,000 in the Pacific Northwest. In every case the predictions were borne out. Despite the fact that class differences were clearly discernible, the tables suggested that religious factors did contribute inde-

[1] This investigation was carried out with the aid of funds granted under National Institute of Mental Health Research Grant M-4309 (R1). The present paper is a revised version of a paper read before the annual meeting of the Pacific Sociological Association in Sacramento, California, April, 1962.

[2] Benton Johnson, "Ascetic Protestantism and Political Preference," *Public Opinion Quarterly*, XXVI (Spring, 1962), 35–46.

[3] Both Max Weber and Ernst Troeltsch used the term "ascetic Protestantism" to refer to the nonliturgical branches of Protestantism. The most prominent of these branches are the Calvinist, the pietist, and the revivalist traditions. In terms of Weber's systematic typology of religions, ascetic Protestantism represents one of the closest historical approximations to the ideal-typical religion of ethical virtuosos serving a single supramundane deity (see Max Weber, "The Social Psychology of the World Religions," in Hans Gerth and C. Wright Mills [eds.], *From Max Weber: Essays in Sociology* [New York: Oxford University Press, 1946], pp. 273 ff.).

pendently to political preference.[4] On the basis of these findings it was argued that a probable consequence of ascetic Protestant influences was the muting or mitigating of class-based political differences among those who are religiously involved. This assertion appeared plausible in light of the fact that the relationship of liberalism and fundamentalism to the class system is almost the reverse of what would be expected if these theological factions acted primarily to lend religious legitimacy to the class-based political propensities of the majority of their supporters. In fact, however, liberalism is predominantly the religion of the middle and upper classes, whereas fundamentalism is predominantly the religion of the working class. Our data showed this peculiar relationship of liberalism and fundamentalism to the class system quite clearly. A substantial majority of our religiously involved respondents were probably being systematically exposed to politically relevant value orientations at odds with the dominant political outlooks of the classes to which they belong. Class-based political differences among this large group of respondents were far less than among the minority of religiously involved respondents whose class interests were presumably being regularly reinforced by religious values.

In that research report the author took note of the prevailing sociological tendency to discount the influence of specifically religious factors on political attitudes and behavior. Political sociologists have long been aware that Protestants are more inclined toward Republicanism than Catholics are, but they have not in general pursued the question of whether religious influences might in part produce this relationship. Even sociologists of religion have been more disposed to focus on the secular impact on religion than on the religious impact on the secular society.[5] The finding of a clear relationship between religious involvement and political preference is therefore likely to call into question some of the interpretations that have ordinarily been made concerning the relationship between Protestantism and other features of American life. For this reason the question of whether the relationships reported in the previous paper can be replicated is bound to be raised. Are these relationships a local phenomenon, perhaps even due to sampling error, or do they hold true over a large area, perhaps throughout the United States?

It has been possible to undertake a replication of the earlier study in a city located in the Deep South. Although the research site was not chosen for the express purpose of retesting our hypotheses, it has several distinct advantages as a locale in which to do so. The South has been cited by sociologists and non-sociologists alike as possessing a cultural tradition that sets it apart from all the other regions of the country. Although it is predominantly ascetic Protestant in its religious tradition, the three denominations that together account for more than half of the entire church-going population of most southern states were historically separated from their northern counterparts and organized into autonomous regional bodies. This long-standing separation, though ended in the case of the Methodists in 1939, persists for both Presbyterians and Baptists. Moreover, despite its Protestant heritage, the South failed to industrialize as early

[4] It should be pointed out that neither the design of this previous research nor that of the research with which this paper is concerned permits conclusions to be made as to the cause of the observed relationships. These relationships strongly suggest, but do not prove, that religion should be regarded in these instances as an independent variable.

[5] The best recent presentation of this viewpoint is to be found in Peter L. Berger's *The Noise of Solemn Assemblies* (Garden City, N.Y.: Doubleday & Co., 1961), pp. 17–104. Other recent work has questioned this perspective by presenting findings suggesting that value orientations associated with religion do independently affect secular behavior. See esp. Gerhard Lenski, *The Religious Factor* (Garden City, N.Y.: Doubleday & Co., 1961). See also Albert J. Mayer and Harry Sharp, "Religious Preference and Worldly Success," *American Sociological Review*, XXVII (April, 1962), 218–27.

as other less unequivocally Protestant parts of the country. Finally, the South has the peculiar political tradition of Democratic party uniformity. Surely it would be difficult to find a locale within the United States where the odds against confirmation of our original findings would be greater. On the other hand, should the earlier findings be reproduced in a southern sample, the grounds for considering them to have generalized validity would be extremely firm.

PROCEDURE

The data to be presented were collected by means of face-to-face interviews with an area-probability sample of 471 white male members of the labor force in Tallahassee, Florida. In terms of population size, approximate rate of recent growth, and predominance of the ascetic Protestant religious tradition, this city is comparable to Eugene, Oregon, the community from which the data reported in the previous paper were drawn. It differs from Eugene primarily in characteristics of a "regional" nature. It should be pointed out that, although Florida as a whole is no longer a characteristically southern state, the north Florida area in which Tallahassee is located remains culturally a part of the Deep South. The interviews were carried out during the summer of 1961.

Since the present aspect of the Tallahassee research was designed as a replication of the previous research, every effort was made to construct indexes similar to those used in Eugene. Church attendance was again used as the index of religious involvement. The category "attends frequently" includes those who reported that they had attended church more than once a month during the preceding year. The category "attends seldom" includes all others who reported that they had attended church at least once during that time. The same breakdown had been used in Eugene. Each respondent in Tallahassee was asked to name the particular church he ordinarily attended. These churches were in turn classified as liberal or fundamentalist on the basis of the theological self-rating of their principal pastors.[6] The classifications that resulted are shown in Table 1.[7] Following Weber's limitation of ascetic Protestantism to denominations growing out of the Calvinist, pietist, and revivalist traditions, Lutherans and Episcopalians were again excluded from analysis.

Following the procedure used in the previous research, two measures were constructed for the dependent variable, political preference. The party identification

[6] Since we wished to establish a link, if possible, between the behavior of the laity and the ideational perspective of religious officials, it seemed wise to classify the various congregations in the community on the basis of the theological outlook of their pastors. In Eugene this classification was arrived at on the basis of a series of relatively unstructured interviews with a sample of pastors from the community. A more refined procedure was followed in Tallahassee. Every pastor of a white or predominantly white church in the city who was primarily responsible for preaching was asked to place himself in one of the following theological categories: liberal, neoliberal, neo-orthodox, orthodox, conservative, fundamentalist. The first three categories were collapsed to form the liberal category and the last three were collapsed to form the fundamentalist category. In view of the replicative aim of the present study and the small number of cases available for analysis, a twofold classification appeared advisable despite the loss of finer theological distinctions that such a procedure entails. The neo-orthodox were classed with the liberals chiefly on the basis of the left-wing political reputation of their leading spokesmen in the United States. In a subsequent survey of clergymen in the state of Oregon the validity of this classification has been supported by the finding that the political preferences of those who report that they are neo-orthodox in theology are far closer to the preferences of those who report that they are liberal than they are to the preferences of those who report that they are conservative or fundamentalist. The neo-orthodox were, in fact, a little more likely than the liberals to report that they had tended to vote for Democratic candidates during the past five years (unpublished research report available from the author on request).

[7] The distribution of pastors' responses for the six theological categories was as follows: liberal, 4; neoliberal, 0; neo-orthodox, 2; orthodox, 5; conservative, 18; fundamentalist, 20. Methodist, Presbyterian, and Disciples of Christ pastors were divided in their views even when the above six categories were collapsed into two.

index is based on the respondent's stated party affiliation.[8] Respondents who claimed no affiliation or listed themselves an Independents were excluded from analysis. The second index, voting behavior, is based on respondents' reported voting in the 1956 and 1960 presidential elections[9] and the 1960 gubernatorial election. With the exception of the 1956 presidential election, the elections used to construct the index of voting behavior in Tallahassee were different from those used to construct this index in Eugene. The methods used were,

TABLE 1*

Liberal	Fundamentalist
Methodist (4 congregations)	Baptist
Presbyterian (1 congregation)	Methodist (3 congregations)
Disciples of Christ (1 congregation)	Presbyterian (5 congregations)
	Church of Christ
	Church of the Nazarene
	Salvation Army
	Latter-day Saints
	Seventh-Day Adventist
	Disciples of Christ (1 congregation)
	All Pentecostal bodies

* The number of congregations of each of these three denominations that were classified as liberal or fundamentalist is indicated in parentheses. All congregations of all the remaining denominations were classified as fundamentalist.

however, identical.[10] Individuals who had not voted in any of the three elections or whose voting had been evenly split between the two parties were excluded from analysis.

Occupational class was again measured by means of the Census occupational classification. A simple white-collar–blue-collar breakdown was derived by collapsing these categories. In order to rule out the possibility that the results might be seriously influenced by the responses of persons of non-southern origin, all respondents who had not spent most of their childhood in the South were excluded from analysis.

FINDINGS

Table 2 presents the political preference of liberals and fundamentalists by frequency of church attendance. When voting behavior is used as the index of

[8] As in the earlier study the actual question posed to respondents was identical to that used in the voting studies of the Survey Research Center, University of Michigan (see Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* [New York: John Wiley & Sons, 1960], p. 122.

[9] The fact that 1960 presidential voting was used in constructing the index of voting behavior raises the possibility that the results presented below may reflect, especially in the case of the fundamentalists, a specific anti-Catholic bias rather than a generalized conservatism. But the method of constructing this index (see n. 10) serves to minimize the effect of such specific, one-time influences. For the vast majority of our respondents at least two reported Republican votes are necessary in order to classify one as a Republican. In other words, the politically liberal or moderate anti-Catholic who voted for Nixon but whose voting record is otherwise Democratic will be classified as a Democrat. As a matter of fact, our data suggest that the "Catholic issue"probably had little independent effect on Protestant voting in Tallahassee in 1960. Inspection of the relation between religion and voting in each election for which data are available reveals very little fluctuation. Interestingly enough, a higher proportion of our respondents voted for Eisenhower in 1956 than voted for Nixon in 1960. The use in a white southern sample of recent presidential voting in constructing our index of voting behavior also raises the possibility that Republican voting may primarily reflect a disenchantment with the Democratic party's stand on racial issues rather than a more general conservatism. Fortunately, Russell Middleton's independent survey of white and Negro voting in Tallahassee in the 1960 presidential election has shown that the race issue played a very small part in influencing the voting decisions of whites ("The Civil Rights Issue and Presidential Voting among Southern Negroes and Whites," *Social Forces*, XL [March, 1962], 209–15).

[10] The voting behavior index was constructed in the following manner: (1) respondents who had voted in all three elections were classified as preferring the party for whose candidates they had voted at least twice; (2) respondents who had voted in only two elections were excluded from analysis if their voting had been split between the two parties, but were otherwise classified according to the party for which they had voted both times; (3) respondents who had voted in only one election were classified as preferring the party for which they had voted in that election. The temporal sequence of respondents' party choices was not taken into account in constructing the index.

political preference, the expected tendency of Republican preference to vary inversely with frequency of attendance among liberal church-goers but directly with frequency of attendance among fundamentalist church-goers clearly emerges. Moreover, the prediction that frequently attending fundamentalists will report a higher incidence of Republican preference than will frequently attending liberals is also borne out. These relationships are identical to those found when a similar analysis was made of the Eugene data.[11]

Table 3 introduces a control for occupational class.[12] When political preference is measured by the index of voting behavior, the direction of the relationships in all cases for which predictions have

[11] Johnson, *op. cit.*, p. 43, Table 4.

[12] It was considered desirable to control for the effects of class because of the demonstrated salience of status factors in determining political preference. Unfortunately, the small number of cases available for analysis in both of the surveys makes it impossible to control simultaneously on a number of status variables. Occupation was selected as the single control variable because it is generally re-

TABLE 2

REPUBLICAN PREFERENCE OF LIBERALS AND FUNDAMENTALISTS BY FREQUENCY OF ATTENDANCE

(Per Cent)

	Liberals		Fundamentalists	
	Attend Frequently	Attend Seldom	Attend Frequently	Attend Seldom
Party identification:				
Per cent	3	0	14	9
N	(30)	(12)	(127)	(44)
Voting behavior:				
Per cent	23	45	44	29
N	(26)	(11)	(106)	(38)

TABLE 3

REPUBLICAN PREFERENCE OF LIBERALS AND FUNDAMENTALISTS, WITH FREQUENCY OF ATTENDANCE AND OCCUPATIONAL CLASS CONTROLLED*

(Per Cent)

	Liberal				Fundamentalist			
	White Collar		Blue Collar		White Collar		Blue Collar	
	Attend Frequently *A*	Attend Seldom *B*	Attend Frequently *C*	Attend Seldom *D*	Attend Frequently *E*	Attend Seldom *F*	Attend Frequently *G*	Attend Seldom *H*
Party identification:								
Per cent	0	0	13	....	15	10	12	7
N	(22)	(11)	(8)	....	(101)	(29)	(26)	(15)
Voting behavior:								
Per cent	26	45	14	....	49	37	21	9
N	(19)	(11)	(7)	....	(87)	(27)	(19)	(11)

* Hypotheses: (1) *A* is less than *B*; (2) *E* is greater than *F*; (3) *G* is greater than *H*; (4) *A* is less than *E*; (5) *C* is less than *G*; (6) *C* is less than *D* (no data available). Hypotheses (2), (3), and (4) are supported regardless of the index of political preference used (note: two of the hypotheses are supported only when voting behavior is used as the index of political preference).

been made and data are available is exactly the same as reported in a similar table of the previous paper.[18] Apparently, then, the two factions of ascetic Protestantism have quite similar bearings on political proclivities in these two widely separated and historically distinct regions of the country. The Tallahassee data show that, as in Eugene, occupational class is related to political preference. But despite this, religious involvement and liberalism-fundamentalism continue to show a strong relationship to political preference.

It should be pointed out that in the case of the Tallahassee data a few of the relationships just summarized are not reproduced when political preference is measured by the index of party identification. In Eugene, on the other hand, the relationships did not vary with the index of political preference used. The author is inclined to believe that in the South, with its strong tradition of Democratic party uniformity, the individual's party identification is less likely than his actual voting behavior to reflect the contemporary ideological orientations with which we are concerned and which we believe to be influenced to some extent by religion.

IMPLICATIONS: THE "MUTING EFFECT" RECONSIDERED

When voting behavior is observed, Table 3 reproduces in every respect the pattern of relationships observed in the Eugene data between occupational class, theological faction, frequency of church attendance, and the dependent variable. We now have firmer grounds for supposing these relationships to be widely prevalent.

As noted above, it was argued in the previous paper that one consequence of the findings just summarized might well be the reduction or muting of political differences attributable to class. It is reasonable to assume that persons of blue-collar occupational status are constrained toward Democratic voting and that persons of white-collar occupational status are constrained toward Republican voting. It now appears plausible to argue that those who are religiously involved in liberal churches are constrained toward Democratic voting and that those who are religiously involved in fundamentalist churches are constrained toward Republican voting. Taking into account only these two sources of constraint, it is possible to divide our frequently attending voters into two categories: those subjected to politically relevant cross-pressures, and those not so subjected. The cross-pressured are blue-collar fundamentalists and white-collar liberals, those not cross-pressured being white-collar fundamentalists and blue-collars liberals.

If we look at the voting behavior of these two categories of respondents as reported in Table 3, it is immediately apparent that the difference in proportion Republican between white- and blue-collar categories is very much smaller for those who are cross-pressured than it is for those who are not. Specifically, among frequently attending respondents, the difference between blue-collar fundamentalists and white-collar liberals amounts to 5 per cent, whereas the difference between blue-collar liberals and white-collar fundamentalists amounts to 35 per cent.

Table 4 summarizes the differences in the proportion Republican (based on the index of voting behavior) between frequently attending white- and blue-collar respondents for each of the two cross-pressure categories. Data from both the

garded as the one best indicator of social status and because it seems to reflect best the factor of material self-interest that has been a historically important basis for the differentiation of liberal and conservative political perspectives. The relationships reported in Table 2 are not materially affected when education, rather than occupational class, is introduced as a single control. Although it is possible that the observed relationships are spurious owing to the operation of an uncontrolled factor, we are inclined to think this unlikely in view of the known political relevance of the theological outlooks in question and the failure of the findings to be shaken by the factor of occupational class.

[18] Johnson, op. cit., p. 44, Table 3.

Eugene and the Tallahassee surveys are included in this table. As the figures indicate, there is a remarkable similarity between the two cities. It is safe to assume, then, that the muting effect is a very real consequence of the politically relevant cross-pressures to which many ascetic Protestant laymen seem to be subjected.

But the demonstration that in both communities class-based political differences are muted among cross-pressured ascetic Protestants does not mean that the larger effects of this phenomenon are similar in the two cities. An inspection of the marginals of Table 4 is sufficient to forestall any such conclusion. In Eugene the forty-three respondents who were cross-pressured constituted nearly two-thirds (63 per cent) of all frequently attending ascetic Protestant voters in our sample. Hence the muting effect documented in Table 4 is probably the major political consequence of the influence of the two factions of ascetic Protestantism among the religiously involved in that city. This is a direct result of the peculiar relationship of these two factions to the class system. This situation obtains in Eugene and is probably the situation ordinarily obtaining in other cities of its size or larger throughout the northern and western states. Wherever it does obtain the muting effect will almost certainly predominate. It does not predominate in Tallahassee, however, for in that city only 29 per cent of all frequently attending ascetic Protestant voters are cross-pressured. This striking departure from the similarity of patterns hitherto observed between the two cities reflects a relationship of liberalism and fundamentalism to the class system that is different from the classical one just described. As in Eugene, the majority of blue-collar respondents of Tallahassee attend fundamentalist churches. Liberal churches in Tallahassee are if anything even more exclusively white collar than their Eugene counterparts. What is different about Tallahassee is the fact that a large majority —82 per cent, in fact—of frequently attending white-collar voters go to fundamentalist churches. It is this fact that is largely responsible for the high proportion of churchmen in that city who are not cross-pressured in our sense of the term.

These are good reasons for assuming that this situation is not a localized phenomenon but is generally characteristic of the southern states. Unlike the North and West, where the older Protestant orthodoxy is nowadays carried on chiefly by newer, relatively low-status denominations, in the South two very influential old-line denominations, the Baptists and the Presbyterians, continue to be largely under conservative leadership. As the list of denominations classified earlier as liberal or fundamentalist shows, all Baptist churches in Tallahassee and all but one Presbyterian church are fundamentalist. Moreover, 80 per cent of all the frequently attending white-collar fundamentalist voters in our sample go to one of these churches. No doubt the persistance of orthodoxy in these two denominations is in large part a consequence of the fact that both these bodies, alone of all the major religious denominations represented in the South, remain organizationally separated from their coreligionists in other regions. As a result, they have remained isolated from the ferment that has taken place in the major intellectual

TABLE 4

PERCENTAGE DIFFERENCES IN REPUBLICAN POLITICAL PREFERENCE BETWEEN FREQUENTLY ATTENDING WHITE-COLLAR AND BLUE-COLLAR ASCETIC PROTESTANTS, BY PRESENCE OR ABSENCE OF CROSS-PRESSURES

(Based on Index of Voting Behavior)

	Cross-pressured*	Not Cross-pressured†
Eugene:		
Percentage difference...	9	33
N.....................	(43)	(25)
Tallahassee:		
Percentage difference...	5	35
N.....................	(38)	(94)

* White-collar liberals and blue-collar fundamentalists.
† White-collar fundamentalists and blue-collar liberals.

centers of Protestantism in the North and have only sporadically participated in the shift to liberalism that has occurred among most Presbyterians and many Baptists elsewhere.

On balance, then, although the present data strengthen our confidence in the thesis that there are discernible religious influences on political opinion and behavior within Protestantism, and that these influences are similar from region to region in the United States, the conclusion reached in the previous paper regarding the probable consequence of these influences must be modified. It is true that in both of our communities the working class is primarily subjected to conservatizing influences from fundamentalism. But in Tallahassee, unlike Eugene, only minor counterbalancing influences of a liberalizing nature confront the higher classes. There, ascetic Protestantism serves to reinforce and perhaps nowadays also to redirect politically[14] a long-established middle-class conservatism. Hence even though the pattern of religious influences on political opinion and behavior is almost identical in the two communities, a simple but crucial difference in the strength and distribution of the two religious factions in the two cities may be responsible for sharp and significant differences in the impact that Protestantism has had in the regions from which our communities were selected.

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[14] The association between fundamentalism and Republican voting in the South is probably of fairly recent origin. It does not make good historical sense to conclude that the association goes back for much more than a generation. As long as the Democratic party in the South was firmly identified with the policy of white supremacy and the national Democratic party was not consistently identified with political liberalism or progressivism, conservative southern churchmen could be strong Democrats. But when the national party began to challenge traditional ascetic Protestant values, as, for example, occurred quite dramatically in 1928 and more effectively if less dramatically during and after the New Deal, it became increasingly difficult for these churchmen to continue to vote Democratic in presidential elections. Whatever other factors may have been involved in the marked increase in Republican presidential voting in the South since World War II, the probable independent influence of an ideologically sensitive fundamentalism should not be overlooked.

CIRCUITOUS PARTICIPATION IN POLITICS[1]

FRED E. KATZ AND FERN V. PIRET

ABSTRACT

It is postulated that there exist forms of political participation outside the formally structured political mechanisms (such as voting and office-holding) which are chiefly based on using non-political mechanisms for political purposes. This is mainly supported by Gallup polls on presidential popularity: a president's popularity appears enhanced following conspicuous failures. From a practical point of view, this raises questions about the current emphasis on public passivity; perhaps there is more political vitality than measurement of formal participation indicates. In terms of sociological theory, our formulation of "circuitous" participation suggests attention to social mechanisms that are temporarily harnessed for purposes for which they were not intended. There is no reason to believe that this condition holds only for the field of political action.

The literature on public apathy to politics is extensive and seemingly unambiguous. Approximately half of the eligible voters actually exercise their political franchise; voters are ill-informed about candidates; criteria for judging politicians reflect ignorance concerning issues at stake. Morris Rosenberg has written: "Whether one measures apathy by the criterion of political involvement, knowledge, or activity, the number of people who satisfy the culturally defined desiderata of participation in politics is small."[2] Robert A. Dahl states:

Only about half of all Americans eligible to vote actually go to the polls in national elections. . . . Probably not more than 20 or 25% of all adult Americans engage in any kind of political activity beyond that of voting itself. The picture for something like 50 to 80% of the adult population is one of almost total political inactivity. . . . What is striking is the relatively small proportion of the adult population engaged in any sort of significant political activity.[3]

In reference to voters' lack of knowledge, Hyman and Sheatsley note: "In almost every instance where the polls have tested information, at least 20% of the population have revealed complete ignorance."[4] For example, during the 1948 election campaign, 12 per cent of the people interviewed did not know that Dewey was the Republican candidate, and 9 per cent did not know that Truman was the Democratic candidate. Yet, despite the evidence of passivity there remains a nagging question as to whether the public is really as politicially passive as is supposed. Oliver Garceau has noted that

a democratic presumption in political research has traditionally focused too exclusively on those few and sporadic interactions which relate individuals directly to formal government. This has contributed to what may prove to be an exaggerated concern with an iron law of oligarchy, with political apathy, with public ignorance and gullibility.[5]

We propose to extend this insight by inquiring whether *existing assessments of political participation may not be ignoring certain forms of participation.*

Our strategy, in pursuing the inquiry, is to focus on *forms* of participation. We are less concerned with how extensively particular forms of participation (such as

[1] We wish to acknowledge indebtedness to Paul C. Rosenblatt and H. Clyde Wilson.

[2] "Some Determinants of Political Apathy," in H. Eulau *et al.* (eds.), *Political Behavior: A Reader in Theory and Research* (Glencoe, Ill.: Free Press, 1956), p. 160.

[3] "Hierarchy, Democracy, and Bargaining in Politics and Economics," *ibid.*, pp. 86–87.

[4] H. H. Hyman and P. B. Sheatsley, "The Current Status of American Public Opinion," in D. Katz *et al.* (eds.), *Public Opinion in Propaganda* (New York: Dryden Press, 1959).

[5] "Research in the Political Process," in Eulau, *op. cit.*, p. 43.

voting and office-holding) are utilized than with clarifying what forms of participation are available and what implications follow from particular forms of participation. Our focus is on structures that are "ancillary" to formal political structures. These ancillary structures have distinctive characteristics: (1) They are not recognized as being part of the *formal* system of politics. They are essentially *informal* patterns which may, nonetheless, affect the operation of the system. However, they do not necessarily affect it. (2) They may involve social relationships that are only occasionally activated.[6] An example of such a relationship is interaction between the President and the citizen. We shall claim that this occurs not only at election time, the time when the political system formally activates it, but also during periods of crisis.

In our society political action is distinctly structured and participation is highly formalized. One holds political office if one is "duly" elected or appointed by a "duly" empowered office-holder; one does not compete for political office outside this formally organized mechanism. Again, one votes when elections are held, not between elections. If one does attempt to vote between elections one must do so in the context of by-elections, referendums, or other similarly formalized processes. The person who insists on voting outside formalized processes is regarded as an ineffectual crackpot. As a member of our society he is expected to know that political action is formalized and thereby explicitly regulated.

If, nonetheless, a citizen wishes to participate in political action outside the formally structured processes, what can he do? If, for example, he wishes to express sentiments toward the government *between* elections, what courses of action are open to him? Three courses appear to be available: (1) He can use the existing political structures as best he can. He can write to his congressman; he can write letters to newspapers; he can try to organize opposition or support for the government at the grass roots, and so forth. In all these he remains within the organized format of our political system. (2) He can try to challenge existing conditions by being politically innovative. He can oppose the conditions passively—by withdrawing from society, such as East Berliners' "voting with their feet," to use President Kennedy's felicitous phrase. At the other extreme, he can oppose the system actively and try to change it fundamentally—by promoting revolution. In less extreme fashion, he can organize political pressure groups aimed at changing the existing balance of political power. In all these efforts he is attempting to change conditions by *innovating* political mechanisms. (3) He can try to use non-political mechanisms for political purposes. This implies converting existing non-political structures into political structures and *bypassing* existing political mechanisms. Since this participation does not operate through the established channels his action is likely to go unrecognized, but it is not necessarily devoid of impact on political affairs. This circuitous participation is the least obvious of the three forms of political action and the least recognized in scientific assessments of political participation. It therefore deserves illustration and further clarification.

To the Western world the clearest large-scale example of non-political structures being converted into political ones is found in Communist countries. Political processes in the West are, in theory at least, far more clearly segregated from non-political processes than those in the Communist world. In the ideology of communism the interests of the social collectivity are so dominant that all activities by citizens ought to be linked to the needs of that collectivity. Concretely, this has come to mean that all organized activity tends to become

[6] See Fred E. Katz, "Occupational Contact Networks," *Social Forces*, XXXVII (October, 1958), 52–55. The author discusses social relationships that are only occasionally activated or mobilized. Between periods of mobilization they exist "on a low simmer," with little interaction or open recognition of the existence of the relationship.

subordinate to the interests of the state. Hence, by definition, there is no such thing as "converting" non-political action into political action. All actions are political.[7] But to Western minds political structures are deemed to be separate from non-political structures since, ideally, individualism comes before collectivism. In this connection it is noteworthy that Communist efforts at infiltrating non-Communist countries have usually involved efforts to promote the "politicizing" of non-political groups—making peace movements, labor unions, civil rights groups, and so forth, into political spearheads. This does violence to Western conceptions of the separation of political from non-political structures. But it does *not* do violence to the Communist conception of the "politicality" of all social activity. Western outrage at Communist infiltration is perhaps based less on opposition to entertaining hostile ideologies than on the realization that the Communist *method* of promoting their ideology involves changing the structure of our society—before we have had a "chance to vote" on the ideology.

In the Communist scheme, then, there is little scope for circuitous political participation since, ideologically at least, all activities are inherently political. By contrast, there are many organized activities in this country which are defined as non-political but which, on occasion, can be converted into political instrumentalities. Sometimes this conversion is quite temporary, and sometimes it is relatively permanent. Thus, the Queensboro Ladies' Thursday Afternoon Thackeray Reading Club may go on record as opposing medicare, and attempt to implement their decision by concerted action (through fund raising, propagandizing, etc.). Here is a temporary conversion of a non-political group into a political action group concerned with a specific political item. On a more permanent basis there are sustained political drives by scientists (such as the Association of Atomic Scientists' efforts at promoting disarmament) and physicians (the AMA's efforts to influence social security legislation). In addition to these well-known patterns there are instances where conversion of non-political actions into political mechanisms are entirely covert and, thereby, hidden from public view. To illustrate this we shall present findings from a study of Gallup polls on presidential popularity.

The polls were studied to judge responsiveness to actions by Presidents or actions for which the President was held accountable.[8] Two patterns were discovered in responses to the question, "Do you approve or disapprove of the way ——— is handling his job as President?" The first was entirely expected. The second was entirely unexpected.

1. There are responses of approval or disapproval following actions by the President that are, respectively, regarded as favorable or unfavorable.

2. Patterns of increased *approval* follow actions by the President (or actions for which he is held accountable) that are clearly recognized as major errors, if not disasters—as independently reflected in newspapers.

Only the second pattern is of concern here. Probably the most clear-cut recent instances of presidential involvement in untenable actions were the U-2 flight over Russia (May 1, 1960) and the abortive invasion of Cuba (April 15, 1961). These actions were widely regarded as failures for

[7] From the perspective of the West it is perhaps equally valid to say that *no* actions are political in the Communist world since, in Western sense, the "purely" political sphere does not exist.

[8] In order to minimize regional variation it was deemed necessary to concentrate on actions that had nationwide impact and elicited widespread attention. An attempt was made to assess reactions to Presidents independently of the polls. This was done by surveying editorials in selected newspapers. A systematic content analysis of a sample of newspapers would have been preferable but was not feasible. The authors believe that the actions singled out are sufficiently unequivocal to make it unnecessary to conduct a more extensive content analysis of a national sample of newspapers. We acknowledge that this aspect requires further verification.

this country and sources of acute embarrassment to the President. Concerning the U-2 incident, for example, an editorial in the *St. Louis Post Dispatch*, on May 8, 1960, stated:

"the Americans have been caught red-handed," Mr. Khrushchev said. It now seems that he was right. This is a shocking occurrence, and a full account should be made promptly to the American people.

TABLE 1

U-2 FLIGHT OVER RUSSIA
(May 1, 1960)

Date of Publication*	Per Cent		
	Approve	Disapprove	No Opinion
April 8	64	22	14
April†	62	22	16
May 24	65	22	12
June 8	68	21	11

* Interviewing usually takes place two weeks prior to publication. Dates of interviews were not available.

† Date of release unknown.

Note: All tables are based on Gallup Poll responses to the question, "Do you approve or disapprove of the way ——— is handling his job as President?" We wish to express gratitude to the American Institute of Public Opinion and to the Roper Public Opinion Research Center for furnishing the data.

Similar sentiments are to be found in other newspapers. The U-2 incident was particularly embarrassing to President Eisenhower, since he was scheduled, within a matter of days after the incident, to meet with the Soviet Premier to discuss peaceful relations between the United States and Russia. The Cuban invasion was similarly embarrassing to the country and to its new President, John F. Kennedy.[9] Yet, following the U-2 incident President Eisenhower's popularity rating *rose*. After the Cuban incident President Kennedy's rating reached a peak that has not been matched to this date (see Tables 1 and 2).

These findings give rise to the question of whether popularity ratings of the President during a period of acute crisis can be regarded as *assessments* of the President. It would seem that the high presidential popularity ratings following a catastrophic error for which the President bears responsibility (however unwittingly) are an expression of solidarity, a form of rallying behind the President at a time of national calamity, rather than an evaluation of the President's action—that they are not calculated assessments of the President but a "silent" form of participation in political affairs.

The conception that the presidential popularity poll may, at times, serve the unintended and latent function of expressing public support for the President is further indicated by the patterns of response after a presidential election. New Presidents enjoy a high rating[10] immediately upon inauguration—at a time when the public has virtually no basis for judging how well the President is "doing his job"—as shown in Table 3.

TABLE 2

CUBAN INVASION
(April 15, 1961)

Date of Publication	Per Cent		
	Approve	Disapprove	No Opinion
March 26	73	7	20
April*	78	6	16
May 5	83	5	12
May*	76	9	15

* Date of release unknown.

Even Presidents who are being inaugurated for a second time find themselves the recipients of a sudden bonus in the popularity ratings shortly before and after their inauguration (see Table 4).

Have they suddenly become better men in the eyes of the public or are they merely

[9] President Kennedy, according to the *New York Times*, was regarded as having "taken a public licking" (James Reston, *New York Times*, April 3, 1961). President Eisenhower was subjected to little personal assault by the press after the U-2 flight—but his administration did not come off lightly.

[10] "High" as compared with their subsequent ratings, as well as compared with the voting plurality in the preceding election.

TABLE 3

First Inauguration Period

Date of Publication	Per Cent			
	Approve	Disapprove	No Opinion	Rejected
Truman (1945):				
May 29, 1945*	82	3	10	5
July 1	87†	3	10	....
November 26	82	9	9	....
Eisenhower (1951–53):				
December 4, 1952 (Truman)	32	56	11	1
January 11, 1953 (Truman)	31	56	13	....
February 22, 1953 (Eisenhower)	68	7	25	....
March 20 (Eisenhower)	67	8	25	....
Kennedy (1960–61):				
December 6, 1960 (Eisenhower)	59	27	13	1
January 13, 1961‡ (Eisenhower)	59	27	14	....
March 1, 1961 (Kennedy)	72	6	22	....
March 26 (Kennedy)	73	7	20	....

* First poll after Mr. Truman's inauguration. No poll data are available for the year preceding this data.

† High popularity point of President Truman's career.

‡ Poll results for February, 1961, were not available.

TABLE 4

Second Inauguration Period

Date of Publication	Per Cent			
	Approve	Disapprove	No Opinion	Rejected
Truman (1948–49):				
June 11, 1948	39	47	14	....
June 16	37	46	16	1
January 5, 1949*	67	18	14	1
January 21	69	17	14	....
March 4	55	26	18	1
March 30	57	24	19	....
Eisenhower (1956–57):				
August 22, 1956 (nomination)	67	20	13	....
November 20	74	15	10	1
December 12	78	11	10	1
January 15, 1957 (inauguration)	73	14	12	1
February 1	79	11	10	....
March†	72	18	10	....

* Data for August to December, 1948, were not available; during this period the nomination of Mr. Truman occurred.

† Date of release unknown.

receiving a public "vote of confidence" delivered through the courtesy of the polling organization? This statement implies reservations about opinion polls. Our position is not that polls are useless, but that their interpretation requires greater flexibility than is generally shown by researchers. It has been aptly stated that "it is perhaps not so much in the development of greater accuracy as in the exercise of more ingenuity that the future of public opinion measurement lies.[11]

Our explanation of the poll results implies that the presidential popularity poll provided the people included in the sample with a medium for a minimal form of participation in national political affairs. To be sure, rallying behind the President constitutes at best remote and indirect participation in decision-making (the catastrophic decisions in the U-2 and the Cuban incidents had already been made). But as an expression of support for the President this unsolicited manifestation of solidarity does constitute a *form* of political participation. Its political impact cannot be accurately predicted; it lies in the extent to which results of polls are taken seriously by those in power. To the extent that they are taken into account they augment the mandate of the President. Kingsley Davis has suggested that an important part of the institutionalized political process in complex societies consists in efforts to establish a link between the rulers, the "agents of the entire society," and the society as a whole. These efforts, he says, "attest the right of a particular person to occupy [a] given position."[12] The patterns of approval appear to fall into this category.[13]

The conception of circuitous participation suggests that the public may be ready for more political participation than what is facilitated by the formal political structures. The poll data suggest that this is especially likely during times of crisis. This formulation ties in with the conception that there exists "dispersion" of political behavior in American society. Thus, in relation to power, Key writes that there are "a multiplicity of points of initiative for collective action, which may be either governmental or nongovernmental."[14] Apparently political participation is also attempted at multiple points. But the formalization of the political process prevents many of the voices from being heard.

SOME PRACTICAL WORDS

In addition to "non-political" mechanisms becoming converted into political participation mechanisms there is some indication that the formal participation mechanisms (such as voting) may be utilized for *forms* of participation for which they were not intended. Such phenomena as the election to public office of a person who was, at the time, under indictment for fraud are not unknown in United States political affairs. More extreme, even, is the recent election (in California) of a person who had died before the election. These instances suggest that

[11] Elmo C. Wilson, "The Measurement of Public Opinion," in H. A. Turner (ed.), *Politics in the United States* (New York: McGraw-Hill Book Co., 1955), p. 35.

[12] Kingsley Davis, *Human Society* (New York: Macmillan Co., 1948), p. 489.

[13] We have emphasized that mass *approval* is a form of circuitous political participation. Is not *disapproval* an equally distinct form of participation? On empirical grounds it must be answered that if there had been high "disapproval" of the President following the Cuban and U-2 failures there would be no way of justifying the claim that the ratings do not constitute efforts at rational assessment of the President. Accordingly, there would be no basis for making the analytic distinction between polls as means of assessing the President as against the conception that polls can serve as vehicles for political participation. We could not reject the possibility that mass disapproval might be a form of political participation. But on the basis of the data we should have to remain noncommittal. However, from a sociological perspective it can be argued that during a crisis there tends to be heightened social solidarity, manifesting itself in positive sentiment (approval) for the President.

[14] For a recent critical review of "groupism" see Stanley Rothman, "Systematic Political Theory: Observations on the Group Approach," *American Political Science Review*, LIV (March, 1960), 15-33.

votes may on occasion serve primarily as *solidarity devices*, as ways of rallying behind others.[15]

CONCLUSION

Our political system is highly formalized. Assessments of political participation tend to report participation in the formal machinery—in voting, in party membership, and so forth. We have suggested that it is necessary to acknowledge forms of participation that bypass the formal structures, and we have indicated some of the "paths" that these circuitous processes take. Finally, we wish to advance the proposition that such circuitous processes may be indicative of political and social vitality. A characteristic of a system that relies on formalization is the *routinization* of effort. This means that orderly procedures have been worked out to handle "routinely" problems and crises that arise. But it also means that participation tends to become "routine"—uninspired, undeviating, undemanding. In political affairs this means that one votes because it is time to vote; one supports one's party because it is one's party. By contrast, the participation we have postulated is apparently less fully routinized. The poll patterns, for example, appear to exhibit participation that is "freely offered" outside the bounds of formalized political structures. This would seem to constitute a kernel of vitality in our political system that has been obscured by the "mass apathy" formulations and that awaits effective recognition.

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[15] This also suggests that *failure may be politically useful*. Practical politicians have long known the value of receiving sympathy and have "used" their shortcomings accordingly. But theorists have usually assumed that any failure or shortcoming on the part of a candidate spells political suicide—that the public wants its political leaders to be pure and perfect. This may presuppose more purism than actually exists.

Can politicians count on reward for *failure?* At the present time it is an unwritten "law" that the party in power should plan *successes* before election time—or, if this is not possible, it should plan to insure tranquility; failure must be avoided at all cost. We are not so rash as to suggest that planned failure ought to replace planned success in a Machiavellian scheme to assure re-election. But we shall go so far as to say that Machiavellian politicians might seriously consider placing "failure" ahead of "tranquility" in their strategy.

THE NEGRO AS AN IMMIGRANT GROUP: RECENT TRENDS IN RACIAL AND ETHNIC SEGREGATION IN CHICAGO[1]

KARL E. TAEUBER AND ALMA F. TAEUBER

ABSTRACT

The processes of social and economic advancement and residential dispersion of Negroes cannot usefully be regarded as following the earlier processes of assimilation of ethnic groups. Negro residential segregation has remained high, despite their social and economic progress. Puerto Ricans and Mexicans, the most recent in-migrants, are economically less well off than Negroes, but their residential segregation is already less. A simple model demonstrates that only a small proportion of Negro residential segregation can be attributed to their low economic status.

During the last half of the nineteenth century and the early decades of the twentieth, millions of immigrants from Europe entered the United States. Many of these immigrants settled initially in ethnic colonies in large northern cities and found jobs as unskilled laborers in burgeoning mass-production industries. With the onset of World War I in Europe, and with the passage of restrictive legislation in the United States in the early 1920's, the period of massive overseas migration came to an end. At the same time, however, there developed a large-scale migration of Negroes from the South to the same large northern industrial cities. Like the immigrants from abroad, the Negro migrants to northern cities filled the lowest occupational niches and rapidly developed highly segregated patterns of residence within the central cities.

In view of many obvious similarities between the Negro migrants and the various immigrant groups preceding them, it has been suggested that northern urban Negroes are but the latest of the immigrant groups, undergoing much the same processes of adaptation to city life and of assimilation into the general social structure as the European groups preceding them.[2] The persistence of Negroes as a residentially segregated and underprivileged group at the lowest levels of socioeconomic status, however, is frequently interpreted in terms of distinctive aspects of the Negro experience, particularly their historical position in American society.[3]

The question of whether or not a northern urban Negro population can fruitfully be viewed as an immigrant population, comparable to European immigrant populations of earlier decades with respect to the nature and speed of assimilation, will be explored on the basis of data permitting analysis of recent trends in racial and ethnic segregation in Chicago.

The processes by which various immigrant groups have been absorbed into American society are complex and have been studied from a variety of viewpoints. Unfortunately there is no sociological consensus on a definition of assimilation and there is nothing approaching a definitive study of the processes of assimilation for any one immigrant group. It is beyond the scope of our task here to attempt to provide such a definition. We feel that a dis-

[1] Paper No. 15 in the series, "Comparative Urban Research," was issued from the Population Research and Training Center, University of Chicago, under a grant from the Ford Foundation. A preliminary version of this paper was read at the 1962 annual meetings of the American Statistical Association. We appreciate the reactions of Stanley Lieberson, Judah Matras, and Margaret G. Reid to that version.

[2] Philip M. Hauser, "On the Impact of Urbanism on Social Organization, Human Nature and the Political Order," *Confluence*, VII (Spring, 1958), 65. Elsewhere Hauser has expressed a more cautious view, emphasizing the lack of definitive knowledge; see his *Population Perspectives* (New Brunswick, N.J.: Rutgers University Press, 1960), p. 129.

[3] D. J. Bogue, "Chicago's Growing Population Problem," *Commerce*, LIX (July, 1962), 31.

tinctively sociological approach to the topic must view assimilation as a process of dispersion of members of the group throughout the social structure. Cultural and psychological processes, we feel, should not be incorporated into a sociological definition, although their relationship to institutional dispersion should, of course, be retained as one focus of research on assimilation.

For our purposes, it will suffice to have a working definition of the process of assimilation considerably less sophisticated than that required for a general sociological theory. Accepting the view that both immigrant groups and Negro migrants originally settled in segregated patterns in central areas of cities and ranked very low in terms of socioeconomic measures, assimilation then consisted in large part of a process of social and economic advancement on the part of the original members of the group and their descendants, along with a decreasing residential concentration in ethnic colonies. Our concern with diminishing residential segregation as a necessary concomitant of the assimilation process derives from Myrdal's discussion of the "mechanical" importance of residential segregation in facilitating other forms of segregation and discrimination, and Hawley's discussion of the impact of spatial patterns on race relations.[4] Our concern with socioeconomic advance reflects the initially low status of the groups with which we are concerned, whereas a more general treatment would need to reckon with the unusually high status of some immigrant stocks, as well as with other aspects of social status and institutional dispersion than those for which we have data.

The data in Table 1 illustrate for selected immigrant groups the patterns of socioeconomic advance and residential dispersion from highly segregated ethnic colonies. For each of the larger ethnic groups, data for 1950 show the average standing on three measures of socioeconomic status, standardized for age, of the first generation (the foreign-born white, FBW) and the second generation (native white of foreign or mixed parentage, NWFMP). The nationality groups are split into "old," "new," and "newer" groups in an extension of the traditional system. On the average, comparing within the first or within the second generation, the "old" immigrant groups are the best off on these measures, the "new" groups are intermediate, and the "newer" groups are the worst off. It cannot be determined from these data to what extent the old immigrants are better off by virtue of their longer average length of residence in the United States, or to what extent they may have been better off at their time of immigration than the newer immigrants were at the time of their move.

Comparisons between the first and second generations might appear to be a more direct means for assessing the extent of socioeconomic advance, particularly since the emphasis in the literature on assimilation is on intergenerational processes rather than simply on processes of upward mobility through time in the status of the original immigrants. Comparisons of corresponding status measures for the first and second generations in Table 1 reveal, in general, the expected pattern of intergenerational advance. Data such as these, however, do not refer directly to a specific set of immigrant parents and their native-born children and must be interpreted with great caution.[5] For instance, it would be unwarranted on the basis of these data to assume that descendants of German immigrants are not as well off as their parents in terms of education. It is more credible that recent immigrants from Germany, under our immigration laws, include a large proportion of persons of high socioeconomic status.

[4] Gunnar Myrdal, *An American Dilemma* (New York: Harper & Bros., 1944), I, 618; Amos H. Hawley, "Dispersion versus Segregation: Apropos of a Solution of Race Problems," *Papers of the Michigan Academy of Science, Arts, and Letters*, XXX (1944), 667–74.

[5] For an enumeration of some of the difficulties see C. A. Price and J. Zubrzycki, "The Use of Inter-marriage Statistics as an Index of Assimilation," *Population Studies*, XVI (July, 1962), 58–69.

Measures of the changing residential patterns of the immigrant groups are given in columns 7–9 of Table 1. The measure, an index of residential segregation between the total foreign stock (FBW + NWFMP) of each nationality and the total native whites of native parentage (NWNP), assumes a value of 100 for maximum residential segregation and a value of 0 if the residential distributions are identical.[6] The indexes were computed from the distribution of each group among the seventy-five community areas of the city of Chicago for 1930 (the last previous census year that included information on the total foreign stock) and 1960. The degree of residential segregation from the native population is highest for the "newer" immigrants and lowest for the "old" immigrants. Between 1930 and 1960, most of the ethnic groups became less segregated from the native population. Only for England, Ireland, and Sweden did the indexes fail to decline, and these were already at relatively low levels.[7]

TABLE 1

SELECTED CHARACTERISTICS (AGE-STANDARDIZED) OF FOREIGN-BORN AND NATIVE ETHNIC POPULATIONS IN 1950, AND INDEXES OF RESIDENTIAL SEGREGATION OF SELECTED GROUPS OF FOREIGN STOCK FROM NATIVE WHITES OF NATIVE PARENTAGE, 1930 AND 1960, CHICAGO*

COUNTRY OF ORIGIN	PER CENT HIGH-SCHOOL GRADUATES (MALES AGE 25 AND OVER)		PER CENT WITH INCOME ABOVE $3,000 (PERSONS WITH INCOME)		PER CENT WITH WHITE-COLLAR JOBS (EMPLOYED MALES)		INDEX OF RESIDENTIAL SEGREGATION (Compared with NWNP)		
	FBW	NWFMP	FBW	NWFMP	FBW	NWFMP	1930	1960	Change
"Old" immigrant groups:									
England and Wales...	45	50	53	58	49	51	11	18	+7
Ireland.....	24	47	47	56	22	47	23	31	+8
Norway......	31	47	54	57	24	51	44	37	−7
Sweden........	25	48	59	60	23	51	26	30	+4
Germany......	37	34	53	55	34	42	22	19	−3
"New" immigrant groups:									
Austria........	29	40	54	57	33	44	30	16	−14
Czechoslovakia.	25	33	44	54	22	36	59	37	−22
Italy.......	15	27	47	53	24	37	52	32	−20
Poland.....	18	25	42	49	25	30	63	38	−25
U.S.S.R........	35	60	60	69	59	74	51	44	−7
"Newer" immigrant groups:									
Mexico........	14	16	38	29	8	13	71	54	−17
Puerto Rico†...	13	29	16	37	22	36	†	67	†

* Data for 1930 and 1950 refer to foreign white stock (foreign-born plus native of foreign or mixed parentage); data for 1960 refer to total foreign stock. Abbreviations used are FBW for foreign-born white, NWFMP for native white of foreign or mixed parentage, and NWNP for native white of native parentage. The three socioeconomic characteristics refer to the Standard Metropolitan Area population, while the segregation indexes are based on community areas within the city. Age-standardization was by the direct method, using age groups 25–44 and 45 and over, with the Standard Metropolitan Area age composition as a standard.

† Socioeconomic characteristics for Puerto Rican population refer to total United States; Puerto Rican population by community areas for Chicago available for 1960 only.

Source: Characteristics from U.S. Bureau of the Census, *U.S. Census of Population: 1950*, Vol. IV, *Special Reports*, Pt. 3, chap. A, "Nativity and Parentage," and chap. D, "Puerto Ricans in Continental United States." Distributions of population by community areas for 1930 and 1960 from data on file at Chicago Community Inventory, University of Chicago.

[6] The index of residential segregation is an index of dissimilarity between the residential distributions of each group. For further discussion, see Otis Dudley Duncan and Beverly Duncan, "A Methodological Analysis of Segregation Indexes," *American Sociological Review*, XX (April, 1955), 210–17.

[7] For a more detailed discussion of these patterns, using data for 1930 and 1950, see Otis Dudley Duncan and Stanley Lieberson, "Ethnic Segregation and Assimilation," *American Journal of Sociology*, LXIV (January, 1959), 364–74.

This general approach to the measurement or assimilation of immigrant groups has been pursued for a number of cities and longer time periods by Lieberson. He found a remarkably persistent and consistent association through time between residential desegregation of an ethnic group and increasing socioeconomic similarity to native whites, and cross-sectionally between the position of each group as compared to others on measures of residential segregation and its relative levels on status measures.[8]

The index of residential segregation between Negroes and NWNP for 1930 was 84, and for 1960, 82. These values are higher than any of those for specific immigrant stocks. Furthermore, each of the immigrant stocks was highly segregated from Negroes in 1930 and 1960. There is relatively little intermixture of Negro residences with those of any group of whites. Even the "newer" immigrant groups, the Puerto Ricans and Mexicans, are not joining or replacing Negroes in established Negro areas but are moving into separate ethnic colonies of their own at the periphery of Negro areas. Negroes clearly occupy a distinctive position as the most residentially segregated of the principal migrant groups. The separation of Negroes from all groups of whites is sharper than any of the patterns of residential segregation between ethnic groups or between socioeconomic groups within the white population.[9] Apparently this pattern has developed during the last few decades. Lieberson has demonstrated that, although prior to the great Negro migrations of World War I there were instances of immigrant stocks being more segregated from native whites than were Negroes, since 1920 there has been a general tendency for Negro residential segregation to be highest.[10]

[8] Stanley Lieberson, *Ethnic Patterns in American Cities* (New York: Free Press of Glencoe, 1963).

[9] For a discussion of class residential segregation in Chicago see Otis Dudley Duncan and Beverly Duncan, "Residential Distribution and Occupational Stratification," *American Journal of Sociology*, LX (March, 1955), 493–503.

Data pertaining specifically to the comparison between whites and non-whites (97 per cent of Chicago's non-whites are Negroes) on measures of socioeconomic status, and of residential segregation are presented in Table 2. For each of four

TABLE 2

SELECTED SOCIOECONOMIC CHARACTERISTICS (UNSTANDARDIZED) OF WHITES AND NON-WHITES, CHICAGO, 1940, 1950, AND 1960

Characteristic	Non-White	White
Residential segregation index, whites vs. Negroes:*		
1930	85	
1940	85	
1950	79	
1960	83	
Per cent high school graduates, ages 25+:		
1940	16	25
1950	25	37
1960	29	37
Per cent white collar, male:		
1940	17	40
1950	17	41
1960	21	40
Per cent home-owners:		
1940	7	26
1950	12	33
1960	16	39
Per cent multiple-person households with 1.01 or more persons per room:		
1940	41	17
1950	46	14
1960	34	10

* These values differ slightly from those cited in the text for Negroes as compared to native whites of native parentage.

Source: Data for 1940 from the 1940 Census Tract Bulletin for Chicago; for 1950 from Philip M. Hauser and Evelyn M. Kitagawa (eds.), *Local Community Fact Book for Chicago, 1950* (Chicago: Chicago Community Inventory, 1953); and for 1960 from the 1960 Census Tract Bulletin for Chicago.

measures reflecting socioeconomic status, there was improvement in the status of the non-white population between 1940 and 1960. (For whites, improving status would be more clearly evident if the data referred to the entire metropolitan area rather than just the city of Chicago.) The indexes of residential segregation between whites and Negroes, in the top panel of the table, show minor fluctuations around an extremely high level and give no indi-

[10] Lieberson, *op. cit.*, pp. 120–32.

cation of the decline anticipated on the basis of the socioeconomic advancement of the Negro population. That this is not an atypical finding is indicated by reference to other data showing a long term historical trend toward increasing residential segregation between whites and non-whites. Increasing racial residential segregation was evident in most large cities of the United States between 1940 and 1950, while during the 1950's, southern cities continued to increase in segregation and northern cities generally registered modest declines.[11]

In broad perspective, the historical trend toward improving socioeconomic status of immigrant groups has gone hand in hand with decreasing residential segregation. In contrast, Negro residential segregation from whites has increased steadily over past decades until it has reached universally high levels in cities throughout the United States, despite advances in the socioeconomic status of Negroes.

We have been unable to locate any data permitting a comparison between Negroes long resident in Chicago, or born and raised in the North, and Negroes with lesser periods of residence in the city. Thus we are not able to make even the crude intergenerational comparisons for Negroes that are possible for the immigrant groups. The only analysis of this type possible with census data is a comparison between recent migrants and the rest of the population, and the only published data are residential distributions, with no socioeconomic characteristics. For 1960, with the seventy-five community areas of Chicago as units, the index of residential segregation betweeen non-whites resident in the metropolitan area five years or more and native whites of native parents is 80.5. Comparing non-whites with less than five years' residence in the metropolitan area and NWNP, the index was 81.0. Comparing the recent in-migrants with the non-whites who were resident in the metropolitan area five years or more, the index was 13. Thus the recent non-white in-migrants are distributed differently from the rest of the non-white population, but each group is highly segregated from the native whites. Unfortunately, these results cannot be readily interpreted in terms of the general assimilation and dispersion processes under consideration. Possibly there are trends toward socioeconomic advancement and residential dispersion on the part of "second generation" Negroes in Chicago that are confounded in the data for the total Negro population.

Decreasing residential concentration of immigrant groups occurred despite the efforts of many nationality organizations to maintain the ethnic colonies.[12] Few Negro organizations have been as explicitly segregationist. In some immigrant groups, many members were dispersing from the ethnic colonies even while large-scale immigration of that group was still under way. For every immigrant group, diminishing residential segregation has been evident since the cessation of large-scale immigration. For Negroes, however, residential segregation has increased since the first period of large-scale inmigration to northern cities, and this increase in residential segregation continued during the late 1920's and 1930's when the volume of migration was at a low level. These observations tend to discredit the argument that a major barrier to residential dispersion of the Negro population of Chicago is its continuing rapid increase. However, the size of the Negro population and the magnitude of its annual increase are larger than for any single ethnic group in the past, and comparisons with smaller groups are not completely convincing. That rapid increase of Negro population does not necessarily lead to increasing residential segregation was demonstrated directly

[11] Karl E. Taeuber, "Negro Residential Segregation, 1940–1960: Changing Trends in the Large Cities of the United States" (paper read at the Annual Meetings of the American Sociological Association, 1962).

[12] David A. Wallace, "Residential Concentration of Negroes in Chicago" (unpublished Ph.D. dissertation, Harvard University, 1953).

in the intercity comparative study previously cited. There was no definite relationship between increase in Negro population and increase in the value of the segregation index. Indeed, during the 1950–60 decade, there appeared to be a slight relationship in the opposite direction.[13]

More significant in accounting for the divergent trends in residential segregation may be the different urban contexts in which the immigrant and Negro populations found themselves. Comparing the residential locations of Italian-born and Polish-born in Chicago in 1899 and in 1920, Wallace observed:

> it can be seen that the areas of greatest dispersion, low proportion, and presumably of "second" settlement for many immigrants were those which were not settled at all in 1899.
>
> The implication of this fact is that the so-called "assimilation" process was not reflected by the geographic dispersion of the immigrant populations into "cosmopolitan American areas." The dispersal was more directly related to an increase in housing alternatives as the city grew at the periphery.[14]

By the time the Negro concentrations were forming near the central areas of Chicago, the city was built up and the urbanized area extended well beyond the present boundaries. Residential alternatives at a price Negroes could afford and located sufficiently close in to permit inexpensive commuting were no longer available.

It has been suggested that considerable time is required for Negroes to make the transition from a "primitive folk culture" to "urbanism as a way of life."[15] Several types of data indicate that large and increasing proportions of the Negro urban population are city-born and raised. For instance, there is a rapidly decreasing color differential in the percentage of the Chicago population born in the state of Illinois. In 1960, 44 per cent of the native-born, non-white residents of Chicago were born in Illinois, as contrasted to 66 per cent of the white population.[16] National estimates for 1958 showed that of all males aged 45–64 living in metropolitan places of 500,000 or more population, 65 per cent of the non-whites, as compared to 77 per cent of the whites, had lived in this size city for twenty years or longer.[17] Estimates of the components of growth of the non-white population of Chicago indicate that between 1950 and 1960 natural increase was as important as net in-migration, and that natural increase will in the future account for rapidly increasing proportions of the growth of the non-white population.[18]

Unfortunately there is inadequate knowledge of the specific length of time under specified conditions for the required cultural transformation to occur. Wallace's observations indicate a significant degree of dispersal over time among first-generation immigrants. Such processes are more often conceived as primarily intergenerational. That many of the "first generation" Negro migrants to northern cities have lived there for twenty years or more and that in the younger adult ages there are sizable numbers of "second generation" urban Negroes suggest that there has been ample time for any necessary adjustment to urban living, at least for large proportions of the Negro population. It is also clear that if northern Negroes remain inadequately educated for urban living and fail to participate fully

[13] Taeuber, *op. cit.*

[14] Wallace, *op. cit.*, p. 205.

[15] Philip M. Hauser, "The Challenge of Metropolitan Growth," *Urban Land*, XVII (December, 1958), 5.

[16] Data from U.S. Bureau of the Census, *U.S. Census of Population, 1960: General Social and Economic Characteristics, Illinois.* Final Report PC(1)-15C, Tables 72 and 77.

[17] Karl E. Taeuber, "Duration-of-Residence Analysis of Internal Migration in the United States," *Milbank Memorial Fund Quarterly*, XXXIX (January, 1961), Table 3.

[18] D. J. Bogue and D. P. Dandekar, *Population Trends and Prospects for the Chicago–Northwestern Indiana Consolidated Metropolitan Area: 1960 to 1990* (Chicago: Population Research and Training Center, University of Chicago, 1962).

in the urban economy, the "primitive folk culture" of the South can less and less be assigned responsibility, and northern cities will be suffering from the neglect of their own human resources.

The "visibility" of Negroes due to skin color and other features which make the large majority of second-, third-, and later-generation descendants readily identifiable as Negroes is often cited as a basic factor in accounting for the distinctive position of Negroes in our society. It is exceedingly difficult to assess the significance of visibility. There is no other group that is strictly comparable to Negroes regarding every factor except visibility. It is not completely irrelevant, however, to note that non-white skin color, by itself, is not an insurmountable handicap in our society. The socioeconomic status of the Japanese population of Chicago in 1950 substantially exceeded that of the Negro population; and their residential segregation from whites, although high, was considerably lower than that between Negroes and whites.[19] Unfortunately there are no trend data available on the characteristics of the Japanese in Chicago. A more appropriate Japanese population for comparison, however, is the much larger one in the San Francisco area. A recent study there affirmed that "ethnic colonies of Japanese are gone or rapidly going" and documented their rapid socioeconomic advance.[20]

In the traditional immigrant pattern, the more recent immigrants displaced the older groups at the bottom socioeconomic levels. How do the Negroes compare with the other "newer" immigrant groups, the Mexicans and the Puerto Ricans? The limited data now available suggest that the Negroes may soon be left alone at the bottom of the social and economic scale. We have already noted (from data in Table 1) that the "newer" groups were, in 1950, of very low status compared to the other immigrant groups, and that their residential segregation from the native whites of native percentage was the highest of all the immigrant groups. For 1960, data on distribution within Chicago of persons born in Puerto Rico are available separately from data on those persons born in the United States of Puerto Rican parentage. Thus it is possible to compute indexes of residential segregation for first- and second-generation Puerto Ricans. For Chicago in 1960, these index values were 68.4 for the first generation and 64.9 for the second generation, indicating that residential dispersion has already begun for the Puerto Ricans. This difference actually understates the amount of dispersion, since the second generation consists in large proportion of children still living with their first-generation parents.

Selected socioeconomic measures for the Puerto Rican and the non-white populations of Chicago in 1960 are shown in Table 3. On every measure, the Puerto Rican population is less well off—it is less educated, has lower income, is more crowded, is less likely to own homes, is less well housed, and lives in older buildings. Yet the index of residential segregation (computed with respect to NWNP) for Puerto Ricans is 67 as compared with 82 for Negroes.

Up to now we have been making comparisons between Negroes and immigrant groups, demonstrating that residential dispersion has not accompanied socioeconomic advance by Negroes in the way that it did for immigrant groups. Economic status and expenditure for housing, however, are clearly correlated, and there is also a correlation between economic status and residential segregation. By virtue of variations in the type, age, and quality of housing, and in the patterns of residential

[19] Although the maximum value of the residential segregation index is less than 100 for ethnic groups of small size, this is not sufficient to vitiate the Negro-Japanese comparison.

[20] Harry H. L. Kitano, "Housing of Japanese-Americans in the San Francisco Bay Area," in Nathan Glazer and Davis McEntire (eds.), *Studies in Housing and Minority Groups* (Berkeley: University of California Press, 1960), p. 184.

choice by persons of varying socioeconomic status, the subareas of a city are differentiated in terms of the average status of their residents. Since Negroes are of much lower average status than whites, they would be expected to be disproportionately represented in low-status residential areas. In fact, an extreme position regarding the relationships between patterns of socioeconomic residential segregation and racial residential segregation would attribute all of the latter to the former. Such a position is sometimes offered as a counterargument to charges of racial discrimination against the real estate business. To the extent that this position is correct, it might be expected that future economic advances on the part of the Negro population should be translated into decreased residential segregation.

The task of partialing out a component of racial segregation due to economic factors involves some difficult methodological problems, and no method is entirely satisfactory.[21] Our approach utilizes indirect standardization of available census data. Let us delineate the status of a residential area in terms of, say, the income distribution of its residents. Specifically, consider for each community area of Chicago the number of families with incomes below $1,000, from $1,000–1,999, from $2,000–2,999, and so forth. For the city as a whole in 1960, 44 per cent of all families with an income below $1,000 were non-white, as were 44 per cent of families with incomes from $1,000–1,999, and 40 per cent of families with incomes from $2,000–2,999. For each community area, we can apply these city-wide percentages to the observed income distribution to obtain the number of non-white families expected if income alone determined the residential locations of whites and non-whites.

[21] A general discussion of this problem can be found in the section on explanation of areal variation in Otis Dudley Duncan, Ray P. Cuzzort, and Beverly Duncan, *Statistical Geography* (Glencoe, Ill.: Free Press, 1961).

By the method of indirect standardization just outlined, we obtain an expected number of non-white and white families for each of the seventy-five community areas. We can then compute an index of residential segregation between expected numbers of non-white and white families. This index can be regarded as the amount of racial residential segregation attributable to patterns of residential differentiation of

TABLE 3

SELECTED SOCIOECONOMIC CHARACTERISTICS (UNSTANDARDIZED) OF PUERTO RICANS AND NON-WHITES, CHICAGO, 1960

Characteristic	Non-White	Puerto Rican
Residential segregation vs. whites	83	67
Per cent high school graduates, total	29	11
Median family income	$4,742	$4,161
Per cent families earning <$3,000	28	27
Per cent families earning >$10,000	9	4
Per cent home-owners	16	6
Per cent substandard dwellings	26	33
Per cent 1.01 or more persons per room	34	52
Per cent housing units built since 1940	12	6
Median gross rent	$88	$79
Median number of rooms	3.9	3.7
Median number of persons	3.0	4.0

Source: Data are from the 1960 Census Tract Bulletin for Chicago.

income groups. For 1950, the index of residential segregation between the numbers of whites and non-whites expected on the basis of income was 11, as compared with the actual segregation index of 79. As a rough measure, then, we can attribute 11/79, or 14 per cent, of the observed racial residential segregation in Chicago in 1950 to income differentials between whites and non-whites. For 1960, the corresponding values are 10 for the expected index, 83 for the observed index, and 12 per cent for the racial segregation attributable to income differentials.

In a recent study of the relationships between housing consumption and income,

Reid has demonstrated many pitfalls in the uncritical use of income distributions in the analysis of housing patterns.[22] We have therefore repeated the above analyses, using distributions by major occupational groups and distributions by educational attainment. For 1960, the index of residential segregation computed from the numbers of whites and non-whites expected on the basis of patterns of occupational differentiation is 9, and that expected on the basis of patterns of educational differentiation is 3. The results using income distributions are thus supported by the results from other measures of socioeconomic status, and the conclusion seems clear that patterns of socioeconomic differentiation of residential areas can account for only a small proportion of observed racial residential segregation.

Reid demonstrated that differences between whites and non-whites in observed patterns of housing consumption are largely attributable to income differentials between whites and non-whites. Our analysis suggests that residential segregation cannot be attributed to these differentials. Apparently the economic structure of the housing market for whites is similar to that for non-whites, even though non-whites are excluded from a large share of the housing supply for which their economic circumstances would allow them to compete.

The judicious conclusion from our review of a variety of pieces of data is that we simply do not yet know enough about immigrant assimilation processes and any corresponding processes among Negro migrants to northern cities to be able to compare the two. We believe that this very lack of knowledge makes questionable any attempt to reason from presumed patterns of assimilation among immigrants in the past to current racial problems in northern cities. Furthermore, such evidence as we could compile indicates that it is more likely to be misleading than instructive to make such comparisons.

Our definition of assimilation as involving socioeconomic advancement and residential dispersion is simple, and greater differences between groups would appear were a more complex definition adopted. Restriction of portions of the analysis to the city of Chicago had little effect on the measures for non-whites, but probably led to an understatement of the degree of assimilation of the immigrant stocks insofar as higher-status members of these groups have moved to the suburbs. The segregation indexes probably overstate somewhat the residential isolation of small groups, such as particular immigrant stocks, as compared with large groups such as total native whites of native parents. Taking account of any of these limitations in our data would tend to increase the differences between Negroes and immigrant groups. Even so, our data showed that second-generation persons from several countries are of higher socioeconomic status than the total native whites of native parentage. Relatively few Negroes in Chicago have white-collar jobs or incomes above the median level for whites. and yet there are large numbers of adult Negroes who were born in the city. Basic differences between the Negroes and the immigrant groups seems to us implicit in the failure of residential desegregation to occur for Negroes while it has continued for the immigrant groups.

In view of the fundamental impact of residential segregation on extralegal segregation of schools, hospitals, parks, stores, and numerous other facilities, the failure of residential dispersion to occur strikes us as an especially serious social problem. Socioeconomic advance and residential dispersion occurred simultaneously for the various immigrant groups. It is apparent that the continued residential segregation of the Negro population is an impediment to the continued "assimilation" of Negroes into full and equal participation in the economy and the society at large.

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[22] Margaret G. Reid, *Housing and Income* (Chicago: University of Chicago Press, 1962).

INTERNAL MIGRATION PATTERNS OF A POPULATION SUBGROUP COLLEGE STUDENTS, 1887–1958[1]

H. THEODORE GROAT

ABSTRACT

This analysis of interstate migration for college enrolment is based on a series of public and private reports dating from 1887 to 1958. Since 1930, about 20 per cent of all students have left their home states for enrolment elsewhere. The patterns of student migration have changed markedly over the period covered by this study, particularly since World War II, with a large westward migration being the most notable recent development. Patterns of migration vary greatly by type of institution (public or private), as well as by level of training involved (graduate or undergraduate). Economic variables highly correlated with total population migration are not similarly correlated with student migration.

The United States Bureau of the Census currently counts college students as residents of the states in which they live while attending college, and hence counts most out-of-state students as migrants.[2] Since students, while in college, probably contribute more to the economy of their school community than of their home community, this policy seems sound at least from a commercial viewpoint. In addition, since evidence suggests that a large number of college graduates do not return to their home community,[3] there is even greater reason for classifying students as migrants. From another viewpoint, we know that rates of migration in the United States are highest for precisely that age group—young adults—which comprises the college population[4] and that by far the highest migration rates are those for college-educated people and the lowest those for people with little formal schooling.[5] These findings, coupled with data on the size of present college enrolments and the prospects for enrolment in the near future,[6] emphasize the importance of ascertaining the extent to which college populations have migrated, historically and at present, as well as of evaluating the possible future role that the college population may play in internal migration in the United States.

DATA

Statistics on student enrolment in colleges and universities throughout the

[1] This paper is based on a portion of the writer's Ph.D. dissertation, "College Student Migration: 1887 to 1958" (Brown University, 1962).

[2] The Census procedure classifies as a migrant anyone who has changed county of residence in the process of moving. Beyond this, the specific question asked by the Census to determine the amount and direction of migration has varied. In 1940 the Census-taker asked where the person being enumerated had lived on April 1, 1935. In 1950 place of residence a year earlier, on April 1, 1949, was asked, but in 1960 the Census again used the five-year interval, asking for place of residence on April 1, 1955. Obviously, the longer the time interval, the more "migrants" were enumerated. By this procedure almost all college students living outside their counties of family residence in 1960 were counted as migrants, but only those students in their first year of enrolment were counted as migrants in 1950. To complicate the matter further, the Census also changed from a *de jure* concept for students in 1940 to a *de facto* concept in 1950 and 1960. The result is that in 1940 most college students were not counted as official migrants, while in 1950 and 1960 many of them were, the proportion varying, of course, because of the difference in specific questions asked by each of these two censuses.

[3] See, e.g., John H. Lane and Donald D. Stewart, "Interstate Migration among College Graduates," *Journal of Higher Education*, XXII (November, 1951), 437–40; and William H. Whyte, Jr., *The Organization Man* (Garden City, N.Y.: Doubleday & Co., 1957), p. 298.

[4] Donald J. Bogue, *The Population of the United States* (Glencoe, Ill.: Free Press, 1959), pp. 379–81.

[5] *Ibid.*, p. 382.

[6] The present enrolment of approximately three and one-half million students has been projected to 1980 figures falling generally within a range from 6.5 million to 9.0 million (see Bureau of the Census, *Current Population Reports*, Ser. P-25, No. 232, "Illustrative Projections to 1980 of School and College Enrollments in the United States," Washington, D.C., June 22, 1961).

country have regularly been compiled by the Bureau of Education and its present counterpart, the Office of Education, for use by registrars, admissions officers, and numerous public and private agencies. In compiling these data, the Office of Education has followed the practice of classifying attendance according to the states in which the respective institutions were located. No attempt was made in the regular reports to classify students according to the states in which they resided.

On several occasions, however, special tabulations were made, by both private and public agencies, showing the number of residents of each state who were attending universities and colleges in the various states during a given academic year. In all there were available for the present study sets of data on college student migration at approximately ten-year intervals from 1887 to 1958, except for a twenty-three year gap between 1897 and 1920. Specifically, data were available for the academic years 1887–88, 1896–97, 1920–21, 1922–23, 1930–31, 1938–39, 1949–50, and 1958–59.[7] Because of the proximity of the 1920–21 and 1922–23 data and because of the much greater scope of the latter, the 1920–21 data are generally excluded from the present analysis. Although the data in these various reports are not uniformly reliable or completely comparable in scope or design, most of these limitations apply only to the two earliest reports, for 1887–88 and 1896–97, and in no way affect the major substantive findings which are based on data since 1922–23.[8] Thus there were available data on student enrolment, cross-tabulated by state of residence,[9] for eight different time periods. In addition, the data included separate tabulations by sex in 1922 and 1958; by graduate and undergraduate enrolment in 1949 and 1958; and by public and private enrolment in 1949 and 1958.

PROCEDURES

For each of the academic years covered by the reports listed above, there were available data on how many students from each of the several states attended college in their home state, as well as in each of the other states and the District of Columbia. It was possible to compute this information from available data where it was not already tabulated in this manner. The data made possible historical comparisons and retabulations by Census divisions and regions.

The data were analyzed both in terms

[7] From the following, respectively: Bureau of Education, *Report of the Commissioner of Education for the Year 1887–88* (Washington, D.C.: Government Printing Office, 1889), pp. 716–20; Bureau of Education, *Report of the Commissioner of Education for the Year 1896–97* (Washington, D.C.: Government Printing Office, 1898), pp. 1657–61; Bureau of Education, *Residence and Migration of College and University Students, 1921–22* (Bulletin 1922, No. 18 [Washington, D.C.: Government Printing Office, 1922]); Bureau of Education, *Residence and Migration of College and University Students, 1922–23* (Bulletin 1926, No. 11 [Washington, D.C.: Government Printing Office, 1926]); Office of Education, *Residence and Migration of College Students, 1930–31* (Pamphlet No. 48 [Washington, D.C.: Government Printing Office, 1934]); Office of Education, *Residence and Migration of College Students, 1938–39* (Pamphlet No. 98 [Washington, D.C.: Government Printing Office, 1943]); Office of Education, *Residence and Migration of College Students, 1949–50* (Misc. No. 14 [Washington, D.C.: Government Printing Office, 1951]); *The Home State and Migration of American College Students, Fall, 1958* (The American Association of Collegiate Registrars and Admissions Officers, Committee on Research and Service, March, 1959).

[8] More specifically, the 1887–88 and 1896–97 data were taken from the catalogs of 288 unidentified colleges and universities which published this information at approximately those times. These early reports also excluded professional and teacher-training institutions with the exception of a few scattered data which could not be tabulated separately. The data since and including the 1922–23 report, upon which the major part of this study relies, are generally similar in scope and design.

[9] Throughout this study state of residence refers to a student's "home state," or state of permanent residence. In general, this means the student's parental home state, or the state in which the student resided immediately prior to his initial registration at an institution. This was the procedure asked of colleges and universities in compiling the several reports used in this analysis.

of absolute numbers and as migration rates per 1,000 student population. For both absolute numbers and rates, in-, out-, and net migrations were computed for each state, division, and region, for all years included in the study. For three time periods, rates were computed to control for differences in the size of base populations. Since a constantly increasing proportion of college enrolment consists of graduate students and students in professional schools, many students today are beyond the traditional college-age group of 18 to 21 years. Accordingly, the 18–21 age group is no longer as valid a measure of student enrolment potential, and the age group 18–24 was used throughout this study as the basis for computing migration rates.[10] All rates used in this analysis are for the total student population. Raw numbers are rounded to hundreds, and percentages to whole per cents.

FINDINGS

Number and distribution of students.— The first step in this study was to determine the number and distribution of students and the volume of student migration. Patterns of college enrolment were traced from 1870, when enrolment was about 52,000, to the 1960 enrolment of over 3,500,000. The increased enrolments over this time reflect both the increase in total population and the higher rates of college attendance. In 1870, for example, college enrolments represented only 1.1 student per 100 persons aged 18–24, while the comparable figure for 1960 was 22.0. While the explanation for this increased rate of enrolment is complex, the availability of tuition-free education, due to the expansion of large, state-supported institutions, is certainly an important factor. Also, of course, increased rates of enrolment reflect the changing educational requirements of the country as the nation has moved toward even greater degrees of urbanization, industrialization, and specialization. In addition, changes in enrolment ratios over this time reflect in part the changing age distribution of both the total population and the college students themselves. The college-age group 18–24, for example, actually decreased in number between 1947 and 1955,[11] With a general consequent inflation in the enrolment ratios over that time. This indicates further, of course, that college attendance is not only a matter of college-age population potential, but also of general economic and social conditions.

The over-all picture drawn from the analysis shown in Table 1 is of a gradual shifting of the burden of higher education more evenly, as compared to the total population, across the country. The Northeast, home of the oldest and most prestigious schools, but also the victim of relatively late developing public institutions, has been losing some of its long-dominant draw upon the nation's youth to other parts of the country. Similarly, the north-central states, with their vast state university systems, are not commanding the high proportions of total national enrolments that they did thirty years ago. This gradual shifting of the college population is, of course, a reflection of the much more general and pervasive shift in the total United States population toward the West.[12] Although there has been a shift in the proportions of total national enrolments by the various regions, enrolments

[10] Earlier studies tended toward use of the 18–21 age group while at present the 18–24 age group, or both, are used extensively (see Office of Education, *Biennial Survey of Education in the United States, 1954–56*, chap. iv, Sec. 1 [Washington, D.C.: Government Printing Office, 1958], p. 9).

[11] *Ibid.*, p. 8, Table 3.

[12] From Table 1 we see that several of the proportionate shifts in enrolment are due largely to changes in the distribution of population aged 18–24. But while the major enrolment shifts reflect this over-all population redistribution, they are clearly not a result merely of this latter factor. For example, between 1930 and 1958, the north-central states experienced a 5 per cent decline in enrolment, but only a 1 per cent decline in population aged 18–24. Similarly, the South and West gained proportionately more students than can be accounted for by college-age population shifts alone.

in every state and every region have been increasing every year in all types of institutions, public and private, big and small.

Underlying the distribution of college students and the gradual shift toward the West of an increasing proportion of the national total is the factor of availability of facilities to accommodate increasingly large numbers of college students. Generally, those states that early developed state-wide public institutions have been in a favorable position to educate the young people of their own respective states. As the population generally has increased throughout the country and more and more young people have wanted a college education, the older private schools have not kept pace in increasing their capacity and/or willingness to accomodate the increased demand. Particularly in the East, where the development of public colleges has lagged behind the rest of the country, this factor has left little choice to many would-be college students other than to leave their home states in search of available facilities elsewhere. The data do not illustrate in any clear way exactly what portion of student migration is due to necessity and what portion is due to other factors. Even though a considerable portion of student migration is probably a direct consequence of students' seeking opportunities for higher education outside their home states owing to limited facilities at home, in a number of states student migration is clearly not simply a response to a need for out-of-state facilities. That is, a large volume of student out-of-state movement results from an exchange between two states or two regions with a relatively small net change in total student redistribution. Apparently many students attend college out of state for reasons other than a lack of opportunity for enrolment at home.

TABLE 1

PROPORTIONS OF TOTAL ENROLMENT, TOTAL POPULATION, AND TOTAL POPULATION AGED 18-24, BY REGIONS AND DIVISIONS, 1930-31 AND 1958-59*

REGIONS AND DIVISIONS	1930-31			1958-59*		
	College Enrolment	Total Population	Total Population Aged 18-24	College Enrolment	Total Population	Total Population Aged 18-24
Northeast	28	28	27	26	25	23
New England	7	7	6	7	6	6
Middle Atlantic	22	21	21	19	19	18
North central	34	32	30	29	29	29
East north central	19	21	20	20	20	20
West north central	14	11	11	9	9	9
South	26	31	34	27	31	34
South Atlantic	11	13	14	11	15	16
East south central	6	8	9	6	7	8
West south central	9	10	11	10	11	10
West	12	10	9	18	16	14
Mountain	4	3	3	5	4	3
Pacific	9	7	6	13	11	11

* 1960 Census data were used for computing proportions of total population.
Source: United States Bureau of the Census, *Current Population Reports*, Ser. P-25, Nos. 132 and 227; and United States Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957* (Washington, D.C., 1960), Table Series A 123-180.

Student migration.—Since 1930, as shown in Table 2, about 20 per cent of college students have found it necessary or desirable to migrate to another state. A detailed analysis of the volume and rates of student migration reveals that a relatively small number of states accounts for a large proportion of the total

student migration, and that proportionate student migration is not merely a function of proportionate college enrolment. That is, several states, such as New York, New Jersey, and Connecticut, have proportionately more student out-migrants than is accounted for by their proportions of total college enrolment. In addition, no state at present commands the high proportion of total student in-migration such as that enjoyed by Massachusetts at an earlier time. The total volume of student in-migration is being spread more evenly throughout the country, a clear secular trend since 1887 being discernible. A smaller number of states today accounts for a larger proportion of the total student out-migration, while a larger number of states accounts for about the same proportion of total student in-migration. It is noteworthy that rankings of the states on the basis of absolute numbers differ significantly from rankings based on migration rates. Nonetheless, whether expressed as a rate or in terms of absolute numbers, the heavy student exodus from Connecticut and New Jersey and the large in-migration into Indiana and Colorado, for example, stand out as extreme instances. The above data are listed in Tables 3 and 4.

Interestingly, the same small group of states—New York, Massachusetts, Pennsylvania, Ohio, and Illinois—has long accounted for relatively large proportions of both in- and out-migration of students. As expected, most of the heavy net student in-migration states in 1887 were in the East. In subsequent decades, however, several of these states in turn lost their position among the top ten in-migration states to north-central states, notably Indiana, Michigan, and Missouri, and by 1958 two western states, Colorado and Utah, were also among the leading states with net student gains.

Generally, although data for 1887–1920 are not completely comparable with later data, there has probably been a slight long-run decrease in the proportion of students who have attended college outside their home states. And while the proportion of migrants to total enrolment, since 1930, has held at about 20 per cent, there has been, of course, considerable variation among individual states over this time. Data for 1958–59, for example, show that sixteen states experienced increases in the proportion of their students going out of

TABLE 2

NUMBER OF STUDENTS AND STUDENT MIGRANTS, AND PER CENT OF STUDENT MIGRANTS TO TOTAL ENROLMENT, 1887–1958

Academic Year	Enrolment (000's)	Migrants (000's)	Per Cent
1887–88*	36.1	9.7	27
1896–97*	85.7	20.9	24
1920–21*	448.3	118.8	27
1930–31	1,006.2	195.9	20
1938–39	1,231.2	236.4	19
1949–50	2,486.2	496.9	20
1958–59	2,854.5	528.7	19

* Excludes students in teacher-training institutions.

state between 1949 and 1958. The percentages of students who left their home states in 1958–59 are given in Table 5.

Public versus private schools.—The majority of students who migrate do so to attend private schools. Private-school students, in fact, are about twice as migratory as students seeking an education in public institutions. In 1958, though private schools enrolled only 43 per cent of all students in the country, they accounted for 68 per cent of all college student migrants. The probable reason for this is that students who wish to attend private schools are more likely to be able to afford to move out of state for that purpose. In addition to this economic selectivity, private colleges are not geographically distributed as evenly as public schools, the result being that private-school destinations are more likely to be in other states.

Another finding was that the same group of states tends to rank highest in volume of out-migration to both public and private schools, while the states with heaviest volumes of in-migration vary considerably between public and private school attend-

ance. More specifically, New York, Illinois, California, Pennsylvania, New Jersey, and Ohio all ranked among the top ten out-migration states for both public and private school enrolment in 1958. For student in-migration, however, there was a considerable difference between the states ranking highest for public schools and those ranking highest for private schools. Only California, Indiana, and Ohio ranked among the top ten states of in-migration for both public and private institutions. Besides these, the highest ranking in-migration states for private schools in 1958 were New York, Massachusetts, Pennsylvania, Illinois, Missouri, Tennessee, and Connecticut, and those for public colleges and universities were Michigan, Colorado, Virginia, Texas, Kansas, Wisconsin, and Iowa.

Table 6 shows some interesting contrasts in student migration between public and private enrolments. Massachusetts, which ranked near the top in total net out-migration to public schools, ranked first in total net in-migration to private schools, reflecting, of course, the strength of the state's private institutions and the relative weakness of its public schools. The other examples of high net out-migration to public schools coupled with high net in-migration to private schools are Pennsylvania and Missouri. Reversing this situation, Michigan and Maryland ranked high in total net out-migration to private schools as well as in total net in-

TABLE 3

TEN STATES OF GREATEST NET STUDENT LOSS AND NET STUDENT GAIN, 1887, 1930, AND 1958

Rank	1887–88		1930–31		1958–59	
	State	No.	State	No.	State	No.
	Net Student Loss (000's)					
1...	N.Y.	0.7	N.J.	15.9	N.J.	37.9
2...	Tex.	.3	Tex.	2.1	N.Y.	20.2
3...	Mo.	.3	Iowa	2.0	Ill.	14.9
4...	Ill.	.2	Miss.	1.9	Conn.	7.7
5...	La.	.2	N.C.	1.8	Fla.	5.2
6...	Miss.	.2	Conn.	1.6	Md.	4.0
7...	6 tied*	0.1	W.Va.	1.6	Va.	3.8
8...			S.D.	1.5	Idaho	3.1
9...			Fla.	1.4	Ark.	2.4
10...			Okla.	1.4	Mont.	1.6
	Net Student Gain (000's)					
1...	Tenn.	0.9	Mass.	10.1	D.C.	20.0
2...	Mass.	.7	D.C.	7.1	Mass.	18.7
3...	Conn.	.6	N.Y.	3.8	Ind.	14.7
4...	Mich.	.5	Md.	3.3	Colo.	8.0
5...	D.C.	.4	Va.	3.2	Tenn.	7.0
6...	Md.	.3	Tenn.	2.4	Utah	6.7
7...	Kan.	.1	Neb.	1.7	N.C.	5.5
8...	N.J.	.1	S.C.	1.5	Vt.	3.3
9...	Va.	.1	Me.	1.5	Mich.	3.3
10...	R.I.	0.05	Ind.	1.1	Me.	2.8

* Ark., Iowa, Minn., Pa., Vt., Wis.

migration to public schools. Obviously the national pattern of student migration is quite different for private and for public enrolments, with the greatest number of private-school students migrating to the northeastern and north-central states and public school students migrating in largest volume primarily to the north central and western states. The geographic distribution of the outstanding private and public universities is, of course, reflected in the patterns of student migration to each type of institution.

Level of training and specialization.— The higher the level of training involved, the higher the migration rates become. The proportion of graduate students who migrate is larger than that of undergraduates and the rates for students enrolled in professional schools are higher than either. While 19 per cent of all students were migrants in 1958, only 17 per cent of all undergraduates migrated, as opposed to 24 per cent of graduate students. Students in professional colleges were most migratory of all, with 31 per cent going out of state. This analysis revealed great variations among states for student migration, depending upon the level of enrolment being considered. In 1922, for example, there were only ten states of net in-migration for graduate enrolment. There were fourteen in 1949 and eighteen in 1958. The greatest gains in this respect have been made by west south-central and Mountain states, where graduate schools have developed late relative to the rest of the country. These data

TABLE 4

TEN STATES WITH HIGHEST NET STUDENT OUT-MIGRATION AND NET STUDENT IN-MIGRATION RATES, 1887, 1930, AND 1958

Rank	1887–88		1930–31		1958–59	
	State	Rate	State	Rate	State	Rate
	Net Out-Migration Rates					
1....	Nev.	5	N.J.	32	N.J.	87
2....	Okla.	4	Del.	30	Nev.	67
3....	Ariz.	3	Wyo.	20	Wyo.	50
4....	Del.	3	Idaho	19	Idaho	46
5....	Vt.	3	Mont.	18	Conn.	42
6....	Fla.	2	S.D.	18	Del.	31
7....	La.	1	Conn.	9	Mont.	26
8....	Colo.	1	Fla.	7	Ill.	18
9....	Utah	1	W.Va.	7	Fla.	16
10....	Daks.*	1	Iowa	7	N.D.	16
	Net In-Migration Rates					
1....	D.C.	11	D.C.	110	D.C.	312
2....	Conn.	6	Mass.	21	Vt.	88
3....	Tenn.	4	Md.	16	Utah	70
4....	Mass.	3	N.H.	16	Colo.	60
5....	Md.	2	Neb.	10	Mass.	45
6....	Mich.	2	Tenn.	7	N.H.	36
7....	Kan.	1	S.C.	6	Ind.	35
8....	R.I.	1	Vt.	6	R.I.	34
9....	N.J.	0.5	Mich.	3	Tenn.	18
10....	Va.	0.5	Me.	3	Ariz.	17

* In 1887, data for North and South Dakota were tabulated as one unit.

clearly demonstrate the differential for enrolment at different levels of training and specialization.

Sex differentials.—A slightly greater proportion of male than of female students migrated in 1958. While males comprised 65 per cent of the total college population, they accounted for 67 per cent of the total student migration. And, although the number of net student out-migration states has decreased for both sexes, the decrease has been more marked for women than for men. This is probably because at an earlier time higher education was more selective of women in the upper classes than it is today, the result being that women who went to college tended to go to a relatively small nucleus of prestigious private schools located primarily in the northeastern region of the country. And, of course, the greater availability of public education today means that a greater proportion of women can remain in their home states.

TABLE 5

PERCENTAGE OF STUDENTS WHO LEFT STATE OF RESIDENCE, BY STATES, 1958–59

State	Per Cent	State	Per Cent
Del.	47	Mass.	20
N.J.	44	W.Va.	20
Nev.	42	Ga.	19
Idaho	42	Ky.	19
Va.	40	Wis.	19
N.H.	39	Ore.	19
Conn.	39	N.Y.	18
Md.	39	Minn.	18
Vt.	38	Ohio	17
Wyo.	36	Ind.	17
Me.	35	Colo.	17
R.I.	35	Tenn.	17
D.C.	29	Kan.	16
Mont.	25	Ala.	15
Iowa	25	Miss.	15
Fla.	24	N.C.	14
Ill.	23	Wash.	14
N.D.	23	Ariz.	13
S.D.	22	Okla.	12
Mo.	22	Mich.	10
N.M.	22	La.	10
Ark.	21	Utah	9
S.C.	21	Calif.	8
Neb.	20	Tex.	7
Pa.	20		

Interregional patterns.—An analysis of intra-and interregional patterns of student migration revealed a striking change in the pattern of student movement between 1922 and 1958. As seen from Table 7, by 1958 the 1922 pattern had gone full circle, with the Northeast, which had been the sole in-migration region in 1922, now the only region of net student out-migration. The north-central and southern regions had been joined by the West as regions of net student gains.

TABLE 6

TEN STATES OF GREATEST NET STUDENT LOSS AND NET STUDENT GAIN, FOR PUBLIC AND PRIVATE SCHOOLS SEPARATELY, 1958–59

Rank	Private Schools		Public Schools	
	State	No.	State	No.
	Net Student Loss			
1	N.J.	31.6	N.Y.	12.3
2	N.Y.	7.9	Ill.	11.5
3	Conn.	6.4	Pa.	6.5
4	Md.	5.7	N.J.	6.3
5	Va.	5.3	Mass.	3.9
6	Mich.	5.0	Mo.	2.4
7	Ill.	3.4	Fla.	2.2
8	Fla.	3.0	Minn.	1.3
9	N.D.	2.0	Ark.	1.3
10	Idaho	1.9	Idaho	1.2
	Net Student Gain			
1	Mass.	22.6	Mich.	8.3
2	D.C.	18.8	Colo.	7.9
3	Ind.	10.8	Ind.	4.0
4	Pa.	7.6	Calif.	3.8
5	Tenn.	6.2	Ariz.	3.4
6	Mo.	5.2	Utah	2.2
7	Utah	4.5	Kan.	2.1
8	N.C.	4.3	Okla.	1.8
9	R.I.	2.6	Ohio	1.8
10	Tex.	2.4	Md.	1.7

The most dramatic increase in net student gains from all parts of the country has occurred in the West. In 1922, this region had net losses to the other three regions, but by 1958 it experienced net gains from each of the others. Through-

out the thirty-six years between 1922 and 1958, interregional student migration has undergone a transition in the following pattern: (1) From before the turn of the century until some time in the 1920's, the Northeast had a heavy net student in-migration from all parts of the country. This was followed by (2) the transition of the South to a region of heavy net in-migration and a gradual tapering off of the heavy in-migration into the Northeast, followed shortly by a net in-migration to the north-central region from the Northeast. Finally, (3) since World War II the Northeast has experienced a complete transition to a region of net student out-migration, and the West to a region of net in-migration. The north-central and southern regions, during the past twenty years, have continued to receive about the same proportions of their total student in-migrants from the West, but have sent increasingly large proportions of their total student out-migrants to that region. From a heavy net in-migration to the Northeast, the center of greatest proportionate net student gains has shifted, first to the South, later and more slightly to the north-central states, and most recently toward the West.

TABLE 7

PATTERNS OF INTRA- AND INTERREGIONAL STUDENT MIGRATION, 1922-23 AND 1958-59

	Northeast		North Central		South		West	
	1922	1958	1922	1958	1922	1958	1922	1958
	Total Migration (000's)							
Northeast:								
Out-migration	30.6	107.5	5.8	36.4	7.2	35.6	.6	9.0
In-migration	30.6	107.5	9.0	18.6	6.7	18.0	1.9	6.1
Net migration	......	....	3.2	−17.8	− .5	−17.6	1.3	−2.9
North central:								
Out-migration	9.0	18.6	25.5	92.9	4.7	22.7	2.2	17.8
In-migration	5.8	36.4	25.5	92.9	5.6	20.8	3.1	11.7
Net migration	−3.2	17.8	......	....	.9	− 1.9	.9	−6.1
South:								
Out-migration	6.6	18.0	5.6	20.8	17.3	82.6	.8	9.1
In-migration	7.2	35.6	4.7	22.7	17.3	82.6	1.1	6.8
Net migration	.4	17.6	− .9	1.9	......	....	.3	−2.3
West:								
Out-migration	1.9	6.1	3.1	11.7	1.1	6.8	4.8	33.1
In-migration	.6	9.0	2.2	17.8	.8	9.1	4.8	33.1
Net migration	−1.3	2.9	− .9	6.1	− .3	2.3	......	......
	Per Cent of Total Migration							
Northeast:								
Out-migration	70	57	13	19	16	19	1	5
In-migration	64	72	19	12	14	12	4	4
North central:								
Out-migration	22	12	62	61	11	15	5	12
In-migration	15	23	64	57	14	13	8	7
South:								
Out-migration	22	14	19	16	57	63	3	7
In-migration	24	24	16	15	57	5	4	5
West:								
Out-migration	17	11	29	20	10	12	44	57
In-migration	7	13	26	15	9	13	58	48

The analysis also revealed a wavelike phenomenon of student migration across the country, from east to west, with large percentage increases in student in-migration to the north-central from the northeastern region coupled with large percentage increases in student in-migration to the western from the north-central states. This westward pattern has also been taking place along the southern tier of states, with the South Atlantic states experiencing net student losses to the east south-central division, and the west south central to the Mountain division. There has, of course, been an eastward counterflow of students from the West, largely to the north-central states, and a northward counterflow from the South, also largely to the north-central region. These counterflows, however, have since 1922 been amounting to increasingly small proportions of the total western and southern student out-migration, while the movements westward have been on the increase.

An analysis of individual divisions within the major regions for 1958–59 disclosed that only the Middle Atlantic division (comprising New York, New Jersey, and Pennsylvania) experienced net student losses to each of the other eight divisions. Every other division, in fact, had at least a slight net student gain from total student migration. New England continues to exert a very great pull upon the nation's youth, but even in this division certain states have large student out-migrations. The Middle Atlantic states, along with Connecticut, historically relied to such an extent upon their private schools that recent college-population pressure has literally forced many would-be students to go to other parts of the country. That is, if all student in-migrants to these states were turned away but all student out-migrants from these states were similarly turned away from other states, it is doubtful if New York, New Jersey, and Connecticut could accommodate the enrolment pressures that would ensue.

There is a definite and obvious drift in higher education toward larger proportions of total enrolment in tax-supported schools.[13] This drift is, of course, moving student migrants in the direction of states that have large and extensive state university systems. The states emerging as regional centers of higher education tend to be those with such facilities: Michigan, Indiana, and Missouri in the north-central region; the District of Columbia, North Carolina, Tennessee, and to a lesser extent, Texas in the South; Colorado, Utah, and Arizona in the Mountain division; and California in the Pacific area. The northeastern states, with the oldest and most famous schools in the country, are losing out in terms of sheer numbers as well as in proportion of total student in-migration. Massachusetts, of course, continues to attract students from all sections of the country for enrolment in the state's numerous private institutions. But even this state is a net out-migration state for public college enrolment.

Student versus total migration.—A comparison of student migration with total population migration showed that while the major stream westward across both the northern and southern tiers of states is of major importance for both migrations, student migration assumed a westward direction only recently. Other major patterns of total migration, however, such as from the South to the North, and toward the Gulf Coast, are not yet, at least, accompanied by comparable movements of students. In addition, very little agreement was found among individual states between net gains and losses from general migration and from migration of students.

When interregional migration of college graduates within the total population is analyzed separately, however, there is a close agreement between the migration of this segment of the population and the total student migration.[14] The Northeast,

[13] Office of Education, *Biennial Survey*, p. 46.

[14] Bureau of the Census, *U.S. Census of Population: 1950*, Vol. IV, Special Reports, Part 4, chap.

for example, experienced a net loss of migrants between 1949 and 1950 in exchange with each of the other three regions. The north-central region had a net gain of college-graduate migrants only from the Northeast, while the South had net gains from each of the other regions. The West experienced a net gain from the northeastern and north-central regions, coupled with a slight net loss to the South.

Economic variables.—Students of migration have long been concerned with theory that might explain migratory behavior. Several well-known studies have shown that migrants tend to seek greater economic "opportunities" and that total migration is usually highly correlated with such variables as per capita income and per capita retail sales. These same variables, however, are not, by and large, similarly correlated with student migration.[15] This suggests once again that the factors underlying these two migrations are separate and distinct. This finding underscores similar findings by other studies that the migration patterns of subgroups within the total population do not necessarily follow those for the population generally. Different migrations are selective of populations with varying characteristics and of populations seeking varying kinds of opportunities. This analysis indicates that migration of the college population is generally toward opportunities that are not highly correlated with those of the total population. Colleges and universities that attract out-of-state students may or may not be located in states of high income or in states of greatest over-all economic opportunities. The historical increase in the college-age population, and in the proportion of this population enrolled in college, has exerted strong pressures on the now-limited opportunities for student migrants in many states of earlier net student in-migration. While these states may still offer over-all economic opportunities to migrants in general, student migrants in search of specific and well-defined opportunities for higher education have necessarily as well as by choice moved to wherever these specific opportunities may be found.

D, *Population Mobility—Characteristics of Migrants* (Washington, D.C.: Government Printing Office, 1957), Table 14, p. 4D-328.

[15] Net interstate migration of the total population for the period April 1, 1950, to July 1, 1958, was highly correlated with 1959 per capita personal income ($r = .598$) and 1959 per capita retail sales ($r = .592$), both correlations significant at the 5 per cent level. Data for these computations were taken from *Rankings of the States, 1961* (Research Report 1961-R1 [Washington, D.C.: Research Division, National Education Association of the United States, January, 1961]).

DISCUSSION

There are so many imponderables associated with future estimates of college student migration that the present study can do no more than demonstrate the great need for further research along these lines. Under existing patterns of student migration and existing estimates of future enrolment, the number of student migrants within ten to fifteen years will probably be two to three times what it was in 1958. Further increases in out-of-state tuitions, or increasing pressure for college attendance from within the states of present heavy student in-migration, could conceivably stem the tide of migrants. So could rapidly developed and expanded state institutions in states of present heavy student out-migration. On the other hand, increased college-age population pressure in present areas of high student out-migration could conceivably push student migration to even greater proportions.

If junior college enrolments continue to increase in proportion to total college enrolments, the net result might be a decrease in student migration. In 1957–58, for example, there were 667 junior colleges, of which 391 were public and 276 private.[16] And nearly 90 per cent of the total enrolment was in the public institu-

[16] Leland L. Medsker, *The Junior College: Progress and Prospect* (New York: McGraw-Hill Book Co., 1960), p. 12.

tions. Furthermore, since the development of two-year colleges seems to tend toward locally controlled and supported "community" colleges, we would expect many potential student migrants to remain nearer home if this trend continues. This could also conceivably reduce the proportion of interstate commuting students, who probably inflate the number of student migrants in the first place. Many students may cross state lines in commuting to college, as for example from New Jersey to New York City, and thus be counted as student migrants. The data do not permit an analysis of this factor but it should be kept in mind as a limitation of the present findings.

While the migration patterns of college graduates are known, we do not know the relationship between place of college graduation and place of postgraduate employment at a later time. However, the close agreement between patterns of migration for college graduates and for college student migrants indicates that states and regions whose educational facilities draw large numbers of out-of-state students also tend to gain more college graduates than they lose through interregional migration. It would seem that higher educational opportunities and employment opportunities for college graduates are closely related.

The implications of this study relate to numerous demographic, educational, and social questions. There is the question, for example, of the advantages or disadvantages resulting from high or low rates of student migration. It may be that states of heavy student in-migration are drawing to them young people of superior intelligence and educational attainment who will, in the long run, more than repay the state of their college attendance for the cost incurred by its taxpayers for their education. Where enrolment is in private institutions, of course, the costs to the state are negligible or non-existent. In this case, the contributions of out-of-state students to the local and state economies is probably a very important consideration to states gaining through student exchanges. In addition, if private institutions tend to draw into given states superior young people who remain in significant number after graduation within the state of their enrolment, the ultimate contribution of the private institutions to the long-run welfare of the states may be very great indeed. There is also the question of the state and local communities bearing the costs of educating young people through high school and perhaps through college as well and then losing them to other states. Obviously the questions posed here are not easily answered. But a necessary starting point is determining the extent to which students tend, after graduation, to remain in the state or region of their college enrolment, to return to the state of their precollege residence, or to go elsewhere.

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CONSENSUS AND MUTUAL ATTRACTION IN NATURAL CLIQUES A STUDY OF ADOLESCENT DRINKERS[1]

C. NORMAN ALEXANDER, JR.

ABSTRACT

The drinking behaviors and attitudes toward alcohol of adolescent males in natural cliques provide a substantive focus for the investigation of several hypotheses derived from balance theory. It is found that in collectivities of high mutual attraction there is a tendency toward consensus in drinking behavior; that, if such consensus is attained, group standards arise to regulate and legitimate attitudes and behaviors related to alcohol usage; and that, if consensus is not achieved, cliques are less attractive and tend to reject the deviant member. The results support the predictive utility of balance theory in a real-life interaction situation.

The influence of the group on the behaviors and attitudes of its individual members has long been a subject for theoretical speculation and empirical research.[2] Recently, among social psychologists, there appears to be an emergent agreement as to the nature of those processes leading to consensus among individuals who are highly attracted to one another. Heider's theory of balance,[3] as formalized by Cartwright and Harary,[4] is probably the most systematic general statement of this theoretical position; and, as Davis[5] has shown, the theory serves to organize and systematize a wide variety of empirical research findings and theoretical generalizations.

Clear, detailed, and axiomatic presentations of the model may be found elsewhere,[6] and the reader unfamiliar with the theory is referred to these sources. In the present paper only a cursory summary is offered. It is reasoned that the necessities of adjustment to the environment require that some degree of perceptual constancy—consistent and stable attitudes and orientations—be maintained with regard to it. The socialized human organism comes to depend upon agreement with others in establishing and maintaining stable attitudes and orientations toward himself, others, and nonperson objects (which may be norms, values, or any cognitive unit) in the environment. Thus, the theory postulates that an uncomfortable psychological condition, "strain"[7] (tension, stress, etc.), results from a perceived disparity between one's own attitude and an attitude attributed to an attractive other with respect to an important object of perceived common relevance.

[1] This research was initiated under support of National Institute of Mental Health Grant M-4302, Ernest Q. Campbell, principal investigator; the analysis was conducted under a grant to the author from the Division of Temperance and General Welfare, General Board of Christian Social Concerns of the Methodist Church. I am indebted to Ernest Q. Campbell for his advice and suggestions and for the use of his data, and to Richard L. Simpson for his critical reading of the preliminary draft of this paper.

[2] An excellent short review of major studies is given by Edward A. Shils, "The Study of the Primary Group," *The Policy Sciences*, ed. Daniel Lerner and Harold D. Lasswell (Stanford, Calif.: Stanford University Press, 1951), pp. 44–69.

[3] Fritz Heider, *The Psychology of Interpersonal Relations* (New York: John Wiley & Sons, 1958).

[4] Dorwin Cartwright and Frank Harary, "Structural Balance: A Generalization of Heider's Theory," in *Group Dynamics*, ed. Dorwin Cartwright and Alvin Zander (2d ed.; Evanston, Ill.: Row, Peterson, & Co., 1960), pp. 705–26.

[5] James A. Davis, "Structural Balance, Mechanical Solidarity, and Interpersonal Relations," *American Journal of Sociology*, LXIII (1963), 444–62.

[6] *Ibid.* Also, Cartwright and Harary, *op. cit.* The present discussion follows very closely the theory as presented by Theodore M. Newcomb, *The Acquaintance Process* (New York: Holt, Rinehart, & Winston), 1961.

[7] Newcomb, *op. cit.*

Strain reduction in individuals necessitates a minimization of *perceived* discrepancies in attitudes (allowing for perceptual distortion). Since this paper concerns consensus among the members of cliques, the focus of interest is upon the results of attempts to reduce strain by the members of a high-attraction collectivity; and in collectivities of high mutual attraction, strain reduction is accomplished by a minimization of *actual* discrepancies in attitudes. At this level of analysis, individual attempts to reduce strain create what Newcomb has called "forces toward balance."[8] These forces arise in a collectivity whenever there exist differences in the attitudes of the members toward an important object of common relevance. They lead the group members to act to reduce the discrepancy or lack of consensus regarding that object—provided the group continues to exist and the object continues to be one of importance and common relevance. The strength of forces toward balance varies directly with (1) the degree of positive attraction among group members, (2) the importance (valence) of the object, (3) the extent to which attitudes toward the object are of common relevance, and (4) the extent of actual disagreement that exists concerning the object. Reduction of these forces may occur through reduction of any or all of these four variables.

If one assumes that the object of orientation will continue to be important and of common relevance to group members, then a lack of consensus among them will lead to one of two things—either to the attainment of consensus or to a reduction in the degree of mutual attraction (which may mean dissolution of the group as a whole or a redefinition of its boundaries to exclude the deviant members). Thus, given the continued common relevance and importance of an object of orientation, the group either reaches consensus or changes its composition.

This brief outline suffices to present the essential ideas, but not the formal elegance, of balance theory. From its basic propositions, the present paper derives specific, testable hypotheses concerning the relationships in natural cliques between mutual attraction and consensus with regard to a single social object. The focus of concern is upon the formation and maintenance of an informal clique relative to the development of consensus and the enforcement of conformity to group standards.

Now the social psychologist working in the laboratory with contrived groups is able to control the relevant objects of orientation so that the relationships among the members are contingent only upon a single, important object of common relevance. However, when dealing with natural groups, and particularly if these are informal, friendship cliques, it is obvious that a large number of objects of common relevance exist. Thus, if the balance-theory model is to provide a basis for prediction of behaviors and attitudes in actual life situations, it is necessary that groups, as a result of changes in attraction and/or attitudes, tend to achieve consensus with regard to every important object of common relevance. The present research provides an example of the utility of applying the balance theory model to a real-life situation when predictions concern a single object of orientation.

The data concern the drinking behaviors and the attitudes toward alcohol of male adolescents as they are influenced by membership in informal cliques. The region from which the respondents are drawn is one in which the dominant religious groups—Southern Baptist, Methodist, and Presbyterian—promulgate total abstinence from alcohol as a moral norm.[9] Despite the abstinence prescriptions of parents and church, more than one-third of the males

[8] *Ibid.*

[9] Unfortunately, there are no survey data available to support this contention. Official church positions require abstinence, but, like official university positions, these may or may not be enforced. The best indication that the contention is true is the observation that over half of the respondents in the larger study (approximately 5,200 high-school seniors) report that they believe that God will punish them for using alcohol.

in this sample have used alcohol at some time during the past year. Given the character of the larger society's normative milieu with regard to alcohol usage, it would seem likely that strong social pressures to drink exist within the adolescent society.

Previous research in the area of adolescent drinking behavior has shown that drinking is predominantly a social activity.[10] Of particular relevance to the present study is Hollingshead's discussion of social pressures in drinking cliques in Elmtown.[11] These findings provide evidence that drinking and attitudes toward alcohol are "social objects" of at least some importance to adolescent males and support anticipations that forces toward balance are operative in adolescent cliques with respect to the use of alcohol. The predominantly negative social definition of adolescent drinking in the area of the country where these data were gathered[12] should increase the opportunities for fruitful observations of attitude formation and change.

If an individual in this environment drinks, it is anticipated that abstinence by a friend is interpreted as an indication that this friend disapproves of drinking (and of drinkers). This fact, together with the simple difference in behavior with regard to an activity of potential common relevance, is expected to create strain. Assuming that the individual continues to drink socially (drinking remains an object of importance and common relevance), he can reduce strain only by associating with another drinker (changing his interpersonal attractions) or by persuading his friend to drink also. The persuasive communications would make the issue of drinking versus abstinence important to the non-drinking friend and of common relevance to himself and his drinking associate. This process should establish the importance and the common relevance of drinking among the members of a clique, thus instituting forces toward balance.

Application of the balance-theory model to the empirical situation produces several expectations which will be formulated as hypotheses in the following section. First, it is expected that strong tendencies toward uniformity of behavior exist in cliques of high mutual attraction, that is, there is a tendency either for all to drink or for none to do so. Now if all the members of a clique drink—if consensus is achieved with regard to overt behavior—then it is possible and likely that group standards will arise to regulate other aspects of drinking behavior, that is, *what* is consumed, *how often*, and to *what effect* (state of intoxication). Moreover, if all clique members drink, the use of alcohol may be considered a group "goal" or activity, and it is anticipated that norms develop to legitimate the use of alcohol by these adolescents (in opposition to the prevailing abstinence norms in the larger social milieu). Thus it is expected that clique-supported drinkers have more positive attitudes toward alcohol than drinkers lacking social support from their clique associates. Finally, if some, but not all, clique members drink—if consensus is not achieved—mutual attraction among group members is predicted to decrease. It is anticipated that non-uniform cliques evince a tendency to reject the deviant member, to redefine group boundaries so as to exclude him. Also, if there is lack of consensus in a clique, the members of the clique are expected to be less attractive,

[10] See the following studies: George L. Maddox, "Teenage Drinking in the U.S.," in *Society, Culture, and Drinking Patterns*, ed. David J. Pittman and Charles R. Snyder (New York: John Wiley & Sons, 1962), pp. 230–45; E. M. Rogers, "Reference Group Influences on Student Drinking Behavior," *Quarterly Journal of Studies on Alcohol*, XXIX (1958), 244–54.; A. D. Slater, "A Study of the Use of Alcoholic Beverages among High School Students in Iowa," *Quarterly Journal of Studies on Alcohol*, XIII (1952), 78–86; Christopher Sower, "Teen-Age Drinking as Group Behavior," *Quarterly Journal of Studies on Alcohol*, XX (1959), 655–68.

[11] August B. Hollingshead, *Elmtown's Youth* (New York: John Wiley & Sons, 1949), pp. 321–23.

[12] Only among *those who drink*, 54.7 per cent report that their parents would *never* permit them to use alcohol, and 40.4 per cent concur with the statement, "Drinking is wrong as a matter of principle; people ought not to drink."

less popular, than members of cliques in which consensus is found to exist.

These expectations may be summarized succinctly in general terms. With regard to an object of importance and common relevance in a clique of high mutual attraction, the following is predicted:

1. There is a tendency toward uniformity of behavior and attitudes toward the object
2. If uniformity is achieved, consensual group standards arise to regulate activities and attitudes related to the object
3. If uniformity is not achieved, mutual attraction decreases, producing
   a) a decrease in the attractiveness of the group
   b) a tendency to reject the deviant member.

TABLE 1

DISTRIBUTION OF CLIQUES AND CLIQUE MEMBERS, BY TYPE OF CLIQUE AND DRINKING BEHAVIOR

Clique Type	No. of Cliques	No. of Abstainers	No. of Drinkers
All drink..........	7	0	34
Some drink........	15	38	36
None drink......	15	66	0
Total*........	37	104	70

* Of 1,410 respondents, 177 made no codable choices; another 279 had no reciprocal relationships; 382 had only one reciprocal relationship. Of the remaining 572, 178 were found to be clique members. However, one clique member failed to answer the questions concerning his use of alcohol; thus he and three non-drinkers in the same clique were excluded from the analysis, leaving 174 members in 37 cliques.

DATA ANALYSIS

In connection with a larger study,[13] questionnaires were given to male seniors in thirty high schools in the eastern and Piedmont sections of North Carolina.[14] Each respondent was asked the following question: "What students here in school *of your own sex* do you go around with most often?" Up to five choices were coded for each case; a choice was considered codable if it was directed to another member of the high-school senior class who returned a signed questionnaire. Cliques were delineated on the basis of these choices. A clique is defined as a group of four or more members, each of whom has either (1) three reciprocal relations with other members or (2) two reciprocal relations, both of which are with members who have three. Between any two clique members there must be at least two connecting paths which do not contain the same reciprocal choices.[15] (A path is defined as a connection between two members by a series of reciprocal choices.)

The drinking behavior of an adolescent is determined by his response to the following question: "Do you now (since the beginning of this school year) drink alcoholic beverages in any form?" Those giving affirmative responses are "drinkers." Balance theory predicts that uniformity of behavior is attained in high-attraction collectivities.

Hypothesis 1: All members of each clique tend either to drink or to abstain.

The distribution of cliques and clique members by the drinking behaviors of members is presented in Table 1.

In order to determine the probability of uniformity of behavior, the number of errors made by predicting the modal behavior in each clique is compared with the average number of errors made by predicting the

[13] "Normative Controls and the Social Use of Alcohol," National Institute of Mental Health Grant M-4302, under the direction of Ernest Q. Campbell.

[14] Questionnaires were given to 5,115 seniors of both sexes in sixty-two high schools. The sample in this paper includes only males in the thirty high schools that met the following criteria: (1) more than 15 males responded; (2) more than 95 per cent of the males gave their names; (3) more than 90 per cent of the males completed the questionnaire; (4) more than one-third of the males planned to go to college. The fourth criterion resulted in the elimination of only one school which would otherwise not have been eliminated. The criterion was included because there are certain data (in the larger study) available only for those adolescents who reported plans to attend college.

[15] C. Norman Alexander, Jr., "A Method for Processing Sociometric Data," *Sociometry*, XXVI (1963), 268–69.

modal behavior for (an infinite number of) aggregates of the same size drawn randomly from the respective social systems, that is, high-school senior classes. It should be noted that expected frequencies for the drinking behaviors of clique members are based on the incidence of drinking behavior within the school class in which these cliques are found. Hence differences by rural-urban residence, size and socioeconomic level of the school, and other "contextual" variables are held constant.

Since the modal response category is predicted, the maximum number of possible errors in prediction equals approximately one-half the total number of clique members. The proportion of actual errors made of all possible errors is 0.69 for randomly drawn aggregates and 0.24 for cliques. Thus, 65.9 per cent fewer errors are made when predicting the drinking behavior of clique members than when predicting the behavior of individuals in randomly drawn aggregates. The probability that the extent of uniformity observed in cliques could have occurred by chance is less than 0.001 by a significance of difference of proportions test.[16]

While these data are consistent with the postulated operation of interpersonal forces toward balance, they do not serve to demonstrate that these exist. As previous authors have noted,[17] the existence of group standards cannot be demonstrated solely by the observation of behavioral uniformity in a collectivity. Thus additional evidence is offered for the existence of group norms.

If members of cliques in which all drink do not drink together, or if drinking is not an activity relevant to the group, then no special similarity of patterns of consumption would be expected. However, it is anticipated that, if all drink, group norms arise to regulate and control drinking behaviors.

Hypothesis 2: Drinkers in cliques in which all drink show greater uniformity regarding what is consumed, frequency of consumption, and most extreme effect (state of intoxication) of consumption, than drinkers in cliques in which more than half, but not all, drink.

The responses of drinking members of cliques to the following questions are examined:

1. *What consumed*: (Do you now drink alcoholic beverages in any form?) "Does this include both beer and whiskey, or just one of them?"

   Responses: Both, only beer, only whiskey.
2. *Frequency of consumption:* "How often have you used alcohol during the last month?"

   Responses: Not at all, only once or twice, about once a week, more than once a week.
3. *Effects of consumption:* "Check the most extreme effect drinking has ever had on you: No noticeable effect; I felt 'high' or 'gay'; I became tight; I became drunk; I passed out."

In each clique the modal response category is predicted; the number of errors made in these predictions is compared with the number of errors made predicting the modal response category for aggregates of drinkers of the same size drawn randomly from the same social systems. The percentages for each question by type of clique, shown in Table 2 below, represent the extent to which predictions for cliques are more accurate than those for randomly drawn aggregates.[18]

The existence in every case of greater intraclique uniformity in cliques in which all drink is viewed as evidence for the exist-

[16] The proportions of errors made in predicting uniformity for cliques and for randomly drawn aggregates were compared by means of significance of difference of proportions test. Cliques were found to differ significantly from random aggregates in the direction of behavior uniformity.

[17] Leon Festinger, Stanley Schachter, and Kurt Back, *Social Pressures in Informal Groups* (New York: Harper & Bros., 1950), p. 99.

[18] The formula: [(number of errors, random aggregates) minus (number of errors, cliques)] divided by (number of errors, random aggregates).

ence of group standards that regulate drinking behaviors.

In addition, it is anticipated that if cliques are able to achieve uniformity of behavior regarding the use or non-use of alcohol, then the formation of social support for legitimation of drinking behavior is facilitated. Consequently, it is expected that positive changes of attitude are more likely to occur in members of cliques in which all drink than in members of cliques in which more than half, but not all, drink. Unfortunately, there are no data regarding the past attitudes of the adolescents. However, it is reasonable to assume that the adolescent who reports that his parents would *never* permit him to use alcohol has been exposed to abstinence norms by his parents and is likely to have had negative attitudes toward alcohol in the past. Thus the difference observed between the attitudes of adolescents in cliques where all drink and the attitudes of those in cliques where most drink is probably the result of clique influences. Differences due to peer influences are also expected when parents are perceived as permissive; however, the differences should be less pronounced.

TABLE 2

IMPROVEMENT OVER CHANCE PREDICTION OF DRINKING PATTERNS, BY UNIFORMITY OF CLIQUE DRINKING BEHAVIORS

(Per Cent)

Drinking Patterns	All Drink ($N=34$)	Most Drink ($N=22$)
What consumed	32.1	−2.2
Frequency of use	29.8	10.4
Effect of use	28.4	15.9

TABLE 3

LEGITIMATION OF DRINKING BY DRINKERS, BY PERCEIVED PARENTAL PERMISSIVENESS AND UNIFORMITY OF CLIQUE DRINKING BEHAVIOR

Clique Uniformity and Parental Permissiveness	Legitimation of Drinking (Per Cent)			
	Legitimate	Non-Legitimate	N	P
Parents permissive:				
All drink	82.6	17.4	23	N.S.
Most drink	66.7	33.3	6	
Parents Non-permissive:				
All drink	90.0	10.0	10	<.01
Most drink	33.3	66.7	15	

Hypothesis 3: Those in cliques in which all drink are more likely to legitimate their use of alcohol than are those in cliques in which more than half, but not all, drink, when differences in perceived parental permissiveness are held constant.

The following question was asked: "So far as moderate drinking by other people your age is concerned, do you think this is all right if they want to do it?" Responses: No, it's not all right; all right for boys but not for girls; yes, it's all right. The data for drinkers by type of clique membership and perceived parental attitudes are presented in Table 3.

There are clear-cut, unequivocal differences in the expected direction. When parents' attitudes toward their son's use of alcohol are held constant, adolescents in cliques in which all drink have more positive attitudes toward peer drinking than those in cliques in which more than half, but not all, drink ($P < .01$ by Fisher's Exact Test). Also, as expected, the differences are smaller (non-significant) among those whose parents are perceived as permissive.[19]

[19] The actual question asked is: "Parents sometimes tell their children that when a certain time comes they can then decide things for themselves. On *drinking alcohol*, which of the following represents the way your parents feel about it?"

Permissive: I can drink if I want to while I am in high school . . . when I graduate from high school . . . when I am 18 . . . when I am 21 . . . after I set up a home of my own.

Non-permissive: They'd never give their permission for me to use alcohol.

Thus it has been shown that there is a tendency for adolescent cliques to be uniform in drinking behavior. When this uniformity is achieved, the clique members tend to legitimate their use of alcohol (independently of their perceptions of parental permissiveness) and to be more similar in patterns of consumption. These several findings, consistent with hypotheses derived from balance theory, lend strong support to the expectation that forces toward balance result in the formation and enforcement of group standards with regard to drinking behaviors and attitudes toward alcohol. Consideration is now given to balance-theory expectations that failure to achieve consensus produces changes in the level of attraction among group members and in the level of attractiveness of the group.

First, it is expected that in non-uniform cliques there is a tendency to reject the deviant member. Since these cliques are very cohesive, as a result of the operational definition of clique, some indication of acceptance-rejection is needed which will allow discrimination among clique members. It is reasoned that choices of associates tend to be listed in order of preference, that is, the most attractive choice first, the next most attractive second, and so forth. Thus choices are weighted according to the order in which they were listed—from a weight of 5 for the first choice down to a weight of 1 for the fifth choice.[20] The average of weighted member-to-member choices is computed for each clique in which there was only one deviant.[21] If there are tendencies to reject the deviant, the sum of weighted choices received by him will be less than the average of weighted choices received by members of his clique.

Hypothesis 4: In cliques in which there is one deviant member (with respect to drinking behavior), the sum of the weighted choices received by the deviant is less than the average received by members of his clique.

There are nine cliques with one deviant; in five of these, the deviant abstains while the rest drink, and, in four, the deviant drinks while the rest abstain. In eight of the nine, the deviant is below average in the sum of weighted choices received. Assuming that the deviant is equally likely to be above or below the average, $P < .01$ by sign test. Since it is probable that those who are of lower status within a group are more likely to deviate from group standards, it is not possible to conclude from this evidence that deviance with respect to drinking behavior promotes rejection. However, the data are consistent with the hypothesized association.

In addition, it is expected that uniformity of behavior is most readily achieved in cliques of relatively high attractiveness and, correspondingly, that cliques which fail to achieve consensus are (or become) relatively less attractive. Given the available data, there is no method by which the *degree* of attraction of the members of the group can be measured directly. The use of "group cohesiveness" is not acceptable since this is not independent of tendencies to reject the deviant member in non-uniform cliques; also, the use of attraction among group members to measure group attractiveness has been (justly) criticized as circular.[22]

[20] The weights are assigned arbitrarily. Some justification for assuming that choices were listed in order of preference is found in the observation that the proportion of choices that are reciprocated decreases steadily from first to fifth choice.

[21] Most of the cliques had four members. Since it is impossible to speak of deviants when the clique is divided equally between drinkers and nondrinkers, it was decided to predict only for those cliques with one deviant.

[22] Robert S. Albert, "Comments on the Scientific Function of the Concepts of Cohesiveness," *American Journal of Sociology*, LIX (1953), 231–34; Bernice Eisman, "Some Operational Measures of Cohesiveness and Their Interrelations," *Human Relations*, XII (1959), 183–89; Paul F. Lazarsfeld, "Problems in Methodology," in *Sociology Today*, ed. Robert K. Merton, Leonard Broom, and

In order to avoid these problems it is proposed that in certain cases it is possible to define attractiveness of the group in terms of the attraction of potential members, membership-eligibles, to the members of the group. In this case, "attractiveness" is almost identical with the informal status of the group within the larger social system. While there is no *necessary* relation between group "status" and group "attractiveness" (insofar as group members are concerned), it is reasonable to expect that there is usually such a relation in the case of informal, friendship groups with non-rigid boundaries. Thus attractiveness of the group is indexed by the attraction of membership-eligibles to the group members.

TABLE 4

AVERAGE NUMBER OF NON-MEMBER CHOICES RECEIVED BY CLIQUE MEMBERS, BY UNIFORMITY OF DRINKING BEHAVIOR IN CLIQUE

Clique Uniformity	Average No. of Choices Received	*N*	*P*
Non-uniform	2.081	74	<.01
Uniform	2.580	100	
All drink	2.206	34	
All abstain	2.773	66	

Obviously, the applicability of this method of measuring group attractiveness is limited to situations in which there are membership-eligibles, but this sort of situation is frequently encountered with friendship cliques in informal social systems. While the index does utilize data secured from the same sociometric choice question, the attractiveness value obtained is independent of the level of intragroup attractions and hence of tendencies to reject the deviant.

Balance theory predicts that cliques that achieve uniformity are more attractive than cliques that do not.

Hypothesis 5: Uniform cliques are more attractive to membership-eligibles than those in which drinking behavior is not uniform.

The number of choices received by each clique member from non-members is computed for those in uniform cliques (either all drink or all abstain) and for those in non-uniform cliques. Table 4 shows the average number of non-member choices received by the members of uniform and non-uniform cliques. By a significance of difference between means test, $P < .01$ in the expected direction.

It has been shown that in cliques that are not uniform in drinking behavior there is a tendency to reject the deviant member. A measure of group attractiveness independent of the level of intragroup attraction permitted the testing of the additional hypothesis that cliques lacking consensus on drinking behavior are less attractive to membership-eligibles than cliques that exhibit consensus. The data strongly supported this hypothesis.

SUMMARY AND CONCLUSION

The present research, involving drinking behaviors and attitudes toward alcohol among male adolescents in high-attraction, natural cliques, has investigated several hypotheses derived from balance theory. It was found that, with regard to a social object of importance and common relevance, consensus tends to be reached in cliques of high mutual attraction. When there is consensus among clique members, group standards arise to regulate and legitimate attitudes and activities related to the object. When consensus is not attained, the clique is observed to be less attractive and, independently, to evince a tendency to reject the deviant member.

Leonard S. Cottrell, Jr. (New York: Basic Books, 1959), esp. pp. 55–59, 65; Neal Gross and William E. Martin, "On Group Cohesiveness"; Stanley Schachter, "Comment"; Gross and Martin, "Rejoinder"; *American Journal of Sociology*, LVII (1952), 546–64.

While laboratory experiments have demonstrated the existence of forces toward balance in contrived groups, the present effort shows that the balance-theory model has predictive value in a real-life interaction situation involving a single, social object. Drinking among adolescents, especially when it violates parental and cultural sanctions, may be considered a form of deviant behavior, relative to the norms of the community. However, this does not imply that the individual drinker lacks positive social support for his actions; in fact, the drinker may be the "conformist" while the non-drinker all of whose friends drink is the "deviant," relative to his peer group. As this research has shown, such "non-conformity" by a group member is associated with differences in the extent of in-group attraction and in the attractiveness of the group to membership-eligibles. Moreover, the resulting lack of consensus with regard to behavior seemingly hinders the ability of the members to achieve group standards regulating and legitimating their behaviors.

A failure to attain group norms means that there can be little effective exercise of social control over members' behaviors. When the behavior concerned is defined as deviant by the community, the individuals who "deviate" are unlikely to have "role models" for their behavior other than deviant ones. Thus, without immediate social support from parents or peers, they are likely to define their own behavior as "wrong" and themselves as deviants. The consequences of these definitions of the self and the drinking situation should have important implications for the individual's future drinking patterns.

The most pressing need in this area, as in so many other areas of sociology, is for studies of a longitudinal nature. Only through such designs can one specify the changes that occur. For example, it may make important differences whether cliques are formed because those who drink become attracted to each other or whether members of previously formed cliques begin drinking due to social pressure from their fellow members. And it would certainly be of concern to the individual if he were left out of important group activities or even rejected by the group because of his non-conforming behavior. These problems, as well as others involving attitudinal and behavioral changes, cannot be solved until more intensive, longitudinal research is undertaken. The results of this analysis suggest that study of these changes as they relate to changes in the individual's peer associates would be substantively and theoretically fruitful.

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MEXICAN SOCIAL TYPES[1]

ORRIN E. KLAPP

ABSTRACT

Mexico, like any country, has a social type system as part of its informal structure. Hero types state positive norms of the ethos. Such themes are found also in public images of admired leaders. Villain and fool types state negative norms. Though some types are universal, the system provides an important key to national character and structural differences among societies.

Slang is a barrier that quickly reminds the newcomer that however well he knows the formal language, he still has much to learn about a country. Its importance is that it labels informal social structure and types of persons and roles that might be missed if one confined his attention to relationships recognized in "good usage." Yet, however useful it may be to natives, most are not proud of their slang and not especially eager to reveal it. Perhaps this is one reason why so little is known of the social types of most countries, which make up, as I hold, an informal system of hundreds of role-norms and models that undergird formal structure and have important functions—such as role-discrimination, status-adjustment, social control, and self-modeling—far beyond what is usually thought of in connection with stereotypes.[2] From a cultural standpoint, social types state the ethos and help make up a group's self-image—hence, in some way reflect national character.[3]

This study aims only to dip into the Mexican type system by identifying some of the more common and salient[4] types

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[2] See O. E. Klapp, *Heroes, Villains and Fools; the Changing American Character* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1962). As here conceived, stereotypes are only part of the social-type system—the comparatively inaccurate popular images of outsiders and unfamiliar situations (which may have functions for conflict, ethnocentric feeling, or stratification); within the social system typing is more accurate, i.e., inaccuracy is a result of lack of interaction. Thus Mexicans stereotype Americans as *gringos* and Europeans as *gabachos;* but they have more realistic concepts of other Mexicans. Likewise, Anglo-Americans stereotype Mexicans, for example, as improvident, undependable, irresponsible, childlike, indolent, willing to give votes to whoever will buy them, unclean, prone to drunkenness, criminal, immoral, deceitful, mysterious, unpredictable, hostile to Americans, musical, romantic, always ready for fiesta, loving flowers and growing them under the most adverse conditions (Ozzie G. Simmons, "Mutual Images and Expectations of Anglo-Americans and Mexicans, *Daedalus,* Spring, 1961, pp. 286–99).

[3] A relatively obscure question, on which social types shed some light by giving an idea of the range of models available, that is, an American forms his identity from a different stock of models from that of a Swede, a Russian, a Mexican, even an Englishman. This should make some difference in the kinds of character and self-images actually found in a population. I am not saying that there is a Mexican national type. Authorities such as Carlos A. Echanove-T. emphatically deny that a country as heterogeneous as Mexico could have an ethnic or national type: "No hay un caracter mexicano porque no hay un tipo mexicano. . . . Mexico es un mosaico de razas y de culturas. No me refiero sólo a los indios, por supuesto. Me refiero sobre todo al mestizaje, que de ninguna manera debe ser considerado como un solo tipo de mestizaje sino como constitúido por una gran variedad de tipos mestizos" (personal letter). But the impact of types, for example of urban types on rural migrants, must be taken into account, even though they lead to heterogeneity rather than homogeneity.

[4] By salient, I mean uppermost in people's minds, readily thought of. Many types are not named in dictionaries nor often in public communication,

that emerged from interviews.[5] The presumption is that most important social types are named in idiom that people actually use when they talk about and characterize each other. The procedure was to compile a list of type-names from slang, then present it to Mexicans for comment and classification.[6] As it "snowballed," the list was subdivided for further interviewing. Subjects were also given lists of American social types and asked if they could think of Mexican equivalents. In the following discussion, Mexican types are presented in parallel with American types, as a semantic bridge—not to imply that they are precisely equivalent or that there is a corresponding Mexican for every American type.

literature and art; some are *sub rosa* or not mentioned in the presence of women; some, though well enough known by educated Mexicans, are shunned as not worthy of the best in Mexico. An individual is aware at a given moment of only a fraction of the types he actually uses. So to say that a sample was statistically representative might require techniques combining Gallup and Kinsey. Yet salient types, like the mountains of a country, are the first an explorer is likely to observe, and characteristic of the country, though there is much more.

[5] One hundred and nine interviews were obtained from adults living in the Tijuana–Ensenada–Mexicali–San Diego area who identified themselves as Mexican and had lived at least fifteen years in Mexico. The period of interviewing was July, 1961, to February, 1962. Composition of sample follows. Locality in which raised: Baja California, 30 (Tijuana 16, Mexicali 7, Ensenada 6, other 1); Federal District, Mexico City 23; Jalisco 12 (Guadalajara 11, Puerto Vallarta 1); Sonora 10 (Nogales 7, Guaymas 3); Sinaloa 4 (Mazatlan 1, other 3); Aguascalientes 3; Chihuahua 3; Tamaulipas (Tampico) 2; Durango 2; Zacatexas 1; Michoacan 1; Nayarit 1; undetermined 17. Highest level of school attended: university 48, secondary 34, primary 18, undetermined 9. Sex: 71 males, 38 females. I am indebted to the following current and former students of San Diego State College who corrected and amplified interview data: Alexander Iñigo, Ruben Dominquez, Pierre Debbaudt, Sergio Miranda, Sergio Noriega, Mario Muñoz, and Aminta and Maria Lara. Interviewing was done in English. This dilemma in intercultural study must be faced: either the interviewer translates, or the people interviewed do the translating. In either case, results are contaminated by American experience. Obviously, in a study such as this, there can be no claim to deal with nuances of language; our concern is with broad categories to which sometimes several names apply. Especially does this apply to parallels between Mexican and American types.

HERO TYPES

Heroes, as models of the "way to be," state positive themes of the ethos. Certain types stand out: a man who is *muy macho* (strong, virile, valiente, stubborn, fuerte),[7] a man's man and a girl's ideal beau. He is described as "a real man, good drinker, lover, singer, fighter, brave and willing to defend what he believes in." Another comments: "A real Mexican type. Always boasting he is a real man, sometimes has an inferiority complex." Tarzan, well known in Mexico, illustrates macho in a crude way.[8] Also admired is shrewdness, keenness, that makes a man quick to seize opportunities, efficient and successful: *un aguila, muy listo* (abusado, agusado). In the United States he might be called a smoothie, smart operator, or fox.[9] He could

[6] As "heroes" (models of the way to be), "villains," "fools," or none of these. This classification is explained in Klapp, *op. cit.* The focus of this study is on types that function as models, positive or negative, so those without marked approbation or disapprobation are not included. The classification actually presented in Spanish was: (1) poco familiar, no conocido; (2) bueno, admirable o heroico; (3) malo, vil o pícaro; (4) tonto o ridículo; (5) ninguna de estos. Subjects could classify types into more than one category if they wished.

[7] Because of the large number of Spanish words used in this paper, I shall at this point depart from the convention of italicizing foreign words and, instead, italicize for emphasis only.

[8] A variant, no doubt, of the strong man admired in all cultures. See my "The Folk Hero," *Journal of American Folklore*, January–March, 1949, pp. 17–25.

[9] In both Mexico and the United States, the type is ambivalent, that is, both admired and condemned. During his heyday Pancho Villa was called "El Zorro de la Sierra Madre," when he baffled efforts of the American Army to catch him. To Mexicans, however, zorro has more negative connotations (con-man, estafador) than does fox in the United States.

be a sharp businessman, or called político (diplomático) because he is suave in handling people. It does not take much imagination to see this as one version of a universal type.[10]

Un caballero has social graces and charm for women, elegant manners combined with courage. "A man with class. He can probably fight but he won't unless he has to." The traditional hero's role of *defensor* (campeón, paladín) might be played by any good man, exemplified, to the educated at least, by figures like Bolívar, El Cid.[11] There is also high respect for a man of intellect, *genio, sabiondo* (inteligentazo, piensa grande, una lumbrera), in North America called a brain. Socrates is recognized as an example, though not a popular type.[12] Institutions like the mordida (customary bribe) do not keep Mexicans from recognizing the very "good" man, *muy moral* (honesto, honrado, impecable, incorrupto, puritano) in politics or elsewhere. He is better than an ordinary buen hombre (who is good only "as man goes"). A type corresponding somewhat to the American do-gooder is reflected in terms like *benefactor* (caritativo, acomedido, bondadoso, buena persona, buena gente, Samaritano), the one who is exceptionally kind and helpful, not only to friends[13] but to all. *Un buen Juan* (bonachón) seems similar to the American Good Joe, a regular fellow with many friends, easy to get along with, kindhearted, sincere, artless, maybe rather simple (sencillo, Juan Llanas). Also similar is un bato a toda madre (a swell guy with many friends).

Then there is *Juan Tenorio* (Don Juan),[14] the great lover, Romeo with the women, sometimes referred to humorously (in speech and popular song) as gavilán pollero (hawk in the chickenhouse) or dueño de las mujeres (because of the number of women who depend on him).

The man of real power and importance, of course, is *el jefe* (jefazo, general, caudillo, el rey, el mero mero, el mero chingón), "the real boss, the big boss." This is not just a formal title but an indicator of where real influence lies. Offsetting somewhat the oligarchic tone of el jefe is the man who is recognized as *persona justa, muy demócrata,* very fair-minded, who does not impose his will arbitrarily upon others even when powerful. Perhaps this is as close as Mexican popular thought gets to the democrat as a heroic ideal.

Other favorable types might be called *independent spirits:* the ambicioso (luchón, luchador) who struggles to succeed (seemingly the Mexican equivalent of the go-getter, self-made man); the life-of-the-party, gay spirit (alegre, alegrón);[15] the daredevil or plunger (atrevido, intrepido, arriesgado, arrojadizo, aventado, entrón) who sticks his neck out and takes chances recklessly—related somewhat to the ace-of-the-wheel (as del volante), an expert driver or hot-rodder; finally, the free spirit (persona libre, rebelde, inconforme, liberal), admired for his independence, except in certain contexts.[16] Judging from such types, there seems in Mexico to be distinct tolerance for a rebel or free spirit, at least among men. Finally, there are very slangy terms for a kind of man who in America

[10] Klapp, "The Clever Hero," *Journal of American Folklore,* LXVII (January–March, 1954), 21–34.

[11] Emiliano Zapata was also conceived as a great man fighting for the people. See Robert Redfield, *Tepoztlán, a Mexican Village* (Chicago: University of Chicago Press, 1930), pp. 197–204.

[12] To Mexicans with little schooling, he may be only "a man in a book, a king or something."

[13] A Mexican is probably more obligated to do favors for kin and compadres (buddies) than in the United States, and his hospitality is often large. The point is that whatever the standard expectations of generosity, a hero must go beyond.

[14] See Samuel M. Waxman, "The Don Juan Legend in Literature," *Journal of American Folklore,* XXI (1908), 184–204.

[15] Though "parrandero" condemns the drunkard or playboy who paints the town red.

[16] As one informant pointed out, *es un poco libre* is not a recommendation for a servant nor family member. Also the freedom of women is markedly limited; see villain types hereinafter described.

would be called a cool cat or sharp guy: *bato chingon* (un bato a toda madre), not a fashion plate but well dressed and "in the know."

Salient models for women include: *la joya*, the perfect woman, beautiful, virtuous, whom a man can be proud to possess; *esposa abnegada*—the devoted, self-effacing wife; *encantadora*, a woman of bewitching beauty and glamor, corresponding to what in America might be called the charmer;[17] *la reina*, the queen among women; and the popular conception of muchacha chula (chulada, buenota, un monumento, un mango), whom informants described in American slang as a sexy, good-looking chick. This type, of course—as, indeed, most of those listed above—represents a man's point of view, though women recognize it and some model themselves accordingly. However, as one informant pointed out, though Mexican men may like a sexy girl, when they marry they often prefer a modest little dove.

In general, there seem to be fewer hero types for women than men in Mexico (and, as will be seen below, probably fewer villains, too), perhaps reflecting restriction of women's role opportunities and the greater mobility and freedom of men. By surveying such types we identify alternative role models, also possible role conflicts. In Mexico, as in the United States, formal ideals held up by school, government, or church are not necessarily in agreement with the thought of the common people; for example, Juan Tenorio can hardly be officially sanctioned in a monogamous Christian country, though he thrives in many. Again, people may admire a man who, though he does wrong, is smart or audacious enough to get away with it and despise a stupid though honest bungler. Social-type analysis helps us to see such unofficial sides of popular morality.

Heroes, as models of the way to be, are a clue to what a Mexican is likely to do and whom he is likely to admire or accept as a friend. It seems reasonable to say that, were a person to embody many of the themes mentioned above, he would be popular among Mexicans.

[17] Or, in the 1920's, the vamp, but without the sinister implications of this type.

PUBLIC IMAGES OF ADMIRED FIGURES

In this light let us look at what Mexicans see in five well-known figures. My own preliminary explorations of their public images are given in Table 1. Pancho Villa is known as a kind of Robin Hood,[18] who might be expected to embody themes such as un hombre muy macho, defensor, aguila or zorro, benefactor of the poor, buen Juan (a hearty good Joe), a rather rough-and-ready Juan Tenorio, and, of course, el jefe.

One can hardly discuss the heroic ideals of a Spanish-speaking people without getting into the question of what symbols such as Don Quixote and the bullfighter mean. Two former presidents of Mexico were chosen: Miguel Alemán, about whom opinion seems rather divided, and Benito Juárez, who has sometimes been called the Abraham Lincoln of Mexico. Table 1 shows traits often attributed to these figures by a sample of Mexicans.

This sample suggests that Pancho Villa is pretty much the man to Mexicans that Americans think he is—a Robin Hood–like hero, whose favorable traits outrank his unfavorable ones. The profile of Don Quixote also fits fairly well what informed people of any country would expect: a gallant man, ready to serve others, idealistic, but rather a fool—except for the "Juan Tenorio" attribute (hardly the picture of a knight devoted to his Dulcinea as portrayed by Cervantes). The themes attributed to Miguel Alemán give some basis for saying that he epitomizes to many Mexicans the crooked politician, though good traits are also attributed to him. From these data, at least, the image must be classed as ambivalent. By contrast, Benito

[18] As one informant put it, he was "Robin Hood plus a little more—he lived hard, loved hard, and died hard." Another called him a "great man."

TABLE 1

How Mexicans Think of Five Well-Known Figures

(Themes Attributed in Rank Order [$N = 81$])*

Rank	Character	No.†
	Pancho Villa	
1	Revolucionario rebelde	76
2	Macho, fuerte, valiente	58
3	Un águila, listo	34
4	Juan Tenorio, gavilán pollero, dueño de las mujeres	33
5	Defensor, campeón, paladín	26
6	Benefactor, caritativo, acomedido, Samaritano	21
7.5	Sinvergüenza, pillo	15
7.5	Un buen Juan	15
	Don Quixote	
1	Un caballero	53
2	Tonto o ridículo	36
3	Defensor, campeón, paladín	27
4	Benefactor, caritativo, acomedido, Samaritano	26
5	Muy moral, honesto, honrado	18
6.5	Genio, sabiondo, intelectual	16
6.5	Un buen Juan	16
8	Juan Tenorio, gavilán pollero dueño de las mujeres	13

Rank	Character	No.
	Miguel Alemán	
1	Político	66
2	Sinvergüenza, pillo	40
3	Águila, listo	27
4	Genio, sabiondo, intelectual	20
5	Un caballero	18
6	Muy demócrata, persona justa	17
7.5	Muy moral, honesto, honrado	12
7.5	Juan Tenorio, gavilán pollero, dueño de las mujeres	12
	Un torero	
1	Macho, fuerte valiente	51
2	Águila, listo	20
3	Un buen Juan	19
4	Juan Tenorio gavilán pollero, dueño de las mujeres	18
5	Defensor, campeón, paladín	13
6	Un caballero	10
7	Tonto o ridículo	9

Rank	Character	No.
	Benito Juárez	
1	Muy moral, honesto, honrado	62
2	Muy demócrata, persona justa	56
3	Genio, sabiondo, intelectual	50
4	Defensor, campeón, paladín	44
5	Benefactor, caritativo, Samaritano	39
6.5	Un caballero	28
6.5	Político	28
8	Macho, fuerte, valiente	20
9	Águila, listo	19
10	Revolucionario, rebelde	11

* Composition of sample: Eighty-one persons identifying themselves as Mexican who have lived at least 15 years in the Republi of Mexico. Twenty were polled in Mexico City, the rest in the San Diego-Tijuana-Mexicali area. Locality in which raised: Baj California 6, Federal District 26, Jalisco 14, Durango 4, Sonora 2, Sinaloa 5, Chihuahua 1, Michoacan 1, Nuevo Leon 1, Guanajuat 1, Nayarit 1, undetermined 19. Highest level of school attended: university 13, secondary or preparatory 28, primary 14, no forma schooling 1, undetermined 25. Sex: male 28, female 29, undetermined 24. I am indebted to Roberto C. Jones for data collected i Mexico City.

† Number of subjects who attributed a theme to a given figure.

Juárez reveals, as expected, the "pure" image of the political incorruptible. His attributes are uniformly favorable. It is interesting to note that "político" ranks seventh while with Alemán it is first, though both were politicians. (It may also be that only the favorable meaning of político—astute, diplomatic—applies to Juárez.) The bullfighter is, as expected, a figure primarily of courage and virility. The "Juan Tenorio" image is not surprising, either as an inference from virility or judging from the popularity of bullfighters among women. The "defensor" and "benefactor" images are consistent with the chivalrous ideal. Other elements indicate that he may not be as highly regarded as some aficionados and writers of bullfight novels would lead us to believe: águila and tonto possibly express disenchantment with the urban, professional, secularized[19] bullfighter as just an entertainer out to make money or shrewd in avoiding real risk (somewhat as North Americans view the professional wrestler). "Tonto" might be the other side of this cynicism: anyone who *really does* risk his life in such a way is stupid (the Sancho Panza view of gallantry?). The low rank of a "caballero" rating may also be significant. These observations are offered merely as illustrations of what part social types may play in image analysis of popular favorites and leaders.

NEGATIVE TYPES

Turning now to the negative side of the model system, we can identify some bad guys (malo, vil, o pícaro) prominent in Mexican thought: a *bully* who abuses people or picks fights with weaker parties (abusón, abusivo, agresivo, atravancado, bravero, pendenciero); a *troublemaker*, who stirs people up by fights or intrigues (picapleitos, puscabulla, busca ruido, buscapleitos, peleonero, alboratador, intrigante, diablo); a *con-man* who defrauds innocent people (approvechado, coyote, zorro, estafador); a *chiseler*, cheater, or hustler (chapusero, ventajoso, chueco, condenado, explotador, trinquetero, tramposo) who by some racket takes something and gives nothing; Mexicans also despise *parasites* such as: zangano, lambizcón (a sly loafer corresponding perhaps to the goldbrick or boondoggler in America), vividor (a moocher or freeloader), colero (a stooge or tagalong who plays the role of yes-man in order to freeload), aselabarba (an apple-polishing climber or kiss-up); a loafer (flojo, peresoso); and a vagrant or beggar (vago, banquetero, pedinche, pediche, pedigueño). There is also a concept of a *good-for-nothing*, a man of no value to others (inutil, un vale nada, que no sirve, uno que no vale madre), who may be classified as a villain if conceived of as a bad apple who corrupts others or a wretch (miserable) so lacking in standards that he would do anything. The *shirker* of important social obligations (irresponsible) is definitely condemned, perhaps a black sheep (oveja negra) who comes to no good and disgraces his family, or a drunkard (borachin, boracho, parrandero). Mexicans are severe toward *unmanly men*, such as the homosexual (joto, fresco, afeminado) or the pimp (padrote, palo blanco) who lives off women. They are also severe toward *women who are too free* and have lost status, especially by sexual promiscuity (mujer de la calle, puta, ramera, andariega, mujer sin dignidad, mujeracha, ofrecida[20]).

Of interest from the standpoint of American equalitarianism are types that offer limits to class presumption and the power of the oligarch or boss (jefe), such as *mandó* (mandamás), who is condemned for being too bossy (although there seems no equivalent in Mexican popular thought for authoritarianism as Americans conceive it);[21] *presumido* (snob, pedante, orgulloso),

[19] Robert Redfield, *The Folk Culture of Yucatan* (Chicago: University of Chicago Press, 1941), pp. 274–275.

[20] A flirt who offers herself.

[21] To Mexicans autoritario is a term of respect and in no sense a villain type. Most Americans, by contrast, rate an authoritarian as a villain, namely, one who is so sure that he is right that he would impose his ideas on others.

a man who sets himself above others with pride or pretension; and the *oppressor* or tyrant (tirano, despota, dictador).

Outright criminals, of course, belong among the villains, such as bandido (bandolero); sinvergüenza (shameless scoundrel); pillo (thief, crook); ratero (racketeer, gangster) pandillero (gangster); the mean one (maldito, malvado), who is worse than an ordinary criminal, a vicious killer, cursed, enemy of mankind; the human beast (odioso, chacal, brutal); also the dope fiend (adicto, morfinomano, grifo, mariguano).

Rebelde sin causa[22] is widespread in Mexico as a name for juvenile delinquent. But "rebelde" itself is not simply villainous; indeed, rebelde and revolucionario carry many connotations of respect, presumably attributable to recent Mexican history.[23]

Treason to the group or treachery to friends is hated in Mexico as in other cultures: the two-faced person (de dos caras, polifacetico); the fair-weather friend (amigo interesado, amigo por interes, amigo por convenencia, convenenciero); liars and deceivers (falso, mentiroso, hipocrita, charlatan, curandero); traitors and renegades (traicionero, traidor, renegado, Malinche,[24] Malinchista, chaquetero, Judas); cowards as potential turncoats and traitors (cobarde, miedoso, coyon, culon, gallina); and stoolpigeons and squealers (chismoso, mitotero, delatador, delator).

Then there is the corruptor (corruptor, pervertidor, aguamala, persona peligrosa), who passes evil to others and undermines character and society. He is somewhat like the American bad apple.

Some villain types have a special function as stereotypes expressing ethnocentrism and suspicion of the outsider, such as gringo (the loud, obnoxious American), gabacho (applied rather indiscriminately to Europeans, especially Frenchmen), Judio[25] (Jew), and intruso (crasher, intruder). However, ethnic stereotypes have a minor place numerically among all of the other unfavorable types, most of which could apply equally well to insiders.

Certain personality traits are disliked sufficiently to be classed often as villains, such as: pinche (a mean, stingy person); egoista (selfish); aborasado (greedy); gruñon (grouch); sangrón, chocante, antipático (a cold unfriendly person corresponding to what in American slang might be called a creep); necio (a real pest who bothers you, gets on your nerves, keeps arguing stubbornly when wrong).

Finally there is the curious ambivalent type of the rogue or scamp (bellaco, pícaro, bribon, canalla, maloso, travieso, vivo), recognized in most cultures as a villain with clownish, even heroic traits.[26] Juan Tenorio undoubtedly belongs here; on his worst side, Mexicans recognize him as a wolf (lobo, mujeriego, carnicero) looking for girls all the time. Another interesting example is *un vivo* (a live one). In favorable reference it may mean bromista, chistoso (cut-up, jokester), or movido (live wire), or águila, agusado (sharp, shrewd). Cantinflas, the well-known Mexican comedian, is often mentioned as un vivo. But it also means a dangerous man, an oppor-

[22] Derived apparently from the American movie *Rebel without a Cause,* based on the book by Robert M. Lindner.

[23] After all, many revolutionaries are still alive and the dominant political party is the Partido Revolutionario Institucional.

[24] An Aztec princess, also known as Marina, who became the mistress of Cortez and served him as interpreter, skilfully manipulating the Aztecs. See George C. Vaillant, *The Aztecs of Mexico* (New York: Penguin Books, 1950), pp. 235–39, 251. Malinche seems to have significance in Mexico similar to that of Benedict Arnold in the United States. It could apply even to a pocho (Americanized Mexican) who has lost pride in his origin and repudiated Mexico. It also bears comparison with "Uncle Tom" among American Negroes, or "kiss-up" among American teenagers (a student who plays up to a teacher to get good grades).

[25] The primary meaning of Judio, however, is probably tightwad, loan-shark (a role, as when Americans say shyster), rather than the ethnic status of Jew.

[26] Such as Eulenspiegel, Volpone, Francois Villon, Robin Hood (see "The Clever Hero," *op. cit*).

tunistic rascal such as one might expect in Renaissance Italy, for whom no act—lying, rape, stealing—is impossible for personal gain. To this kind of vivo terms like un audaz (audacious rascal), un pillo (rascal, thief) might also apply.[27]

Though there are differences of emphasis, most of these villain types do not tell much about cultural differences. Indeed, they stress the likelihood that major categories of the villain are universal, probably because the group functions (threatened by the villain) are universal. However, my impression is that Mexican vilification is more severe than American for certain categories of behavior, such as male homosexuality, and the emancipated, promiscuous, or fallen woman; and that it is somewhat less severe toward rebels, revolutionaries and Communists (Izquierdistas —left-wingers—are not markedly vilified). Perhaps our own revolutionary days are a little further back in history, or our attachment to capitalism is a little stronger. The Mexican attitude toward the político and burócrata seems more like than unlike that of Americans: most politicians are a little crooked, some are big crooks, comparatively few are incorrupto.[28]

Fools represent a less serious dimension of social evaluation, not a threat to the group but a minor role failure or personal shortcoming at which people are inclined to laugh. Mexicans classify the following types as "tonto o ridículo." Some center about mental deficiencies: stupid fools (baboso, bobo, buey, maje, menso, tonto,[29] cabeza de piedra), corresponding to the American sap, dope; the clumsy one who can't do things right (mentecato) or is too slow (torpe, tortuga); the crackpot or oddball who is loose in the head (chiflado, loco, zafado), including the crazy chick (chica loca); the big thinker who claims too much (piensa grande, super sabio), the crack-brained theorist (sabio loco). Perhaps we may place here Don Quixote, often rated by Mexicans as a high-minded fool.

Other faults lead to failure to live up to expectations: the tightwad (codo duro, agarrado)—at the opposite pole from the spendthrift, wastrel (derrochador, despilfarrador); the greenhorn (verde) who doesn't know the ropes (conoce la movida) so can't act right; the glutton (tragón, glotón) who fails in self-restraint and manners; the old fogey, foolish old man (chocho, betarro); the wet blanket or party-pooper (agua fiestas, aguador, aguitón) who is a drag to those who wish to be gay —at the opposite extreme from the cut-up or joker (bromista, chistoso, payaso), who also can be viewed as failing on some occasions to live up to obligations of sobriety and decorum.

Mexicans are especially prompt to ridicule lack of manliness: marica (maricón, mariquita, fifí, faldillero, chiquiado, mimado, delicado), the sissy or mama's boy; llorón (chillón), the crybaby, bleeding heart; joto (puto, fresco), the effeminate male; popoff, the fop; barbero (colero, coliche), the stooge, flatterer, yes-man;

[27] I am indebted to Dr. Victor Goldkind for this interpretation, with which Mexicans agreed.

[28] To the question, "Does Tijuana have an Al Capone?" one informant promptly answered with the name of a high state official. To some Mexicans, the name of a former president of Mexico is a synonym for government crook. (Of course, similar views of government officials are easily found in the United States.) Tax collection, one community leader explained, is often a "deal" between officials and businessmen, based on factors such as exchange of favors, prestige, and the ability to threaten. It is also often pointed out that Mexican policemen receive only a fraction of the salary of American policemen, and naturally will not be above payoffs. However, for a view of generous, public-spirited types to be found in Mexican community leadership, see Klapp and L. V. Padgett, "Power Structure and Decision-Making in a Mexican Border City," *American Journal of Sociology*, LXV (January, 1960), 400–406.

[29] Tonto is a broad generic term with other meanings. In one important sense, it means rustic, illiterate, from the hills. In some parts of Mexico, including rural, it means ill-mannered, ill-bred, improper as opposed to correcto: a poor, uneducated man can be correcto if he acts well. It also might be applied to a man who permitted himself to be fooled by others or who was dominated by his wife (Oscar Lewis, "Tepoztlan Restudied," *Rural Sociology*, XVIII [1953], 121–34).

aguado, the jellyfish, a "lazy fat jerk"; miedoso (nervioso), the timid, nervous, jittery man. Such types emphasize the *macho* theme previously listed among heroes. A real man may not show such weaknesses, stoop to unmanly postures, or be too fancy. The macho theme also, I think, makes Mexicans rather tolerant of a daredevil or plunger (arriesgado, arrajadizo, atrevido, aventado, intrepido) who sticks his neck out and does rash things. Although rashness is disapproved of, contempt for the opposite-fault (miedoso) is so strong that a Mexican may prefer to be a reckless fool rather than a coward.

Discounting is an important function of fool types—to "take a person down" from the value he claims for himself or role he pretends to. Vanity is well represented among such types: the bighead (cabezón, crédo, fachoso, se cree muy sabroso, se cree muy salsa), the showoff (fanfarrón), the vainglorious bragger (valentón), the swell or fop (farsante, catrín, catrinsillo, catrín sin dinero[80]), and the snob (presumido, pedante, pretencioso) who may be viewed, ambivalently, as a comic stuffed shirt or a villain.[81] Mexicans also recognize that the mouth is a great source of folly: the chatterbox (perico, tarabilla), the loudmouth (gritón), the bullshooter (hablador, bocón). Pico de oro (beak of gold) is described as a flimflammer or softsoaper, possibly an orator. Bunk-shooters, when not seriously injurious, get comic discounting by types like quack, humbug (charlatán, curandero).

Overconformers, who go "too far" in what is nominally, or in some people's opinion, the correct thing are also discounted: the prig or prude (rígido, estricto, puritano, recatado); the diehard who sticks too obstinately to a course of action or point of view (terco, mentecato); the one who goes to extremes of faith (fanático, allelula). Perhaps here is the place to mention the religious hypocrite (beata, santucha, rata de iglesia), always in church making a pious show.

Mexicans do not seem to have a very close equivalent to what Americans call an "oddball." Seeking such types, I drew names like un tipo raro, excentrico, extraño, inconforme, rebelde, and bohemio; but none who knew both American and Mexican types well felt there really was an equivalent and they often expressed the opinion that Mexicans are more matter-of-fact in accepting the one who is different and are less inclined to close the ranks against oddballs and push everyone toward the "regular fellow."[82]

All social types have structural implications, but some rather plainly reflect certain kinds of relationships, such as the category of the *one who is too nosy*, a buttinski or kibitzer, for whom Mexicans have various names: entremetido, metiche, mequetrefe, pico largo (big beak), busca nuevas (gossip-seeker); conchudo (crasher, uninvited guest), also the one who hangs about too closely, the pest (molón, encajoso) who "gets in your hair." Dislike of intruders seems plainly related to close in-group structure. It is not unreasonable to suppose that Mexican compadre relationships combined with informal loyalty to el jefe produce at the same time the stooge who hangs around and the pest who hopes to be an insider. Another category reflecting social structure is that of the *fall guy*, a dupe or easy mark (engeñada, maje, facil, inocentón, pendejo, tarugo, pelele[83]). It is reasonable to suppose that where there are many such types,

[80] "He wears a big suit, smokes a big cigar, but he has no money" (interview).

[81] When he hurts people by snubs or closes the door on privileges, he is logically viewed as an enemy by underdogs (los humildes) and, of course, socialists.

[82] "We Mexicans are more individualistic than you Americans, for all your talk about individualism." Another added the qualification: "Mexicans are individualistic and don't penalize differences of personality except in regard to being a man" (for example, a male may not wear shorts on the street because it would compromise his masculinity).

[83] In this sense, a stooge who lets others boss him; another meaning is trash.

relationships are rather exploitative; this is accepted as a fact of life and the one who does not take account of it, who lets himself be "taken," is a sap to blame for his own trouble.[84]

CONCLUSIONS

This study has barely scratched the surface of the Mexican social-type system; yet perhaps enough types have been described to show that they are not just a miscellaneous lot of images but do function for control, normative integration, and so on, as do corresponding American types.

In a sense, they speak for themselves as value judgments: One can hardly look at salient types without getting some feeling for the Mexican ethos—for example, emphasis on courage and virility (macho) and contempt for the sissy (marica). While this study does not reach conclusions about national character, I believe it implies that social types should be taken account of in any such appraisal and may offer a fruitful basis for comparative study. The suggestion is that various cultures could be compared by sorting social types along HVF dimensions as indicated in Figure 1. It may be possible, from the relative location[85] of equivalent types in "value space," to make comparative statements, such as, Mexicans take a sterner view of "sissy" (marica) than do Americans, or Americans are less tolerant of an "oddball" or "snob" than are Englishmen.

Comparatively few of the types surveyed here give clear evidence of diffusion (as do Tarzán, Romeo, Don Juan, Quijote, rebelde sin causa). And even if diffusion did occur, it would not prove that the functions were not universal (and could not have been met by indigenous typing). The existence of many parallel types in different cultures suggests that, regardless of diffusion, common life and organizational problems are being met in similar ways. Many fool and villain types are not so much a comment on a particular culture as a representation of failings of human nature (role failures) to be expected anywhere in the world.

The structural implications of social types should be further explored. They indicate informal institutionalization, what
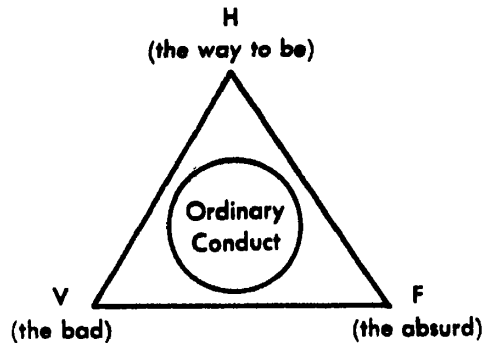
FIG. 1

is "going on," "the ropes," the "other face" of bureaucracy, and so on. A particular area might be charted by social types and then their mutual relationships investigated. For example, in Mexican society stratification is reflected in types like: the snob (presumido, pretencioso, altoso); the lower class (pobre, los de abajos, humildes, la pelusa, el pelado, peon); the oportunista looking for his chance; the dandy of high society (popoff); the man who is very equalitarian (muy demócrata); the failure or defeated one (derrocado, fracasado); the no-account, someone to be dismissed as of no importance, nadie (a jerk, a nobody, a face in the crowd), pobre diablo (a poor fool), pelele (trash), banquetero (floater). Such types reflect the vertical status dimension of success-failure and a closed rank against the outsider. Likewise informal power relationships may be re-

[84] When Sophie Tucker, the American comedienne and singer, called out to customers, "Hello, sucker!" she was stating such a structural fact of nightclub life.

[85] I am experimenting with such a method of scaling by having subjects sort Mexican and American types along HVF dimensions.

vealed[86] by types like the big shot (el jefe, el rey, manda más), the stooge (barbero, colero), the one who manipulates others (abusado), the fall guy (inocentón, engañada), the one who gets gravy without effort (lambizcón), the "comer" to watch out for (ambicioso, empuje), the one who stands above corruption and influence (persona justa, impecable).

The ambivalence of social types also deserves further study. What does it mean if people have sharply contrasting feelings about the same role? For example, some call a man who chases women Juan Tenorio (great lover) approvingly, others call him mujeriego (wolf); some call a man who makes fast deals aprovechado (con-man) and others say listo, águila (smart one) approvingly. Does ambivalence reflect loss of value-consensus in a society, or does it imply nuances from one situation to another that insiders recognize but outsiders fail to catch—in other words, that consensus is high but distinctions are fine? In one case, social type ambivalence might mean that a moral order was breaking down, possibly through urbanization and acculturation; people are unable to make reliable ethical judgments. In the other case, it could mean that in a complex social structure, perspectives change with social position,[87] role norms are clear though more relative and less universalistic. In either case, however, the hypothesis would be that the more pluralistic, acculturated, urbanized and industrialized a society, the greater the proportion of ambivalent types.

San Diego State College

[86] As suggestively done by Floyd Hunter, *Community Power Structure* (Chapel Hill: University of North Carolina Press, 1953), p. 42.

[87] For example, a man might have begun as an aprovechado and worked his way up in society to a position where he now despises that sort of thing, or he might have been a woman-chaser while single but now is the protective father of a daughter. His changing life-situation causes him to look at things with new eyes, he is ambivalent because of his status mobility.

LETTERS TO THE EDITOR

September 26, 1963

To the Editor:

I am writing a book on the University of Chicago Department of Sociology during the 1920's and early 1930's and would be grateful for suggestions concerning sources of information.

ROBERT E. L. FARIS

University of Washington

GRUSKY, "MANAGERIAL SUCCESSION AND ORGANIZATIONAL EFFECTIVENESS"

September 20, 1963

To the Editor:

I would like to compliment Oscar Grusky ("Managerial Succession and Organizational Effectiveness," *American Journal of Sociology,* LXIX [July, 1963], 21–31) for bringing to light the fact that organized athletics may have something to offer as a field for sociological analysis. This area has been almost totally neglected by sociologists, even though it affects the life of many people in the United States, either as participants or spectators.

However, I would like to question Grusky's operational measure of one important variable in his study: the effectiveness of a major-league baseball team. He contends that team standing is the best measure of effectiveness, although he admits that financial profit is also important. Yet, it would seem to me that from the standpoint of the total organization the best measure of effectiveness would be the drawing power of the team, as measured by total yearly attendance, rather than team standing. This would be true in spite of the strong positive relationship Grusky reports between these two variables, because there are obvious and glaring exceptions to it. For instance, the New York Yankees finished first in league standing thirteen times in the seventeen-year period 1946–62, yet their attendance was highest in 1946 and in 1948, when they placed third. Thus the Yankees' drawing power was at its peak when their effectiveness as measured by team standing was lowest. In 1962 the New York Mets won 40 games, lost 120, and finished last in team standing, some 60½ games behind the league leaders. They were generally considered by most expert observers to be one of the worst, if not the worst, team of all time. *But* the Mets drew more fans than a number of teams in both leagues who had higher team standings. (Data on yearly attendance may be found in the *Dope Book* [St. Louis, Mo.: Sporting News, 1963].)

If I might draw an example from the business world: as far as I can ascertain, Ford's Edsel car of a few years ago was fairly effective in mechanical performance, yet it produced little consumer interest and did not sell. I doubt whether Grusky would want to measure the effectiveness of the Ford corporation in that year in terms of the mechanical performance of the Edsel. In this case the more "effective" measure of effectiveness would seem to be whether the organization was able to sell its product. I feel the same would hold true of a baseball team.

JAMES K. SKIPPER, JR.

Child Study Center
Yale University

REJOINDER

October 3, 1963

To the Editor:

I am very grateful to Dr. Skipper for his kind comments. However, it should be noted (as I neglected to do in my study) that others, such as Charles H. Page and Hubert M. Blalock, have pointed out the importance of organized athletics for sociological study.

As Skipper is well aware, the measurement of organizational effectiveness is a complicated affair. The type of measure selected reflects the theoretical orientation of the investigator. Effectiveness may be viewed in terms of an organization's relative ability to attain its official objectives, as I preferred in my study, or its ability to satisfy the requirements of the clientele, as preferred by Skipper. Or, effectiveness may be defined in terms of the implementation of the values of the organization's executives or in terms of the type and amount of satisfactions provided for the general membership. As in the present case, these different viewpoints may yield measures of effectiveness that are closely related. Nevertheless, criteria often exist for the selection of a primary measure. Skipper argues that attendance is a better primary measure of effectiveness than team standing. His major reason appears to be that the ultimate measure of an organization's performance, be it a business firm or professional baseball team, lies in the clientele's receptivity to the "product." I feel that in comparing organizations of a similar kind, an adequate measure of effectiveness should take into consideration, to the extent possible, the major features of the type of organization studied, including the nature of its official objectives. As professional baseball organizations do have some features of business systems, profitability is a relevant criterion of effectiveness. As they also resemble public entertainment organizations, attendance is important in itself and not only because it is related to profitability. However, a number of features clearly differentiate professional baseball organizations from ordinary business and entertainment organizations. Paramount among these is the nature of the task or the official objectives. Rule 1.02 of the *Official Rules of Baseball* states: "The object of each team is to win by scoring more runs than the opponent." Team standing reflects this primary concern with winning, a concern of vital importance to the organization's executives, staff, team members, and clientele. The decisiveness of the contest and its public nature emphasize the importance of this criterion of performance. Thus I believe that the primary measure of effectiveness selected was an appropriate one.

OSCAR GRUSKY

University of California
Los Angeles

ERRATUM

The *Journal* regrets an error in the review by Oscar Grusky of Muzafer Sherif's book, *Intergroup Relations and Leadership*, which appeared in its September, 1963, issue (p. 201). The next to the last sentence should have ended with the words "no, *essential*," and *not* "not *essential*" as printed.

BOOK REVIEWS

Outsiders: Studies in the Sociology of Deviance. By HOWARD S. BECKER. New York: Free Press of Glencoe, 1963. Pp. x+179. $5.00.

Any person who wishes to become better acquainted with the major trends in contemporary sociology will profit from studying this new book by Howard S. Becker. The point of view represented in the book will not surprise sociologists already familiar with Becker's earlier papers on marijuana users and dance musicians, both of which appear here in much their original form, but the interesting work he began in those papers has been rounded out here into a relevant and well-argued position. The best way to describe Becker's approach, perhaps, is to say that it belongs to the older Chicago tradition: it is pure sociology, drawing the reader's attention wholly to the social context in which deviance occurs and paying little attention to larger abstractions like the "American culture" or smaller abstractions like "individual personality."

Becker begins his argument by noting that definitions of deviancy vary widely as we range across the various groups and classes whch make up social life. Since no single criterion can be used to decide what forms of conduct are deviant and what forms are not, we can only develop a reasonable grasp of the problem by studying the setting in which one group of persons confers a deviant label on another. "The central fact" about deviant behavior, Becker says, is that "it is created by society."

> I do not mean this in the way it is ordinarily understood, in which the causes of deviance are located in the social situation of the deviant or in "social factors" which prompt his action. I mean, rather, that *social groups create deviance by making the rules whose infraction constitutes deviance*, and by applying those rules to particular people and labeling them as outsiders. From this point of view, deviance is *not* a quality of the act a person commits, but rather a consequence of the application by others of rules and sanctions to an "offender." The deviant is one to whom that label has successfully been applied; deviant behavior is behavior that people so label [pp. 8–9].

The population covered by Becker's definition, then, includes all people who "share the label and the experience of being labeled as outsiders" (p. 10). Deviance is not a property of behavior; it is an outcome of the interaction between someone who is thought to have violated the rules of the group and others who call him to account for that delinquency.

One of the most compelling themes in the book is Becker's contrast between what he calls "simultaneous" and "sequential" models of deviation. The ingredient missing in most contemporary models of deviant behavior, he argues, is a coherent sense of the passage of time: social-science literature is full of studies which relate deviant behavior to one or another kind of background factor—the broken family is a traditional example—but few of these studies inquire whether these background factors act on the individual in any kind of orderly sequence. Becker suggests that this is indeed the case and describes the process by which an individual learns to develop a deviant style as if it were analogous to the process by which the rest of us learn a "career." People who become marijuana users or dance musicians, like those who become physicians or airline pilots, learn their trades and the behavior modes appropriate to them in a sequence of steps, a specific chronology. Assuming that most readers of this *Journal* are already familiar with Becker's work on marijuana users and dance musicians, where this argument was first spelled out, I need only add that the older papers and the newer theory fit together in a convincing fashion. The case presented is a good one and is skilfully argued.

Another important theme of the book is that the study of deviance must pay as much attention to the social condition of the rule *enforcer* as it does to that of the rule *violator*. In some respects, Becker views the interactions between the two as almost a raw contest for power. Enforcers are people with enough power to impose their moral preferences on others, and violators are people who fall victim to this authority. Marijuana users

and dance musicians (and by extension other kinds of deviants) do not feel that their way of life is improper, but they become outsiders in the sociological sense because people with other views are able to confer a deviant label on them. There is even an underlying suggestion in Becker's argument that deviant groups and the agencies which censure them live by two different cultural standards: dance musicians reject the members of their audience by calling them "square," and, according to Becker's account, the audience rejects the musicians as "deviant." Should the tables be turned (and in certain conditions they are) it is presumably possible that the deviants become the conformists and impose their moral preferences on the new minority. All of this sounds a little as if an army of occupation were imposing its rules on an oppressed populace; it certainly does not give much credit to the force of a larger culture hanging over both groups. But Becker does a fascinating job with this rather unusual thesis. He begins with the story of the Marijuana Tax Act showing that the enforcers really invented a new morality in their efforts to prosecute users of the drug, and manages to make a strong argument for his point. It makes powerful reading.

The book is rich in thoughtful concepts, many more than a review of this length could begin to discuss. Although most readers will admire both the scope and style of the book, however, there are several points which may cause some concern.

Some readers, for example, will wonder how useful it is to employ terms like "deviant" and "outsider" interchangeably. It can be done, of course, and with good effect—but from a purely tactical point of view it would seem that the kinds of social pressures which isolate dance musicians from the main flow of social life and the kinds of pressures which drive other individuals into mental hospitals or prisons are different enough to require a narrower set of definitions.

Other readers will share with me a certain disappointment that the book has so little appreciation of personality theory. I do not mind that Becker fails to talk about human motivation, but I am disturbed that his approach almost entirely rules it out as a relevant variable. "Instead of deviant motives leading to deviant behavior," he says, "it is the other way around; the deviant behavior in time produces the deviant motivation" (p. 42). There is a good deal of truth in this assertion, of course, more than is recognized by many students of personality; yet on the whole this is probably a more relevant way to look at marijuana users than at other people typically considered deviant. Few students familiar with the hospital setting will find this a compelling way to look at mental patients. And in general, the evidence that deviant styles usually fall to those who are in some way motivated to receive them is far too weighty to be dismissed so abruptly. Personality disorder may not be the most important qualification for deviant niches in the social order—even for beds in the mental hospital—but it is difficult to visualize the transactions by which people are selected for deviant labels unless we look closely at the motives of those who manage to get themselves chosen.

Finally, one of the most impressive motifs in Becker's book is also one of the most difficult to come to terms with—namely, the "career" analogy which runs throughout the text. There are many respects in which the life history of a deviant becomes clearer if we think in terms of career lines: the use of the term helps suggest that deviant styles are learned and often occur in gangs, clubs, and other kinds of miniature societies. Yet the analogy is only useful up to a point, for many of the people we call deviant become social problems exactly *because* they find it difficult to organize their own lives into a coherent pattern (even a deviant one) or to join together into protective bands. Perhaps certain groups of marijuana users and dance musicians are able to achieve this kind of organization and continuity, but many others are not. Again, the mental patient is a prominent example, and within certain limits the same can be said of many of the people who end up in prison or other institutions of control.

Something in the structure of the situation prevents deviant groups from becoming too well organized, too confident of their own group moralities, or else we would be in a perpetual state of insurrection. This "something," it seems to me, is the fact that most of the people who find themselves labeled deviant accept this label as legitimate and even help attach it to themselves. They may not like the label; they may even invent ideol-

ogies which scorn the standards used to censure them; but at some level of consciousness they too accept the legitimacy of the rules which have resulted in their fate.

These objections may seem to cover a wide scope, but they are minor all the same. The main value of Becker's book is that he remains true to his own special angle of vision on the problem and has carried the logic of that position to its richest conclusion. The book is highly recommended.

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On Revolution. By HANNAH ARENDT. New York. Viking Press, 1963. Pp. 343. $6.50.

When confronted by intuitive brilliance, there is a temptation to review the personality rather than the performance. *On Revolution* is a continuation of discussions first broached in *The Human Condition* and in *The Origins of Totalitarianism.* Since this work is something less than social science and something more than mere speculation, perhaps a prosaic ordering of Miss Arendt's materials is not only forgivable but necessary. Overlooking her contempt for the "modern debunking 'sciences' psychology and sociology," I shall state her position in propositional form and offer some lines of disagreement and further inquiry.

1. War and revolution have violence as their common denominator. Conflict derives from fratricidal instincts, while political organization has its roots in crime. Crucial to revolution in the modern age is the concurrence of the idea of freedom and the experience of new social beginnings, of apocalypse.

2. Revolution gains a new significance as war, its partner in violence, becomes an implausible way to effect social change. Total annihilation has transformed the character of the military from protector of *civitas* into a futile avenger. Even prior to the nuclear age, wars had become politically, though not yet physically, a matter of national survival because of the widespread fear that the vanquished power will suffer the subjugation of its political organization. Non-technological factors in warfare have been eliminated so that the results of war may be calculated in advance with perfect precision. Foreknowledge of victory and defeat may well end a war that need never explode into reality. If we are to survive, this cannot become a century of warfare, but it most certainly will become a century of total revolutions. The universal goal of war is revolution. But even without the possibility of limited agreements, revolution will come to define the character of the modern uses of violence and the present impulse toward freedom and liberty.

3. Revolution in the modern age has been concerned with two distinct drives: liberation (absence of restraint and increase in social mobility) and freedom (political level of life). While liberation is consonant with various forms of government, freedom is only possible through a republican form of government, which explains why the American, French, and Russian revolutions all adopted this form of rule.

4. The two fundamental models of revolution are the American and French revolutions—though only the French Revolution became *the* basic model for Marxism. The American Revolution adhered to the original purpose of revolution-making—freedom—while the French Revolution abdicated freedom in the name of historical necessity. The American Revolution was thus profoundly political and antihistorical while the French Revolution was historical and profoundly antipolitical.

5. The French revolutionary model, the model adopted by Marxism and which penetrated the ideological and organizational aspects of the Russian Revolution, was concerned with the social question—with problems of exploitation, mass alienation and poverty. It was inspired by the idea of compassion. The American revolutionary model was concerned with the political question, with problems of policies and the predicaments which flowed from an elitist theory of mass human nature. Its revolutionary passion did not give way to compassion.

6. The weaknesses of the "classic" French model are revealed in the abortive aspects of the major revolutions of the modern era—the Paris Commune, the Russian Revolution, the Hungarian uprising. In each case there was the rise of two distinctive forces: the party, acting in the name of the people, and the voluntary associations (workers' councils, soviets, communes) or the people as a collective. The force of power over the people, in the betrayal of the revolution, came through the consecration of political parties, while the council system, because it failed to realize itself as a new form of government as such (as in the American Revolution) tended to be short-lived. It is this fact which accounts for the perfidy of modern revolutionary movements—the breakdown of voluntary association and its replacement by a swollen bureaucracy.

These propositions indicate Miss Arendt's morphology of revolution. While it is not possible to argue this book's thesis in terms

of "right" and "wrong," a number of questions arise. The key problem of the book is the relative absence of evidence. How does one evaluate speculations? The abundant confidence with which *On Revolution* is written is far from persuasive. The unsystematic prose style which keeps the reader hopping about looking for the continuing threads does not enhance a ready acceptance of her perspectives.

Miss Arendt reveals little knowledge of modern warfare, that is, little about the ambiguities of modern conflict—counterinsurgency, paramilitary struggle, police action, guerrilla action—that would show that war is becoming obsolete. It might be correct to note that *thermonuclear* warfare would make total international conflict obsolete—since it is like a gun with two barrels pointing in opposite directions, with the triggering mechanism designed to kill the gunman as well as the victim. But the absence of any distinction between war and annihilation throws all of the weight of her discussion on revolution onto the questionable assumption that war is obsolete by reason of self-interest. The absence of knowledge of problems of contemporary warfare is excusable—war and peace studies are dismal—but conceit is no reply. And when the author states that "the only discussion of the war question I know which dares to face the horrors of nuclear weapons and the threat of totalitarianism, and is therefore entirely free of mental reservation, is Karl Jaspers' *The Future of Mankind*," she is only revealing her ignorance of a widespread and valuable literature which has just this relationship as its central concern. Nor is the definition of revolution particularly enlightening. To see revolution as having everywhere a violent quality is to fail to distinguish between change in social structure and strategies *sometimes* used in such changes. Even if we generously assume that Miss Arendt is speaking exclusively in terms of political revolution, it is *not* the case that violence is a necessary or sufficient component.

Contradictory statements blemish her views. "The part of the professional revolutionists usually consists not in making a revolution but in rising to power after it has broken out, and their great advantage in this power struggle lies less in their theories and mental or organizational preparation than in the simple fact that their names are the only ones which are publicly known" (p. 263). But elsewhere she says that "without Lenin's slogan 'All power to the Soviets' there would never have been an October Revolution in Russia" (p. 269). Which cliché should be believed? Miss Arendt's repeated assertion that the consequence of revolution is always less freedom and liberty than previously existed is belied by an appreciation of the *positive* outcome of the American Revolution. Indeed, it is precisely her dislike for revolutionary process that causes her to search out special features in the American Revolution not found in Europe.

Miss Arendt belongs in the unusual category of a revolutionary conservative. For although she is bent on demonstrating the negative aspects of Thermidor and Robespierre and the positive aspects of the *Federalist* papers and the founders of the American Republic, she nevertheless is seeking at the deepest level for a way to make revolutionary movements responsible to revolutionary men. Thus it is that councils of workers, soviets, and so forth are held to be useful models of voluntary control. The revolutionists constitute a "new aristocracy" which would properly spell the end of general suffrage. As she puts it: "only those who as voluntary members of an 'elementary republic' have demonstrated that they care for more than their private happiness and are concerned about the state of the world would have the right to be heard in the conduct of the business of the republic." The revolutionary elite would be guardian of the nation. How this differs from the "betrayal" of revolutions by political parties, how this guardianship could avoid becoming a political party, is not discussed. Miss Arendt respects the "spirit of revolution" but scores its failures to find an "appropriate institution." She has located such an institution in the voluntary councils which accompany revolutions, but what is amazing is her unwillingness to support her theory with evidence: for example, there is no discussion of the functioning of the Yugoslav worker councils or of the Israeli *Kibbutzim*. This is the result of a failure to confront real contemporary revolutions, to take up the political revolution of freedom in relation to the economic revolution of abundance. Her comments in this direction reveal an awareness of the potential antagonism between

conomic development and political freedom, ut not a consistent understanding of how nd where they intersect. The big question of evolution is precisely the "mix" between conomic rationalization and political reason. olarization of these may make a stimulating reatise but it cannot define the experimental haracter of most contemporary revolutions. 'or Miss Arendt, the French and American evolutions are diametric opposites. For eoples of revolutionary lands, they both tand as beacons in the search for the new. 'erhaps politics and people have been olarized in European models, and bureau-ratic and voluntary associations have be-ome sundered. But if massive revolution lefines the century, might it not be wiser to each for new combinations of policy and ublics rather than to look with nostalgia pon the Greek city-states and their prudent litism which rested, after all, on a slave ase? This question I pose for Miss Arendt, ince she has presented so many worthwhile ssues for this reviewer to contemplate.

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'he Story of an Underground: The Resistance of the Jews of Kovno (Lithuania) in the Second World War. By ZVIE A. BROWN and DOV LEVIN. (Hebrew with English translation of Table of Contents and Introduction.) Jerusalem: Yad Vashem, 1962. Pp. xvii+422.

Immediately following the second World Var some hundreds of personal reports were ritten on the Nazi destruction of European ewry and on the Jewish resistance to this estruction. These included personal ex-eriences of survivors, diaries of victims, nemoirs of Nazis who engineered the de-truction and of observers who witnessed :. Each reported the situation as it ap-eared to him personally. Two organiza-ions, Yad Vashem in Jeruslaem and YIVO Yiddish Scientific Institute) in New York, ave been collecting, maintaining reference les, and publishing bibliographies of these eports (see, e.g., Philip Friedman and Jacob obinson, *Guide to Jewish History under Vazi Impact, Bibliographical Series No. 1* [New York: Yad Vashem and Heroes Memorial Authority and YIVO Institute for Jewish Research, 1960]). More recently, these individual reports have been combined into more complete accounts of the struggle. *The Story of an Underground* is such a synthesis of materials referring to a single city.

When the Nazis conquered Lithuania in 1941, one-third of the population of Kovno was Jewish. When the Red Army returned in 1944, only 8 per cent of these Jews were still alive. In the interim a ghetto underground fought annihilation. This book traces the development of that underground. Five types of data are interwoven. The authors use their personal experience, at first in the ghetto and later with the Lithuanian Soviet Brigade in the forest. Second, they collected documents relating to underground organization in the ghetto: documents of the Council of Elders established under Nazi aegis as a Jewish government, of the Gestapo and German occupation authorities who supervised the ghetto and its liquidation, and reports of other Lithuanian undergrounds. Third, they supplemented these documents with personal testimony from individuals involved in the above groups. Fourth, they used published memoirs. Finally, ninety questionnaires were submitted to eyewitnesses to the events. This source material is constantly kept before the reader; in the book's 422 pages there are 954 footnotes. These footnotes give the sources for statements in the text, supply information about individuals mentioned (such as the postwar fate of some particular Nazi) and present conflicting evidence when the sources disagree.

Three basic themes are developed. The first concerns life in the ghetto. A description of political organization is an account of the formation and activities of the Council of Elders and its departments such as education, health, finance, and labor. Economic life revolved about forced labor, some trading both within the ghetto and with the non-Jewish population outside, and the necessary artisan production. Internal control was maintained through the Jewish police under the surveillance of the Gestapo. The existence of internal order made it easier for the Nazis to exterminate Jews but also provided conditions for the operation of the resistance. By a combination of ruse, threat, and direct

force, the Nazis first slew those held to have been associated with the Soviets, then those unable to work, then the children and the aged, and finally, most of the survivors. The Nazis were able to extract the first groups from the ghetto by pretending that they were to be sent to labor camps. The final groups had to be hunted down by the Gestapo.

The second theme concerns the underground within the ghetto, the struggle between Communist, Zionist, and other political elements for control of the resistance, the disappearance of this interparty strife with increasing Nazi pressure, and the early failure of liaison with the Lithuanian partisans in the forests due both to the Nazis' success in foiling the attempts and to early rebuffs by the partisans. Survival plans which were debated, tried, or rejected, and efforts to obtain arms are recounted. There is a detailed account of an escape from a fortress prison where the Jews were forced to exhume and burn thousands of corpses as part of the Nazi effort to erase evidence of their crime. This part includes introspective descriptions of ways in which these prisoners came to terms psychologically with their task.

The third theme concerns the experiences of the ghetto resistance fighters who succeeded in joining forest partisans. This includes a discussion of the leadership offered by Soviet parachutists and escaped prisoners of war, problems of organization of a partisan camp, housing, discipline, the position of women, forays against Lithuanians sympathetic to the Nazis and against Nazi supply lines and bases, social relations among partisan groups, especially between the Jewish and non-Jewish partisans, and the allocation of prestige according to whether the individual performed a fighting or a service function within the group. There is material here on the adaptation of an urban population to the requirements of the forest.

The personal documents that appeared after the war have proved a fruitful source of psychological generalizations about behavior under stress. Synthetic works of this type are an invaluable basis for sociological generalizations about communities under stress. Despite the personal involvement of the authors and their relation to the victims, the book is a conscientious work showing dispassionate attention to detail and judicious sifting of the more valid from the less valid reports. *The Story of an Underground* is a significant contribution to a social history of the holocaust.

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Sociological Theory, Values, and Sociocultural Change: Essays in Honor of Pitirim A. Sorokin. Edited by EDWARD A. TIRYAKIAN. New York: Free Press of Glencoe, 1963. Pp. xv+302. $5.95.

Pitirim A. Sorokin in Review. ("The American Sociological Forum.") Edited by PHILIP J. ALLEN. Durham, N.C.: Duke University Press, 1963. Pp. xxii+527. $10.00.

In these two volumes, published within a few weeks of each other in honor of Professor Sorokin's seventy-fourth birthday, some thirty-one scholars pay tribute to "a distinguished elder statesman of sociology, not only in the United States but also throughout the world"—to repeat the words of Talcott Parsons. One may now hope that the discipline is at last ready to embark upon the formidable task af assimilating the substance and import of Sorokin's contributions to sociology.

The collection of essays assembled by Tiryakian is in the pattern of a *Festschrift*. Most of the contributors were associated with Sorokin at Harvard, and they do him honor by advancing propositions and ideas drawn from their own current work. For the most part, the themes of the essays are related but loosely and tangentially to Sorokin's research and outlook. They testify more to the diffuse challenge of his remarkable intellect than to any specific impact of his theories. Indeed, in the first essay Arthur K. Davis argues that Sorokin's students and colleagues learned all too little of the "Lessons from Sorokin."

The *Festschrift* papers qualify as "theory" first and foremost by the fact that none of them is embarrassed by any systematic attention to a body of data—a usage somewhat foreign to Sorokin's own, inasmuch as he has consistently been preoccupied with the discovery, formulation, verification, and explanation of empirical uniformities in phenomena. As "interpretations" or conceptual exercises, however, several of the essays are

ıot lacking in interest, even if none is exactly nspired. The "institutionalization of values" s treated variously by Parsons, O'Dea, and Firey. Florence Kluckhohn contributes a restatement of her classification of value orientations. In characteristically instructive manner, Merton, with Elinor Barber, shows hat ambivalence has structural as well as situational and personal sources. In what is he closest approximation to a report on an nvestigation, Bernard Barber sifts the evidence for the proposition that business is becoming a profession, concluding that it is not professionalized to any great degree and s not likely to become so for some time to come. The reader's response to these and a half-dozen other essays will depend on his astes and interests.

Sorokin in Review is another matter. Here ıll contributors address themselves to one or another kind of problem in the master's vork and thought. Some content themselves vith a summary and appreciation of Sorokin's fforts in a particular field. Most, however, provide discerning and incisive critical appraisals. The level of intelligence and competence shown by the critics is high. They have he advantage, of course, of working with ich and rewarding materials. It turns out hat a very good vehicle for an essay on sex nd society, philosophy of history, or sociological measurement is a critique of Sorokin's writings on the subject.

Reviewing *Social Mobility* in the light of dvances in knowledge since 1927, Gösta Carlsson manages both to demonstrate the ermanent importance of the work itself and o set down an agenda of live topics that urrently merit attention. Merton and Bernard Barber, examining "questions that puzzled us" bout Sorokin's sociology of science, have, dvertently or otherwise, formulated a first-ate set of questions for any student of that ubject to ponder. Alex Inkeles, in the course f a sharp criticism of Sorokin's perspectives n *Russia and the United States,* lays bare he whole issue of what the comparative sociologist is to make of similarities and differences in the structure of national societies.

No less than half the contributors are oreign scholars, a fact that faithfully reects the high evaluation of Sorokin's work broad. Among their number is Arnold Toynee, whose magnificent statement on his encounter with *Social and Cultural Dynamics* may well be the most appropriate place to begin a study of Sorokin's *magnum opus.*

Sorokin is not a silent partner in this enterprise. The volume opens with a superb sociological autobiography, "Sociology of My Mental Life," which reveals facets of his development not hitherto known even to intimates of several decades. Then, in the "Reply to My Critics" that closes the book, Sorokin responds to the main points of the contributors. The discussion, though not failing to display its author's familiar vigor of expression, maintains the spirit of impersonal search for truth established in the preceding chapters. It is not evident, to me at least, that Sorokin has the best of all the arguments. Still, it could well be, as Toynbee suggests, that "Sorokin will come out strategically victorious from any number of tactical defeats."

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Propaganda and Psychological Warfare. By Terrence H. Qualter. New York: Random House, 1962. Pp. 176. $1.95.

This small book discusses with clarity and precision the historical development and present use of propaganda and psychological warfare. While a carefully documented review of the literature in this field falls short of yielding a promised "theory of propaganda," it does clarify the essential functions which propaganda performs in peace and in war, in dictatorships as well as in democracies.

The author defines propaganda as the deliberate attempt "to form, control, or alter the attitudes of other groups," through the use of symbols, so as to make them act as the propagandist desires. He thus distinguishes it from education, which teaches skills and transmits what is believed to be the truth. Advertising, on the other hand, is seen as a form of propaganda used to influence decisions about goods, services, or ideas.

Like other recent writers in the field, Professor Qualter stresses that the propagandist may employ truth or falsehood, and that his intentions may be honorable or evil. He may even utilize the educational system or commercial advertising. Propaganda, Qualter rightly maintains, threatens democratic proc-

esses only when it becomes a monopoly and succeeds in censoring all unorthodox or unpopular views.

A well-reasoned chapter relates the rapid development of propaganda in the twentieth century to the growing influence of a newly enfranchised electorate, improvement in the means of communication, the rise of nation-states, and the demands of two world wars. Another chapter is a good review of the strengths and weaknesses of different propaganda techniques.

In discussing psychological warfare—defined as "propaganda tied in and coordinated with military, political, and economic strategy and policy"—the author points to its new importance. In the nuclear age, it may become a substitute for military action and the only form of warfare the great powers can afford to wage.

Qualter performs a service for the international propagandist when he notes that psychological warfare is not just large-scale advertising. It requires detailed study of the aspirations and values of foreign peoples, a knowledge of their attitudes toward each of the major powers, an understanding of the forces shaping their opinions and of the communication channels most likely to reach different groups.

Both students and practitioners of propaganda and psychological warfare will find this book most rewarding. In view of its general excellence, some minor factual errors on the current status and organization of the United States propaganda effort can easily be forgiven.

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Mass, Class and Bureaucracy: The Evolution of Contemporary Society. By JOSEPH BENSMAN and BERNARD ROSENBERG. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. xii+548. $10.00.

There is a potential market for albums of recorded music to accompany introductory textbooks in sociology. Those texts that present a solid, scientific luster prepare the student for advanced courses and should be accompanied by marching bands, leading us all in the upward parade of progress. Those that present a more philosophical glow often seek a personal involvement and should be read against mood music or progressive jazz. The book reviewed here belongs in the last group, but its notes are all in a minor key, its mood one of Weltschmerz and its result academic and educational dissonance.

The unifying theme of this text is the disolution of folk and feudal societies and the emergence of a modern mass society. It is an old idea, set in the authors' view of modern society as destructive of human values. The effort to develop a theme of importance to ethical and moral behavior is laudable. Unfortunately it is a bad book with little regard for disciplined thought, scholarly work, or precision of statement.

Introductory texts can hardly be criticized for simplifying areas of thought or study, but the high arch of generalization is here upheld by very thin pillars of fact. Consider this rather frequent type of gratuitous overstatement: "Without some degree of warmth, no durable human interaction has ever been observed to take place" (p. 302). Apparently legal systems, economic markets, and formal association are all illusory! To this sin there is added a tone of certitude for dubious, unsubstantiated propositions, such as "Father has long since withdrawn from any major role in socializing the young" (p. 90). The strength of this is not tested against any of the studies of middle-class nuclear families.

Some failure to come to grips with the body of complex work done by sociologists runs through much of the book, giving evidence that the authors have not done much homework for this assignment. One of a number of examples is in their discussion of middle-class norms and child-rearing. It bears no indication that they ever consulted the complexity of the existing studies or Bronfenbrenner's excellent organization of them.

What remains is a vague and excessively overdone piece of pleading for the position of the mass-society critic. The description of historical evolution is replete with the telescoping of centuries. Thus they tell us (p. 332) that "printing gave rise to the metropolitan press" although several hundred years elapsed between Gutenberg and the development of mass newspapers in the 1840's in the United States. They ignore theories and studies that do not quite fit their denunciation of modern life. They tell us about the

decline and loss of familial functions but do not balance it by discussing the view of increased affectional functions.

In discussing the mass media, the authors say that "a typical example of mass culture, indeed its purest distillation, is the slogan" (p. 351). With this they have characterized their own cliché-ridden work. It is difficult to believe that these two writers, whose astuteness and capabilities have been shown elsewhere, can have written this book. Such mood music is not helpful to students. It teaches the woodwinds to blow hard and the percussions to bang away in happy oblivion of audience or orchestra. Such lack of concern for patient rehearsal only produces musicians who don't know the score.

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Sociology through Literature: An Introductory Reader. Edited by LEWIS A. COSER. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 408. $4.95 (paper).

The idea that our understanding of social processes can be advanced through the study of great works of literature was common at the University of Chicago years ago. Today it has become a cliché to which most sensitive sociologists nod assent. Lewis Coser, on the other hand, has gone beyond lip service and has actually implemented this idea in his new reader, *Sociology through Literature*. From the literary traditions of thirteen countries, Coser has selected ninety-five excerpts from novels, short stories, essays, poetry, and other forms and organized them in such a way as to illustrate the fundamental concepts and topical areas of sociology. For his effort to make the study of introductory sociology more humanistic and aesthetically rewarding, Coser should be commended strongly. Whether this particular reader will be a successful instrument in this endeavor can be determined only after it has been used and evaluated.

As a moderately priced paperback, *Sociology through Literature* is intended as a supplementary source of reading in introductory and basic principle courses. Its section organization parallels such conventional textbook chapter headings as culture, social control, socialization, stratification, and bureaucracy. Each section is introduced with a brief theoretical definition of concepts and discussion of major concerns, followed by an indication in one sentence of how each literary excerpt is related to or throws light upon them. To give a typical example of Coser's method, his high critical standards which make the book such enjoyable reading, and the range of literature he has drawn upon, the concepts of status and role are illustrated by excerpts from Proust on the French upper-class social world, short stories of Sholom Aleichem and Anton Chekhov, Melville's description of rank order aboard ship in *White Jacket*, Orwell's analysis of occupational hierarchy in Parisian restaurants in *Down and Out in London and Paris*, and Shakespeare's famous account of the stages of the life-span from *As You Like It*.

In his general introduction, Coser observes that "the trained sensibilities of a novelist or a poet may provide a richer source of social insight than, say, the impressions of untrained informants on which so much sociological research currently rests." But the truth of this statement does not also imply that literature is the best source material for the development of skills of social analysis among untrained students. My uncertainty about the pedagogic value of Coser's book for introductory courses is based on a feeling that the extraction of the sociological kernel from the literary shell depends on the prior existence of a sociological perspective. This is a way of looking at the world that undergraduates rarely possess and is hopefully acquired in introductory and principles courses.

The specific contents of *Sociology through Literature* may make it difficult to develop this perspective, thus compounding the dilemma. I think the book attempts to cover too many topics and presents too many literary excerpts. Since they average only four pages each and most of them are pulled out of the context of larger works, the sociological relevance may often be obscure to the beginning student. Coser's own commentary on each selection is limited to one sentence and thus provides the student with little guidance. This restraint on the part of the editor is wise from a literary point of view, for Coser has for the most part refrained from mechanically reducing works of

art into the language of sociology. But the result is a gap which may leave students bewildered and confused unless the instructor is creative as a sociologist-teacher, understanding and respecting diverse forms of literature. Unfortunately, most of us lack the historical and literary erudition of Coser himself who no doubt has successfully used these materials in his own teaching.

Perhaps this book is best when the excerpts can be approached as case materials which illustrate the universal and culturally divergent aspects of specific social institutions. Such a section is the one on religion where selections from Melville, Forster, Joyce, and Sinclair Lewis capture the variations in ritual and belief in the religious systems of Polynesia, India, Ireland, and the American Middle West. But in many cases the freshness and appeal of the excerpts seem related to the fact that sociologists no longer write readable prose. When we contrast the editor's selections with those of more literate sociologists, we can ask whether De Quincey and Romains' descriptions of London and Paris add anything to the classic urban analyses of Simmel and Park, which, in addition to an almost equally lively and graphic style, also present the student with generalizations to chew over.

It is in the more advanced specialized courses having students with sociological perspectives that literature can most profitably be employed, and here in undiluted doses rather than truncated excerpts. I think courses and even seminars in American ethnic groups could be organized around the reading and analysis of the novels of James Farrell, Meyer Levin, Richard Wright, Pietro DiDonato and countless lesser writers who give the flavor of ethnic subcultures and value systems better than most sociological accounts. And similarly for the use of Balzac, Dickens, Orwell, and other classics in courses on stratification and occupations.

But if I question the value of Coser's volume for the student, I recommend it highly to the professional sociologist. It can be an excellent source of lecture materials. The excerpt from Silone's *The School for Dictators* is one of the clearest and most penetrating statements of the nature of totalitarian commitment; Ambrose Bierce's short story, "One Kind of Officer," is one of the best illustrations of the subversion of organizational goals through ritualistic adherence to procedure. Further, the book is fun to read, although in some places the change in mood required to shift from Dorothy Parker to Proust and from Proust to Sholom Alecheim cannot be managed and is a source of irritation. Coser reminds us how much we have missed in neglecting our favorite writers in order to "keep up" with professional publications, and in the process he invites us to sample further the work of many who have been previously unfamiliar to us.

ROBERT BLAUNER

University of California
Berkeley

The Ladder of Success in Imperial China: Aspects of Social Mobility, 1368–1911. By PING-TI HO. New York: Columbia University Press, 1962. Pp. xviii+385. $8.00.

Professor Ping-ti Ho, who has been connected at different times with Columbia University and the University of British Columbia while he was working on this book, and who is now at the University of Chicago, has given us a work of prodigious scholarship with several points of important interest for sociologists. First, there is a case here of how one historian has striven diligently, and on the whole successfully, to apply sociological analysis and methods to historical data. Shortcomings remain—of data, of analysis, and of method; but, on balance, sociological history as exemplified in this book should seem to the reader feasible and rewarding, though difficult. Not more difficult than any other kind of sociological research, but difficult nonetheless and for much the same reasons encountered in any attempt to collect and control masses of empirical data. Second, and of equal interest to sociologists, there is a case here of comparative analysis, comparative analysis between what have, until recently, seemed to be two entirely different types of society, Europe and China from the fourteenth century to the present. In the present state of our sociological theory and historical data, such comparisons can all too easily be dismissed as crude and even useless. Yet they make evident broad similarities of institutional structure and functioning which can serve for the better analysis of both of these societies. Such broad similarities, that is to suggest, can become the base

for further research which seeks to *refine* our understanding of these similarities, and lead us into better knowledge of differences as well. If we are neither enthusiastic without limit, nor contemptuous beyond bound, with enterprises in broad comparative analysis, we can take them as the necessary beginnings, not the conclusions, of an essential sociological method. Third, and finally, there is a case here in Ping-ti Ho's book of valuable substantive findings about amounts, processes, and determinants of upward and downward social mobility. On the basis of statistical and qualitative evidence, he finds "substantial" amounts of both upward and downward mobility at all levels in Chinese society from the fourteenth to the twentieth century. During the first part of this period, in the Ming dynasty, mobility was increasing, but it decreased later, from the seventieth century on, in the Ching dynasty. Both the processes of mobility and the social determinants of the changing quantities of mobility were similar in many respects to those of Europe at the same time. In Europe, however, from the seventeenth century on, these determinants were favorable to increasing mobility, whereas in China they were unfavorable, not only in comparison with contemporary Europe but even in comparison with its own immediate past.

In brief, this is a valuable and attractive book for that rare bird, "the general sociological reader," wherever he exists, as well as for the specialist. Sinologists and sociologists both can look forward to Ping-ti Ho's further studies in sociological history.

BERNARD BARBER

Barnard College
Columbia University

Explanation in Social Science. By ROBERT BROWN. Chicago: Aldine Publishing Co., 1963. Pp. 198. $5.00.

This is a book about the logical structure of explanations as used in the social sciences. A typology is first developed, and then the emerging seven types of explanation are discussed in detail. Dr. Brown, a Fellow of the Institute of Advanced Studies at the Australian National University, classifies explanations along two dimensions. As to form, he distinguishes *genetic* explanations (as to historic origin) and explanations through general laws; the latter are divided into empirical *generalisations* and *theories*. As to content, he distinguishes *intentions* (of the actor), *reasons* (motives) and *dispositions* (to act in a certain way). To this he adds *functional* explanations (where an event happens because it fulfils a stated end within a system).

The book is stimulating, modestly written and—like so many British books—pleasantly modest in length. Its shortcoming is its self-set frame of reference. Explanations in the social sciences should not be treated by themselves, because they are at best a subspecies of scientific explanations in general. The very borderlines of the so-called social sciences are difficult to draw. Are they characterized by the involvement of human actors? Where then, for instance, do we consider ethology, the comparative study of animal behavior? Where history? Where epidemiology? And so forth. Had the social-science explanations been treated in their more general context, the author, and we with him, would have perceived more clearly their true nature. Some of the major problems are germane to explanations in general: Events that involve time as a variable have a different form of explanation than, for instance, Boyle's law that relates pressure, temperature, and volume of a gas; then there are stochastic explanations and explanations that have no such limitation: there is the explanation of the unique event as against the explanation of a class of phenomena; the problem of the infinite regress of explanations (the falling stone is explained by gravity; gravity is explained by etc.).

All these questions concern explanations in general, but in this book they are not properly separated from those that are clearly specific to the social sciences, such as *motives* and *intentions.* We have a hint that author himself, in retrospect, must have regretted this somewhat parochial limitation of his study: his slightly embarrassed reference to Nagel's *The Structure of Science,* "which arrived too late to save [him] from mistakes." The fact, of course, is that, for instance, a reading of *Problems of Description and Explanation in the Empirical Sciences* (a section in Feigl and Sellars, *Readings in Philosophical Analysis,* published in 1949) would have helped the author to tie his thoughts more effectively into

the development of these questions and be less surprised by Nagel's book.

Totally absent from the book also are all references to the primarily American efforts to render social-science explanations in quantitative terms. This is a fatal omission, because these efforts are necessarily predicated on the type of logical analysis that Dr. Brown tries to advance. The nexus between *motives* and *dispositions*, and the relatively unimportant *intentions*, would have become much clearer had he not made this omission. "I wanted to kill him" is, of course, an explanation of a homicide; it excludes accidental killing. But from then on even the motive ("I was jealous") will provide only a most unsatisfactory explanation of why jealousy here resulted in killing.

There is another difficulty. The book is written in a quasi-Socratic manner; queries are put and answered in gingerly discourse. But unlike Socrates' questions, those used here are not always pertinent and thus often prove to be stumbling blocks rather than guides to understanding. Partly this is due to the author's predilection for queries that were actually made, sometimes by very undiscerning critics of the social sciences. This method of discourse deprives the analysis of the rigor needed for a logical inquiry such as this one aims to be. One squirms if a comparison yields "exactly similar" results (p. 149).

But there is such a dearth of writing on these methodological problems that one gladly welcomes this book even where one disagrees or finds it wanting.

HANS ZEISEL

University of Chicago Law School

The Origins of Scientific Sociology. By JOHN MADGE. New York: Free Press of Glencoe, 1962. Pp. viii+600. $6.00.

Written by the deputy director of one of England's foremost social research organizations, this volume had its inception in a graduate methods course offered during the author's visit at Brooklyn College in 1957–58. Apparently, Madge's purpose is to present and assess the historical development of empirical sociology as represented by major selected research contributions over the last half-century. Works were included because they displayed a concern for understanding and controlling social problems, introduced or refined investigative techniques, and provided empirically based concepts. Specific chapters, which range from about thirty to more than fifty pages, are devoted successively to Durkheim and especially his *Suicide*, Thomas and Znaniecki's *The Polish Peasant in Europe and America*, the University of Chicago research of the 1930's as exemplified by Zorbaugh's *The Gold Coast and the Slum*, the Lynds's *Middletown* and *Middletown in Transition*, Roethlisberger and Dickson's *Management and the Worker* as typical of the Elton Mayo industrial relations approach, Whyte's *Street Corner Society*, Myrdal's *An American Dilemma*, *The American Soldier* by Stouffer and his colleagues Kinsey's *Sexual Behavior in the Human Male* and *Sexual Behavior in the Human Female*, Adorno, Frenkel-Brunswik, Levinson, and Sanford's *The Authoritarian Personality*, Bales' *Interaction Process Analysis*, and Festinger and Kelley's *Changing Attitudes through Social Contacts* as an illustration of the group-dynamics approach. Madge insists that the overwhelmingly American character of the selections is simply a recognition of the actual facts in the development of empirical sociology. (Nevertheless, American sociologists are likely to be uneasy over the broad conception of sociology to which Madge apparently sub-subscribes, for four chapters deal with the research of persons who are not academically or otherwise directly associated with the field.)

Except for the introduction and conclusion, all of the chapters are organized similarly. They present the social, historical, and personal context in which the inquiry began, the nature of the data collected and the techniques employed, the concepts within which the empirical material was structured or which it helped create, and the major substantive findings of the investigation.

The assets and liabilities of this book seem obvious. Madge has performed a valuable service for the discipline in compiling data about the setting in which the various researches were initiated and in summarizing the major conclusions reached. But the chapters are of variable quality, among the less impressive being the analysis of the University of Chicago school. Readers of this *Journal* will be particularly interested to learn that the works of the Chicago school were unconcerned "with individual personality formation and

motivation" and lacked "any references to Freud or psychoanalytic theory" (p. 562). (It would appear that Madge did not consult the index of Park and Burgess' *Introduction to the Science of Sociology* under Freud or the article on "Sociology and Psychoanalysis" in Becker and Boskoff's *Modern Sociological Theory in Continuity and Change.*) Curiously, the interpretations of the chapters characteristically fail to use the declared conceptions of science and the scientific method in the instances in which they are available or the more fragmentary allusions to the nomothetic-idiographic, explanation-description, or causation-probability controversies. And disappointment is the reaction of this reviewer to the conclusions. Illustrative of the caliber of the generalizations about data-collecting techniques is the declaration that the main development in interviewing has been the systematization of the technique. At the other extreme, the typically limited scope of the bulk of the research studied undermines the author's efforts to draw inferences about more macro-sociological conceptualizations and theorization.

ROSCOE C. HINKLE

Ohio State University

Collected Papers, I: The Problem of Social Reality. By ALFRED SCHUTZ, Edited by MAURICE NATANSON. The Hague: Martinus Nijhoff, 1962. Pp. xlvii+361. Gld. 28.

This is a very important collection. Regrettably few sociologists know Schutz's work in part because his World War II exile to the United States served to scatter his writings widely. It is a great service to have them collated in this memorial series.

Schutz created a remarkable synthesis of German and American thought. Much of our literature juxtaposes Weber, Mead, Thomas, Simmel; here Schutz has absorbed and extended those conceptions that have proven to be fundamental for sociology (e.g., definition of the situation, social types, types of legitimate orders). This is a work in general theory which is not reductionist, nor pseudo-analytical, nor too abstract to permit of testing. Like any collection, it requires thought and effort to integrate his contributions before translating them into research designs, but the challenge is compelling.

For the common language of normative theory (values, norms, belief systems) Schutz substitutes another (actor constructs, presuppositions, "attitude"), not of neologisms, but to the end of ridding the normative approach of much that has been criticized about it. Among the consequences of these alterations are that the sociologist builds theory, rather than merely taking over those of the actors, and he does not find himself befuddled by the phenomenon of "false consciousness." Schutz's effort to avoid a mixed bag of theory merits careful attention, both for its potential and for the way it avoids the difficulties of current approaches. The reader will need to immerse himself in this singular theory to appreciate fully its advantages, however, for Schutz is disposed to creation rather than to criticism.

Schutz would make into definite propositions the presuppositions that sociologists may testably infer persons in social relationships to *use.* (By referring to actual "use," he avoids *verstehen* entanglements with fringe meanings such as of "knowledge" or "consciousness.") Differences in social relationships are described neatly as suspensions and alterations in these intersubjectively sanctioned presuppositions. Schutz suggests many imputations which we can infer that actors use or suspend to give distinctive characteristics to social actions of widely differing kinds. His theory appears to fulfil usefully the function of theory to order logically what is known and to generate critical questions.

Schutz's starting point is to identify the presuppositions of "common sense" behavior, thus beginning as did Weber with "traditional conduct." Often he explains these by contrast to science as a social activity, and greatly clarifies the troublesome distinctions between social actor and social scientist. The terminological novelty of the paper "Multiple Realties" is rewarding in showing the scope to which this theory can be extended. The ways in which differences in social relations may be accounted for by the differences in signs and symbols used socially to communicate them is the important topic of "Symbol, Reality, and Society." Schutz's treatments of Scheler and Sartre are particularly helpful to anyone interested in what features make and sustain a relationship as social, or what makes for sociology rather than for psychology.

It is surprising to find Schutz's significant

paper on "The Problem of Rationality in the Social World" omitted, for it would seem to be most appropriate in the introductory volume. We may hope that his major work in German will be translated for this series.

Schutz writes clearly about these difficult problems and is accessible at many levels. He suggested and had begun to refine several dimensions that appear to have considerable power to differentiate among a great variety of fundamental social relationships and that should generate some very important research.

JAMES WILKINS

Northwestern University

America's Forgotten Labor Organization. By ARTHUR B. SHOSTAK. Princeton, N.J.: Industrial Relations Section, Princeton University, 1962. Pp. x+141. $3.75.

While the larger American trade unions have received a good deal of attention from researchers in many fields, the small, single-firm independent unions have been sadly neglected. It is estimated that as many as 1,400 different unions, spread throughout the fifty states, may contain 400,000 workers. In such industries as the chemical, telephone, and petroleum, these unions may well be the dominant form of organization. Attempting to remedy the neglect of this area of research, Arthur Shostak has undertaken a somewhat limited study of the independents in New Jersey, a state containing many such unions.

Shostak divides his sample of forty unions into weak and strong blue- and white-collar unions. The weak blue-collar unions tend to be very small inept bargainers, formed primarily to escape the invasions of larger national unions. Most of them are relatively young and are not related to the company unions of the early thirties. Located in semi-rural or urban fringes, their leaders tend to be older men with long seniority, whose concern lest they lose their jobs pervades the entire union-management relationship. What little ideology they possess is oriented more against large unions, business, and government than toward the furtherance of the union goals.

In contrast, the strong blue-collar unions have had a variety of origins, and some date back many years. They have a wide range in size and are located in mainly urban industrial areas. Leaders have a great deal of personal dynamism. Using a threat of affiliation with national unions, labor-management negotiations tend to be more complex and bargaining is effectively practiced.

Shostak's sample also included some white-collar office-worker unions. These tend to be quite conservative and quite employer-centered. While these white-collar unions also may be viewed as weak or strong, the impression given is that they tend to be quite weak and unable to justify their existence. They are checked both by strong paternalistic employers and by a generally small, unenthusiastic, female membership. Obviously, such unions do not focus on any labor movement and merely offer themselves as alternatives to no organization at all.

The last group of unions Shostak discusses are the professional engineering unions which also exist in two types: the guilds and those resembling regular unions. The former would bar technicians, prefering an elitism that is more cosmopolitan and less shop-focused. Rather than bargain for union-shop clauses, the guild type prefers to solicit members. Leadership is weak because it tries to speak for the entire membership in a meticulously democratic organization. To a great extent leaders tend to be more socially aware than the other members or those who use the union as a medium of self-help and self-expression. Needless to say, such men are frequently lost to management. The few professional engineering societies that exist tend to oppose both the guild and regular-type unions on the grounds that their professional image will be compromised.

The single-firm union generally faces many problems of association. Many of these small unions do not even know that such larger organizations exist although some do belong to company-wide associations or state federations. Interestingly, large internationals do not seem to know about the existence of many of these smaller unions either. The reasons why unions do not want to join either associations or international unions seem to be about the same: they are afraid it will ruin the present union-management relationships or they are unwilling to raise the per capita taxes that would be required. Obviously, the delegation of authority would also be resented.

Then too, the ideology of many of the smaller single-firm unions is fundamentally against large organizations of any sort, and they are firmly committed to the autonomy of the small local based upon co-operative interpersonal relationships. Frequently, the smaller affiliates of the unions will call upon the larger association only when their bargaining has reached an impasse and the association can offer little assistance. Not only are they faced with employer opposition, but their strike funds are limited, and they know that even those leaders committed to the formation of the larger association frequently cannot control and deliver the support of the component members.

This study makes an important attempt to explore and explain some of the behavior of the single-firm union. Unfortunately, the book is at best a tantalizer and, I believe, somewhat premature in its presentation to a wide sociological audience. There is no systematic framework employed to explain the findings; as a result, the usefulness of the book is limited to students of the labor movement in a very narrow sense. Insights that the single-firm union could provide into the labor movement or organizations as a whole are glaringly avoided. Theoretical issues are never raised and sociological implications are studiously eschewed. Even granting the pilot nature of the study does not excuse the author from the responsibility of providing his readers with a more systematic presentation that has implications for further research. Shostak's book may whet the appetite of a small group of sociologists, but they would then have to reconstruct this material properly.

PHILIP M. MARCUS

Survey Research Center
University of Michigan

The American Polity: A Social and Cultural Intrepretation. By WILLIAM C. MITCHELL. New York: Free Press of Glencoe, 1962. Pp. xiv+434. $6.00.

This book begins with a short, vague review of Talcott Parsons' more recent concepts of the "polity." The student (and this appears to be an introductory undergraduate text) who has not had the advantage of substantial previous exposure to Parsons a structural-functional analysis will almost c tainly be at a loss to understand why he being asked to read this material. When t exposition of the conceptual framework sto and the essay on American politics begi the failure to take a specific phenomen from political life and explain it leads to combination of hearsay statements, perso judgments, and a few statistics collect under chapter headings and subheadings tl recall the earlier conceptual scheme but little to make it appear useful.

A great deal of the book deals with sy bols, values, and norms. Despite the v hoard of survey research findings the autl finds it convenient to discuss the normati order without benefit of fact. This is p ticularly annoying when the few facts tl are cited are not integrated with the c ceptual frame but left to stand as isolat "propositions." Typical of the four propo tions the author has discovered concerni the distribution of the tax burden is th "The second largest contributor to the r tional tax receipts are families making $10,0 or more per annum." I am not opposed inventories of social facts, but I would w come a discussion of why the facts are they are. Why, for example, do families wi incomes under $2,000 per year (1958) p a larger proportion of their income to ta than any other income group, save the per cent of families above the $15,000 lev To say that taxes are distributed among t population is a long way from a structur functional analysis of the data. Other "pro sitions" are simply the author's opinio "The American political system has be extraordinarily wasteful in the use of resources and implementation of goals" (303).

The book's promise to be a "systema application of the insights of the behavio sciences to the institutions, values and b havior patterns of thte American politi community" seems excessive. Very few the reputed functional relations are support by solid analysis. Omission of comparati data on non-American political systems mal the multitude of comparative judgments "American versus *X*" meaningless.

The usefulness of the scheme can be demo strated only if existing knowledge can usefully organized by the conceptual ap

ratus. The author's failure to fit the wealth of available facts to the conceptual frame may indicate that the theory is hostile to the facts rather than an alternative claim that the facts assembled by behavioral scientists are inadequate to the task.

PHILLIPS CUTRIGHT

Social Security Administration

Professional Staffs of Congress. By KENNETH KOFMEHL. ("Purdue University Studies, Humanities Series.") West Lafayette, Ind.: Purdue University, 1962. Pp. xi+282. $6.00.

This book is a competent descriptive study of three professional staffs: those of the standing committees of the Congress, of Senatorial offices, and of the Office of the Legislative Counsel (the bill-drafting agency of the Congress). Based on about 180 interviews with staff members, the study covers primarily the period 1947–53, after the Legislative Reorganization Act of 1946 became effective. A brief postscript discusses developments up to 1961. The bulk of the book is devoted to a description of the standing committees and their staffs, with relatively brief chapters devoted to the Senatorial offices and the Legislative Counsel.

The author's major concerns are to describe and evaluate, not to explain and theorize. The recruitment and qualifications of the various kinds of staffs, the organization of the offices, the associations of one staff with another, and the details of work are described in great detail. Kofmehl evaluates the staffs by how successfully they fulfil the "philosophy" of the 1946 Act: non-partisanship, small size, high-quality recruitment, the maintenance of boundaries between executive and legislative and between professional and policy functions. His conclusion is that, in general, they maintain high standards.

The phrase in the foregoing sentence—"in general"—illustrates the main shortcoming of this book. Its usefulness, as a consequence, is mainly as a sourcebook of information concerning a little-known aspect of government. To put the matter technically, the book consists of a series of distributions of the marginals for many dependent variables, with very few even speculative discussions of what the cross-tabulations with other variables might indicate. There is little discussion of the factors associated with one form of staff organization rather than another, the extent of non-partisanship of the staff, the degree of "overstaffing," etc. Seldom are such factors as the size of the standing committee, the type of work that it does, or the character of its leadership mentioned as possible explanations for variations in organization and work. Many paragraphs are composed of a series of sentences beginning: "the most prevalent practice," then "some committees," "often," "occasionally," "in a few instances," "once in a great while," with no indication of the factors associated with the given variation.

Granted, the author did not have the kind of data that would have allowed him to present statistical tables. The book is a good example, however, of the kind of qualitative analysis that would be considerably more illuminating if the *logic* of multivariate analysis and elaboration were used. If, for example, the author had interpreted his data as referring to characteristics of individuals (education attainments of the professional staffs, for example), characteristics of groups (the size, function, and party composition of a committee, for example), and events (a change of committee chairmen, a crisis demanding the sustained co-ordination of the entire staff), he could have formulated many hypotheses for which his data would have been relevant, if not conclusive.

Many provocative questions can be asked which the author does not raise. For example: To what extent do staff members actually influence the content of bills they work on? Frequently staff members (not necessarily the same ones) determine the agenda for hearings on bills, choose the speakers, frame questions for committee members, and then write the bills. But the author assumes that, for example, "in attempting to work out alterations" in a measure to suit various interest groups, "a committee staff member always operated within the guide lines of policy established by his committee" (pp. 117–18). How does he know that? There is no evidence to support such an assertion or the similar one concerning the Legislative Counsel staff: "With a profound insight into the legislative process, they realistically appraised the various considerations they should take into account in their work" (p. 187). Such statements indicate that Kofmehl took his in-

formants a little too seriously. The book's emphasis on evaluating the staffs by rational bureaucratic standards hinders the raising of questions concerning their actual behavior and political role.

ROBERT R. ALFORD

University of Wisconsin

The Rise of Democracy in Pre-Revolutionary Russia: Political and Social Institutions under the Last Three Czars. By JACOB WALKIN. New York: Frederick A. Praeger, Inc., 1962. Pp. viii+320. $6.50.

Works on pre-Revolutionary democratic trends in Russia, which establish that by the end of 1916 Russia genuinely faced an open political situation, are no longer novel. Such works frequently encounter difficulty, however, in explaining why the revolution occurred and how the Bolshevik regime so soon regressed to totalitarianism.

Jacob Walkin, equipped with commendable documentation, undertakes to solve these troublesome problems. He succeeds in major part where other scholars like Nicholas Timasheff fare less well. As a result of an institutional approach, Walkin demonstrates that a fissure separated an emerging Russian society eager for governmental responsibilities from an inflexible tsarist autocracy.

The rift became symbolized in terms of "we" versus "they." "We" included even the conservative gentry working with local government units called zemstvos, who disdained the regime as "they." "State and society became two warring and irreconcilable camps between which there could be no collaboration."

Not satisfied with this plausible account of revolution, Walkin unfortunately goes beyond his data. He asserts "without qualification whatsoever" that pre-Revolutionary Russian democratic processes, if not interrupted by revolution, would have transformed Russia more completely than the subsequent four and a half decades of communism.

More questionable, however, is the author's explanation of bolshevism's triumph, a viewpoint he attributes originally to V. A. Maklakov, Duma representative from the Kadet party. Two main antithetical political elements were alleged to be at work in Russia: the traditional state authority and the forces of unrest. The inept monarchy undermined its own traditional power, leaving only the liberals of the Provisional Government who in turn could not cope with anarchy. Thus, according to Walkin, the way was opened to mob rule and Bolshevik victory.

Victor Chernov, Russian sociologist and chairman of the Socialist Revolutionary party, also noted that Lenin came to power on the crest of mob violence. But Lenin, as Chernov points out, did not remain a slave to the mob; he organized and eventually quelled it. How was it then that Lenin mastered the mob where others, like Kerensky, tried and failed? The answer to this question, once it is forthcoming, will more readily explain Bolshevik power than a simple invocation of the mob.

Walkin's study, despite these shortcomings, stands high above other efforts in the same area, precisely because it is a contribution to historical sociology. Of special interest are the chapter on the zemstvos and the section on the role of the intelligentsia. Both are particularly relevant to contemporary democratic problems in underdeveloped areas.

ALEX GARBER

University of Colorado

The Theory of Political Coalitions. By WILLIAM H. RIKER. New Haven, Conn.: Yale University Press, 1962. Pp. xii+300. $6.00.

It is refreshing to find a political scientist using the word "theory" to mean a set of related and verifiable propositions about political behavior. This is a bold book. The author claims that his theory is relevant to the understanding of both political party coalitions and international alliances, a subject of many historical and descriptive studies. Nor does he shrink from using the definite article in titling his offering.

The theory builds and elaborates on two general principles that have appeared in other recent works on coalition formation. The first of these is that, under certain specified conditions, coalitions tend toward the *minimal* winning size. This is called here the "size principle" and has been elsewhere called the "cheapest winning coalition" hypothesis. The second principle focuses on process, viewing

the formation of coalitions as a step-by-step affair in which strategies are (not necessarily consciously) oriented toward the minimal winning coalition strategy. From these blocks, a considerable building emerges.

First of all, the author, using the theory of *n*-person games gives the size principle and the strategy of coalition formation a rigorous logical foundation (stated formally in appendixes). The evidential basis, however, is not as solid, although he deserves maximum praise for the difficult and perilous exercise of using historical instances as a basis for evaluating the theory. In particular, he examines three instances of overwhelming majorities in American party politics and three instances of coalition behavior following total wars. These represent all the relevant instances of the classes of events defined. In each case, total coalitions tended to dissolve into minimal winning ones. Unfortunately, the author neither deals conscientiously with alternative explanations (e.g., cyclical fluctuations around some larger-than-minimal size) nor engages in any search for negative instances. We do not know, for example, if there have been instances in American politics in which already-winning coalitions have made vigorous attempts to increase membership or have remained stable for prolonged periods.

There is a lucid analysis contrasting balance of power theory in international politics with the coalition theory offered. It is marred somewhat by confusion about the extent to which the coalition theory is an equilibrium model. The author does not seem to recognize fully that the size principle specifies an equilibrium at the minimal winning coalition point, but asserts instead that the model "lacks any kind of equilibrium." This peculiarity has the generally salutary effect of leading him to a thorough analysis of sources of instability both from within and from without the system.

The book is weakened by a final chapter on the Cold War that is only very loosely connected with the main structure of the book. It would be a pity if the casual reader were to judge the general significance of this valuable and generally closely reasoned theory of coalition formation by this final obiter dictum.

WILLIAM A. GAMSON

University of Michigan

The Community and Its Hospitals: A Comparative Analysis. By IVAN BELKNAP and JOHN G. STEINLE. Syracuse, N.Y.: Syracuse University Press, 1963. Pp. xix+234. $5.75

The stated purpose of this study was "to find out if possible why some American communities have good general hospitals and some do not." Judging from the bilious literature emanating from standard-setting committees, this is an invitation to pious remarks and vacuous recommendations. Belknap and Steinle are not afflicted with this refined form of academic gout. They have turned out an empirical study which, though it does not answer their initial question, deals with it in a way that will interest students of organizations, community power, research techniques, and. of course, medical sociology.

The authors provide a good account of their research strategy which started with a national survey of hospitals and communities and ended up, oddly enough, with an intensive study of two Texas communities of 150,000 and 120,000 persons. One may quarrel with the wisdom of selecting communities which are unrepresentative of the nation's population merely because they are uncontaminated by other cities and of manageable size, but this method has a venerable history in community studies. The two cities are matched in terms of economic and professional resources, thus controlling a possible major source of variation. but differ greatly in the quality of the health services to the community. The community hospital of the "good" city had fairly high standards of care, an active teaching program. and a good physical plant; it provided considerable care to indigent persons in the community. The two hospitals in the community with poor services had low standards of care, no teaching, poor physical resources, and provided almost no care to the indigent population. Why were there these differences?

The community with poor medical services was controlled by elected county officials who were more interested in roads for farmers than in supporting medical care through increased taxes. The city officials could have prevailed over this obstacle, but the competition between the two religious hospitals of different faiths prevented unified action. Furthermore, neither denomination included many economic or political elites, so the trustees of the two hospitals lacked community power and adopted

conservative attitudes toward health problems. Though some physicians favored adequate care for the indigents, in part to provide the basis for an accredited teaching program and higher standards of medical care in the hospitals, the trustees opposed it. Both hospitals were "non-profit" but neither hospital was poor. One of them regularly made a ten per cent "profit," which it placed in a swelling contingency fund. Nevertheless, it policed the doctors by keeping a running account of the number of bad financial risks a doctor admitted. A large number of risks was grounds for removing hospital privileges.

In the other community an autocratic and cohesive group of economic dominants worked hand in hand with the heads of the county medical society to provide a community hospital with high standards and adequate care of indigents. The community hospital was supported by city and county taxes and by strong pressure for donations from wealthy individuals. Doctors who opposed free care were brought into line by the controlling medical cliques. An informal system of screening insured that elected officials in the city and county would be tractable and support the hospital with tax money. A competing church-sponsored hospital managed to drain off many of the more wealthy patients and provided no free care, but it had lower standards of care. The contrast of the two communities, one run by elected officials, the other by a "hidden power structure," provides a beguiling alternative to the usual civics lesson. The interviews with the trustees are themselves worth the price of this book.

There are a number of all too brief discussions which could be expanded into useful articles. The results of a national survey challenge the outmoded taxonomy of hospitals as it exists in the literature, making it clear that many so-called non-profit hospitals are really very close to proprietary hospitals. A realistic and successful attempt to measure hospital goals is another part of the authors' inclusive research design and deserves attention. Without fanfare they assess the community's power structure by utilizing the best of the two major approaches, though much too little detail is presented. The discussion of the difficulty of using a panel of consultants in research is worth pondering even though only the bare bones are reported by the authors. There is excessive detail on physical plant, architecture, and the inevitable history of hospitals up t the twentieth century, but this is a mere quit ble. Altogether, this is a thoroughly profe sional and rewarding volume for students i many fields.

CHARLES PERRO

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Farming in Cultural Change. By B. BENVE NUTI. New York: Humanities Press, 196 Pp. xx+467. $7.50.

Quite in contrast to most American studi which attempt to explain the adoption of far practices and farm-operator response to con munication media by empirical testing of r lations between many independent variabl and adoption behavior, Benvenuti approache the problem from a configurational perspe tive in which the cultural setting is taken the primary focus of attention. The judgme of "knowledgeables," empirical testing of r lationships, and case studies are used to e tablish the validity of the postulated traditio alism-modernism cultural complexes of whic the adoption of farm practices and the requ site mental capacity and thought patterns a a part.

The central thesis of the book is that the is a typical cultural pattern of the progressi farmer characterized by orientation to t outside world and by particular personalit traits; that this cultural pattern develops a result of cultural contacts with the outsic world in general and the western town in pa ticular, the latter functioning as a vehicle modernization; furthermore, that this cultur pattern influences total behavior of the grou concerned including performance as a far manager.

Winterswijk, a province in the Netherlanc popularly referred to there as "the back co ner," was selected for the study. An almo total lack of modern means of communicatio between this region and the rest of the Nethe lands had kept the area relatively isolate from outside contacts until the beginning this century. With subsequent exposure to ou side influences through new channels of con munication and textile industries, a veneer modernism was added to a relatively trad tionalistic society. Since all persons did n participate equally in this modernization, pe

ality types which exemplify the cultural lance between traditionalism and modern- could be found.

. random sample of 484 farmers, individu- interviewed, and selected secondary rces provided the information needed for analysis. Responses to ten questions re- ling public issues were used as the opera- al measure of "social awareness" and thus degree of the individual's social function- in the present form of modern western nization. Validity of this measure, and of distinctive cultural types, was tested asking twenty-five judges to describe high- low-ranking farmers on the scale score and the use of six case studies which also pro- d insights regarding the interaction of ure and personality. Labor efficiency was l as the prime measure of farm-manage- t proficiency. A global qualitative ap- ch to the question of whether there exists attern of the progressive farmer was fol- d by a statistical cross-tabulation of scale e positions with the cultural aspects of operator's life, the home-family environ- t, and farm-management factors.

provocative implication of the configu- nal nature of practice adoption and the in which adoption is conditioned by atti- s of favorability to change is that adop- s may be better implemented by programs ring about rationalistic, broadly oriented s of thinking characteristic of modernism . by encouraging the adoption of specific practices. Furthermore, successful at- pts to get specific adoptions may result he use of practices without the requisite city to capitalize on their potential bene-

he author is to be commended for meth- ogical adequacy, conceptual formulation the thoroughness with which he estab- d the validity of postulated cultural dif- nces existing in Winterswijk and the way hich these cultural patterns were reflected e thought processes, capacities, communi- e behavior, and farm-management per- ance of the farm operators studied. Al- gh still in the nature of hypotheses to ested, he seems to have suggested a con- ual scheme for explaining farm-practice tion behavior on a cross-cultural basis for explaining the divergent roles that al functionaries sometimes play in either implementing or retarding the acceptance of new practices.

Certainly the author has made an important contribution to an understanding of the inter- action of cultural and personal factors in the adoption of farm practices and the process by which adoptions occur. Those interested in this particular subject or the broader subject of implemented social change will surely want to read this book. One unfortunate note is the presence of an undue number of editing deficiencies.

HERBERT F. LIONBERGER

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Communications and Political Development. Edited by LUCIAN W. PYE. Princeton, N.J.: Princeton University Press, 1963. Pp. xiv+ 381. $6.50.

"This volume," Gabriel Almond indicates in the Foreword, "is the first in a series of Studies in Political Development sponsored by the Committee on Comparative Politics of the Social Science Research Council under a grant from the Ford Foundation for the period 1960–63." The ten contributors, coming from political science, sociology, and psychology and, with one or two exceptions, bristling with sophisticated immodesty concerning their own research and ideas, not unexpectedly use dif- ferent approaches to the problem of relating "the communications dimensions of social ac- tion" to developing countries. They all offer, however, generally provocative and potentially harmonizable generalizations and differ only with respect to the scope and kind of support- ing evidence. Aside from those pontificating ex cathedra (Shils, Lerner), some make casual references to many countries (Hyman); oth- ers devotedly concentrate on a single land (Yu, Frey). Some offer rubbed-down citations (Pool) or a handful of scattered figures (Schramm); others provide information in the best correlational (McClelland) or historical (Passin) tradition; and one (Mosel) supplies new, impressive, imaginative materials. Unlike most collections of papers which originally functioned as tickets of admission to the rich food and drink served in a luxurious hotel or estate during a long weekend and paid for out of foundation funds, this book is not scat- tered and not especially uneven, because it has

been magnificently and creatively edited by Lucian Pye. His general introduction and deservedly lengthy introductions to each chapter or section merit thoughtful attention in their own right; they alone justify the volume.

In the last chapter Daniel Lerner comments, before unveiling his own stimulating hunches, that "no paper [in the symposium] achieves such a comprehensive theoretical understanding—a statement of such force as to suffuse us with the beautiful feeling of perfect illumination— as does a Newtonian account of the solar system as a *gravitational* system." As often happens, Lerner is right; we are not suffused; but, as he also pleads, tears must not flow. Actually, with a squirm or two, it is possible to discover reasons for feeling restrained joy after digesting this book. In the first place, concentrating upon communication in the context of developing countries has been clearly fruitful not only per se but beyond the immediate context: new data are collected, old theories in social science are tested cross-culturally. Then the topic under discussion has been discovered and mined almost exclusively by political scientists and thus reflects a stimulating renaissance within some echelons of that discipline. For opposite reasons, both the softer and the harder social sciences—which here must be left coyly undefined—may be inspired by Pye, his present associates, and other political scientists: the former because they can observe that systematically gathered facts may be as humanistically illuminating as hunches with or without footnotes; the latter because they can note that a touch of daring recklessness going beyond, sometimes way beyond, the facts can be tremendously useful. Finally, those who did not enjoy the liquid hospitality at the Dobbs Ferry conference, the site of the volume's fertilization, may find it sobering to watch the interplay between the particular historical events within each developing country, or the concrete data issuing therefrom, and the nascent principles of these scholars. With hope, gratitude, and a friendly grin we applaud and learn from the *Sturm und Drang* which one branch of political science, aided a little by its divorced spouse and greatly by its friendly counsins, is now exhibiting.

LEONARD W. DOOB

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Societies in the Making: A Study of Development and Redevelopment within a County Borough. By HILDA JENNINGS. New York: Humanities Press, 1962. Pp. xii+275. $6.50.

Barton Hill, a working-class district of Bristol, England, was an area marked by a high density of substandard small houses and economically marginal shops and pubs in 1952. Its inhabitants, however, participated in a strong network of familial and personal attachments which frequently included neighbors in quasi-familial roles.

When plans for redevelopment were announced, Hilda Jennings, who had been active in the local settlement house for twenty years, undertook a study of the impact of renewal on the Bartonians. From her knowledge of the neighborhood and from interviews with 411 families over a five-year period, Miss Jennings presents a description of the relocation process and the reactions of the Bartonians to their new situations.

Many of the respondents reluctantly conceded the need for physical reconstruction. While Miss Jennings notes that the local officials made considerable effort to ease the problems of relocation and separation, her general impression is that individual needs were often overlooked in the planning process. There are occasional references in the volume to the proper responsibilities of government in housing, but the study is mainly concerned with what happened to former residents of Barton Hill.

Despite a rather self-conscious even-handedness, Miss Jennings tends to weigh the five years of rather difficult adjustment against the hundred years of evolved neighborliness with the former the apparent loser. The contrast is made striking by a description of one outlying housing development to which many Bartonians were moved. "Mossdene" was inconveniently located in regard to employment; it had a temporary quality brought about by inadequate facilities, including poor shops; many residents were relocated there reluctantly when no space was available in the inner city; the project mixed "problem" families injudiciously with "respectable" working-class elements.

Sketchy evidence is presented to bolster the argument that Bartonians were changed by the move, but much of the discontent at Mossdene seems to have arisen from the het-

erogeneity of backgrounds and the simple physical shortcomings of the location rather than from any intrinsic disruption of previous neighborhood ties. The five-year span of the study proved inadequate for determining the direction in which future relationships were headed. Indeed, near the end of the study new elements working for stability began to appear at Mossdene.

Finally, Miss Jennings was able to survey residents of one section of flats in "redeveloped" Barton Hills which had been occupied for six months. Compared with Mossdene, social problems were minor. A substantial majority of former Bartonians indorsed the new flats.

While Miss Jennings' rather limited data point in the direction of dissatisfaction due to a decline in long-term social ties, it is difficult to distinguish this line of argument from the short-term discontent due to other factors. Clearly, greater governmental foresight in concrete matters would have prevented some of the problems which arose, particularly at Mossdene. More direct transportation to employment areas, a greater number of shopping facilities, dispersion of light industries to provide female employment, and an effective means of child supervision might have facilitated an atmosphere conducive to positive social interaction. On its own terms, then, Miss Jennings' study is an interesting social history of the adjustments in personal relations required by redevelopment. It is not, however, the systematic exploration of relationships between socioeconomic conditions and community life which the title seems to imply.

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The Politics of Scarcity: Public Pressure and Political Response in India. By MYRON WEINER. Chicago: University of Chicago Press, 1962. Pp. xix+240. $5.00.

In this book Professor Weiner analyzes "the impact of political demands from society upon government and the problems that such demands raise in a society and polity of scarcity." These demands take a variety of forms—demands for more colleges and universities, more industries, higher wages, better irrigation facilities, more equitable land distribution, and for demarcation of state boundaries along linguistic lines. Weiner suggests that at present a gap exists between organized demands and government resources and that during the next few years, as economic growth, education, and mass communication stimulate more voluntary political organizations and higher organized aspirations, this gap will probably widen (p. 238). Unless these voluntary political organizations feel that their voices are being given a sympathetic hearing in government circles, there is a danger that they will "move politics from the conference room to the streets," that demonstrations and riots will replace coalition formation and the use of the ballot box, and that "neither democratic institutions nor effective government policy will survive" (p. xviii).

This danger from one side is matched by a danger from the other. If the government gives too sympathetic a hearing to the voices of voluntary political organizations, systematic governmental planning may be rendered ineffective, and over-all national development may be retarded. Weiner poses the dilemma as follows: "How can the state satisfy demands without making economic planning an impossible farce and without denying the demands of organized groups to so great an extent that political democracy is destroyed?" (p. 220).

Chapter i raises the question that remains the theme of the book, that is, do organized interests in India make demands on the government "that are so violent and so beyond the government's capabilities that they have the effect of reducing both public order and economic development?" (p. 10). Chapter ii suggests that a number of features of today's pressures on government have their origins in the struggle for independence, for example the high level of aspirations, the expectation that the government rather than private agencies will satisfy these aspirations, the control of interest groups by political parties, and so forth. Chapters iii–vii deal with specific interest groups: community (religious, caste, language, or tribal) associations, trade unions, organized business, agrarian movements, and students. In chapter viii, Weiner deals with the techniques used by political groups to influence the state. Although he does not overlook such

means as consultative bodies, petitions, deputations, *hartals*, and local Congress party organizations, Weiner focuses on violence as a means and explores the degree to which such violence is organized. He notes with concern how frequently the government has capitulated to pressure when that pressure has become violent.

In the final chapter Weiner proposes two broad methods whereby India's government might handle the dilemma of fostering lawlessness on one hand and abandoning systematic economic planning on the other. The first method would reduce demands on the state, that is, by reducing government's part in certain political and economic controversies (e.g., between labor and management), by improving communication between government and organized groups, by giving additional powers to local institutions (e.g., *panchayats*), by transferring some decisions from the legislative to the executive branch of government, and by developing a "public philosophy" within organized groups. The second method would deal with whatever demands still remained by exhibiting greater responsiveness to legitimately channeled demands, by more frequent use of symbolic status-bearing gestures, by allowing a degree of flexibility in the lowest echelons of the administration, and by refusing to capitulate to violence and civil disobedience.

For one who has been looking in scattered books and articles for histories of various voluntary associations in India, Weiner's book fills a long-felt need. Chapters iii–vii present in readable and concise form a wealth of detail on communal groups, trade unions, organized business, agrarian movements, and student agitations. Furthermore, he has singled out for analysis problems that have considerable relevance for sociologists—the strain within a society between the process of decision-making and that of integration, the dilemma of allocating scarce rewards within a society with multiple competing groups each feeling relatively deprived, the problem of engendering confidence and competence in bureaucracies such as government agencies or universities when they are imbedded in a society laced with hereditary and kinship loyalties, and the difficulties of achieving compromise or accommodation in a highly status-conscious society.

These materials are complicated, and it is not surprising that at times Weiner appears to contradict himself. For example, he suggests (p. 223) that in order to increase national stability, the government should give additional powers to such local institutions as *panchayats*, when it is in these institutions that the number of community associations is smallest and, according to him, the threat "to orderly state government and to merit standards in recruitment is most pronounced" (p. 72). One of the reasons for Weiner's apparently contradictory suggestion is the "strong moral argument that self-government is preferable to efficient government" (p. 24). Maybe this argument is valid for American political scientists, but during my years in Indian villages I frequently heard strong disagreement from Chamars, Muslims, and other minority groups who saw no chance for equality of opportunity as long as governmental powers were relegated to their traditional—and local—oppressors. There could be little question that these groups felt alienated from the political process. On the whole, however, such inconsistencies in Weiner's presentation are few, and such insertions of possible American bias are exceptional.

One area that Weiner might have explored more fully in order to balance his somewhat anxious conclusions is the area of non-political actions by "community associations." At the time of independence many Indians turned to government for meeting their aspirations. But as the years have passed and the government has not supplied their wants, they have sometimes decided to achieve their goals outside the governmental framework. In this regard, the importance of caste and communal associations in establishing schools, building hostels, floating loans for new businesses or agricultural improvements, sending members abroad for higher learning, and getting them good jobs when they return—outside of the strictly political framework—may actually be reducing the pressure on government to provide them with benefits. The emergence of these associations in India may reduce the likelihood of one of Weiner's predictions, that economic growth will increase the strain placed on government (p. 238).

Weiner's choice of Bengal for his primary research site may have somewhat colored his conclusions regarding the tendency of Indians

to take to the streets to achieve political ends. Aside from Delhi in 1947 and Bombay in 1956, the other major cities in India have not been characterized by the *hartals* and violent demonstrations that have periodically shaken Calcutta. Weiner recognizes this in his discussion of the "beliefs of Calcutta's Bengali middle classes" (p. 214). Furthermore, although it is true that at times the central and state governments have not responded to public demand until it became violent (e.g., state's reorganization), one cannot overlook the fact that the Indian government has made many significant policy decisions in anticipation of demands, such as its decision to establish a socialistic pattern of society, to inaugurate the five-year plans, to launch the rural and urban community development projects, to abolish *zamindars*, to consolidate landholdings, to provide extra facilities for the scheduled castes and tribes, and so on. In connection with these decisions, there has been a considerable amount of "wheeling and dealing—coalition-making and compromising," that is, a considerable amount of the democratic process.

Weiner has provided us with a valuable study of pressure groups in a developing nation. His is a pioneering work in the field. We can hope that similar studies of equal caliber will be carried out by other scholars in other developing nations.

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Differential Fertility in Central India. By EDWIN D. DRIVER. Princeton, N.J.: Princeton University Press, 1963. Pp. xx+152. $4.50.

This small book is an excellent contribution to the series of studies conducted in various parts of India on measuring fertility during the fifties; in it the author has compressed a great amount of information. The book is based on a survey conducted during January–May, 1958, in the Nagpur district in central India, covering about 1 per cent of the households in the rural and urban sectors of the district.

In the opening chapter the author sets the general purpose of the study—to determine whether it is reasonable to assume that changes in social structure will have any bearing on the general fertility rate. For this, he has tried to demonstrate that fertility varies among groups which are currently differentiated in social status.

After analyzing the data for differentials in the joint family system and age at marriage by socioeconomic status, Driver considers the fertility differentials by family structure, age at marriage, and socioeconomic status in chapter v. He concludes that high fertility is associated with both joint family living and early age at marriage. However, the association between high fertility and the joint family virtually disappears when the age of wives is standardized in the two groups. Several socioeconomic strata differ significantly in their fertility patterns, whereas others do not. The patterns are quite similar for couples who are distinguished on the basis of either place of residence, occupational class, or income level.

Although the title indicates that the study covers the whole of central India, one easily finds that the survey was conducted only in the Nagpur district. It is doubtful how far the results of the present study can be generalized to such a large area as central India when the author has recognized the differences in fertility patterns and certain dimensions of social status from one district to another from the studies conducted by the Gokhale Institute, Poona, in four districts of western India (p. 134). The sampling technique used is not a scientific one, and the urban areas have higher weight than their counterparts. For a major part of the analysis, the point for dichotomizing couples into old and young is taken as thirty-five years of age for wives; but no explanation is given for this choice.

Chapter vii deals with the interest in birth control. Although interviews were conducted separately for both members of the couple. no analysis has been presented about the differences in their attitudes toward family planning.

In spite of these minor shortcomings, the book has served its purpose and suggests many lines of approach which should be followed in future studies on differential fertility.

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NOTES ON THE CONCEPT OF A POPULATION[1]

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ABSTRACT

This paper contains a description of the basic population model and a discussion of applications of the model to some problems of common concern to demographers and sociologists. The concept of a population is advocated as a frame of reference in investigations of population composition and process, in the resolution of differences between macroanalysis and microanalysis, and in the design of studies of social change.

There are many peculiar aspects to demography as an academic calling. In some senses it is a field of sociology; in other senses it is neither a field nor even sociology at all. On the basis of its elegant models and quantitative rigor it has a claim to be considered the most advanced area in social science, and yet it might be rejected from that realm altogether as consisting essentially of a form of macrobiometry. It has endured for three centuries as "Political Arithmetic." As sometimes described, its scope seems to encompass the whole world of social statistics, but many social statisticians have neither training nor interest in demography. In the United States, where it has had its most extensive development, it has status nowhere as an independent academic discipline. Most often the demographer is found inside the Department of Sociology, but it is not always clear that he is either a welcome or or a comfortable guest. When demographers gather, they manifest a host of varied and sometimes conflicting interests: natural and social, pure and applied, scientific and propagandistic.

This essay is an attempt to identify some distinctive characteristics of the demographic approach to social analysis, with emphasis on the contributions that can be made by the concept of a population. The effort has been prompted by several recent publications with similar intent but somewhat different conclusions.[2] In the first section of this essay the basic population model is introduced and described. This model is then used to provide a basis for distinguishing the demographer's special contribution to the study of population

[1] Revised version of a paper entitled "The Demographer's Ken," which was delivered at the annual meetings of the Population Association of America, Madison, Wisconsin, May, 1962. The writer wishes to acknowledge the financial support of the Social Systems Research Institute, University of Wisconsin, and the intellectual support of O. Dudley Duncan and George J. Stolnitz in the preparation of this paper.

[2] See the following papers in Philip M. Hauser and O. Dudley Duncan (eds.), *The Study of Population* (Chicago: University of Chicago Press, 1959): Philip M. Hauser and O. Dudley Duncan, "Demography as a Science," Part I, pp. 29–120; John V. Grauman, "Population Estimates and Projections," pp. 544–75; and Amos H. Hawley, "Population Composition," pp. 361–82. See also Leo F. Schnore, "Social Mobility in Demographic Perspective," *American Sociological Review*, XXVI (June, 1961), 407–23.

composition and population processes. The penultimate section introduces the concept of a population into the controversy concerning the interrelationships of microanalysis and macroanalysis. Finally, some suggestions are made concerning the contributions a demographic approach can make to the study of social change. The pervasive theme of the article is the way in which the concept of a population forces the sociologist to give time a central place in his theory and research.

I. THE BASIC POPULATION MODEL

The backbone of population study is formal demography. The demographer is equipped with a special type of mathematical model which is adaptable to a wide range of problems and which yields proposals that particular kinds of data be studied with particular techniques of description and measurement. Formal demography is the deductive study of the necessary relationships between the quantities serving to describe the state of a population and those serving to describe changes in that state, in abstraction from their association with other phenomena.[3] The central features of demography as a body of knowledge and methods may be approached by considering the population as a model. The generic concept of a population is an abstract view of a universe of phenomena comprising recognizable individual elements Although demography is concerned substantively with humans, it has formal affinity with the analysis of all such collectivities. The first contribution by Alfred J. Lotka, the man most responsible for modern demography, was a study of "the mode of growth of material aggregates."[4] In that paper, he presented the essence of demography in his opening observation that, in a material system, certain individual constituent elements may each have a limited life-period, but the aggregate of a number of such individuals may nevertheless have a prolonged existence, provided there is some process for the formation of new individuals as the old ones are eliminated.

The elements of the basic population model may be specified as follows:

1. The population is characterized as an aggregate of individuals which conform to a given definition. This definition is ordinarily at least spatial and temporal in specificity.

2. The central question concerns the change of the aggregate number of constituent elements through time. Population research is dynamic in this elementary sense.

3. The change through time in the aggregate number is conceptualized as the difference between the number of additions to, and number of subtractions from, that total during the time interval of observation.[5] Decomposition in this way is so characteristic of the demographer's behavior that it is often specified in a definition of the field.[6] If additions and subtractions are further distinguished as births and immigrations on the one hand, and deaths and emigrations on the other, the proposition becomes the so-called demographic equation.[7] In this section of the paper, the population is assumed to be closed to migration; in a subsequent section migration is given special attention.

4. The model is microdynamic as well as macrodynamic. That is to say, the passage

[3] Alfred J. Lotka, *Théorie analytique des associations biologiques*, Part 2: *Analyse demographique avec application particulière a l'espèce humaine* (Paris: Hermann & Cie, 1939).

[4] "Studies on the Mode of Growth of Material Aggregates," *American Journal of Science*, XXIV (1907), 199–216. The author emphasized the generality he intended by using biological terms like "birth" and "death" only in quotation marks.

[5] Boulding has called this proposition perhaps the most fundamental of all science (Kenneth E. Boulding, *A Reconstruction of Economics* [New York: John Wiley & Sons, 1950], p. 190).

[6] See, e.g., Hauser and Duncan, "Demography as a Science," *op. cit.*

[7] Kingsley Davis, "The Demographic Equation," in *Human Society* (New York: Macmillan Co., 1948), pp. 551–94.

of time is identified for the individual constituent elements as well as for the population as a whole. This emphasizes, in Lotka's terms, the distinction between persistence of the individual and persistence of the aggregate. Individual entries and exits are dated, and the difference between date of entry and date of observation, for the individual, is his age at that time. Age is the central variable in the demographic model. It identifies birth cohort membership (as discussed below). It is a measure of the interval of time spent within the population, and thus of exposure to the risk of occurrence of the event of leaving the population, and more generally is a surrogate for the experience which causes changing probabilities of behavior of various kinds. Age as the passage of personal time is, in short, the link between the history of the individual and the history of the population.[8]

5. Once increases and decreases are identified in terms of both personal and population time, attention is focused on properties of the system that determine the limitation of the life-period of the constituents and the formation of new constituents. The emphasis passes from the deaths and births that occur to the individuals, to mortality and fertility, which are cohort processes.[9]

6. The population model is completed by linking together three kinds of functions. The first of these is the number of person-years of exposure of the population within each age interval and time interval; the second is the number of births occurring within each age interval and time interval per person-year of exposure; the third is the number of deaths occurring within each age interval and time interval per person-year of exposure. In brief, they are the age-time structure of the population, and the age-time processes of fertility and mortality. These three functions represent a network of identities within a complete deterministic model. The formal theory of demography is concerned with working out the logico-mathematical relationships among these components and elaborating schemes for their analysis in terms suggested by the structure of the model.

At several points above, reference has been made to the distinction between individual occurrences and cohort processes. The significance of this may be exemplified by reference to mortality. An individual has a lifetime of, say, x years, which is begun at birth and terminated by death. In the population accounts this is recorded as an addition to, and then, x years later, as a subtraction from, the aggregate of one unit. From the standpoint of population size as a function of time, the age of the individual at death is irrelevant. Only the fact and time of death enter the accounting procedure, since the question of which individual dies does not affect the size of the population. The individual is assigned to a temporal aggregate on the basis of his time of birth, because this offers some obvious arithmetical conveniences. Such an aggregate is termed a (birth) cohort.[10] The mortality process for the cohort is the distribution of its membership by age at death (and, since time of birth is identical among the members, by the time of death as well). This distribution is a characteristic of the cohort as an aggregate, but the argument of the function representing it is individual time. Thus the events of subtraction of individuals from the aggregate are trans-

[8] Age is only the most useful case of the general category of intervals, measured from the time of entry into particular kinds of subpopulations or quasi-populations. These are discussed in Sec. II.

[9] An important distinction can be made between biological populations, to which new members are added as a consequence of the event of parenthood occurring to existing members of the population, and other situations in which the population concept is applicable, but the additions are properties of the population as a whole and its total environment. Immigration is a case in point.

[10] The cohort approach has analytical as well as arithmetical advantages. See my "Cohort Analysis," *International Encyclopaedia of the Social Sciences*, forthcoming.

formed into rates for each age-time interval, the numerator of the rate being the number of occurrences of the event of death, and the denominator the area of person-years of exposure of the aggregate to the risk of occurrence of that event during the particular time interval. By this form of calculation the event which characterizes the individual is transformed into the process which characterizes the cohort.

In considering the demographic history of a cohort, there is an obvious cause-and-effect relationship between its mortality process and its age-time structure, that is, the distribution through time of the person-years of exposure. Considered as an age structure, the population at any moment of time is a cross-section of cohorts as age structures, when the cohorts are viewed diagrammatically as if they were stacked in uniformly staggered fashion, each atop its predecessor in time. The procedure for relating the parameters of these two kinds of age structures has been developed by the writer and named the process of "demographic translation."[11]

To complete the basic population model as a web of structures and processes, some fertility mechanism is required. The size of any cohort at birth is provided by the number of births in the period that dates the cohort. Those births that occur in any period may be viewed as a product sum of the age structure of the population in the period and the fertility rates of the cohorts that occupy the various ages of parenthood at that time. These rates are of the occurrence-exposure type elaborated for mortality.[12] Now the period age structure is a cross-sectional translation of the age structures of the successive participant cohorts, and the fertility rates are a cross-sectional translation of the fertility rates of the same cohorts. Thus the process of demographic translation between period and cohort functions is intrinsic to the establishment of interdependencies among all three functions of the basic population model.

To summarize, the system provides a way of generating changes in population size through time, because of the continual destruction and creation of members, as a joint product of population structures and cohort processes. As noted, the population structure in any period is a translation of cohort age structures, and these are in turn the outcome of cohort mortality processes. The circle of formal analysis moves from (1) individual acts of procreation and death to (2) cohort processes of fertility and mortality, and thence to (3) cohort age structures. These are translated into (4) period age structures which combine with period translations of the cohort vital processes to yield (5) the births and deaths which change the size of the population. This mode of analysis presents the problem of structural transformation in terms of the processes that shape and reshape the structure. Thus it is attuned to the tendency of present-day science to regard events rather than things, processes rather than states, as the ultimate components of the world of reality.[13] The contributions of Lotka[14] have established the determinacy of the population structure implicit in fixed processes of cohort fertility and mortality and have provided, at this level, a comprehensive representation of the stable equilibrium model. Work is now proceeding on the establishment of the structural consequences of systematic change in the cohort processes, in order to develop models which are

[11] See my "The Process of Demographic Translation" (paper presented at the 1963 annual meetings of the Population Association of America, Philadelphia), to be published in the *Demography Annual*.

[12] The process is purposely described here as if parenthood were monosexual or non-sexual, for reasons to be amplified in Sec. III.

[13] The philosophical term for this view is "actuality theory" (see "Actuality Theory" in *Encyclopaedia Britannica* (1957 printing), I, 138.

[14] *Op. cit.*, 1939.

dynamic in the customary sense of the term.[15]

II. POPULATION COMPOSITION

The basic population model presented above can be used in consideration of social composition and social mobility as topics for demographic inquiry.[16] Population composition has been defined as the relative frequency of any enumerable or measurable characteristic, quality, trait, attribute, or variable observed for individuals in a population, that is, as any view of an aggregate that recognizes any differences among its individual components. A list of such items would include residential location; ethnic group membership; religion; education; employment; occupation; industry; social roles and memberships; anthropometric, biometric, and psychometric traits; genetic constitution; health status; and attained skills.[17] From this vantage point the demographer's ken seems boundless. Perhaps it is impossible to draw a boundary line around demography and confine its scope to such-and-such phenomena and no others.[18] Perhaps the range of possibilities is limited only by convention, so that the choice of subjects for demographic purview is largely fortuitous.[19] And perhaps the criterion for labeling something as demographic is essentially the same as the criterion for its inclusion in the census, that is, any characteristic of an individual that is useful in administration and policy determination and that can be collected by non-professionals.[20] While such specifications may indeed be apt characterizations of what demographers do, they are less than satisfying as guidelines for the future development of the field. The concern in this section is to attempt to identify some criteria for drawing a line between demographic and non-demographic variables, in order to arrive at an understanding of what it is about demographic analysis that could distinguish it from any other kind of statistical social analysis.[21] The position to be advanced is that limits can be established for the sphere of demographic competence by considering not so much the substance of any characteristic as its adaptability to formulation in the terms of the basic population model.[22]

The argument begins with consideration of a distinction that has been proposed between characteristics that are fixed and characteristics that are changeable.[23] Some characteristics are determinable at birth and fixed for life. These may be distinguished for convenience as the genetic inheritance and the cultural inheritance, in both cases derivative from the parents. The most prominent representatives of the former are sex and color. Under the latter

[15] See my "The Translation Model of Demographic Change," in *Emerging Techniques in Population Research* (Milbank Memorial Fund, 1963), pp. 65–81.

[16] See Schnore, *op. cit.*, for the ideas which prompted this section.

[17] United Nations, Department of Economic and Social Affairs, *Multilingual Demographic Dictionary*, English Section ("Population Studies," No. 29 [New York, 1958]); Hauser and Duncan, "Demography as a Science," *op. cit.*

[18] Hauser and Duncan, *op. cit.*

[19] Schnore, *op. cit.*

[20] Hawley, *op. cit.*

[21] One of the beginnings of sociology as an academic discipline in the United States was the quantitative treatment of social problems. Some such departments were first called "Statistics" but later changed their name to "Sociology." "Population problems" gradually became identified as a major subdivision of these departments (F. Lorimer, "The Development of Demography," in Hauser and Duncan [eds.], *op. cit.*, pp. 124–79).

[22] An analogous approach has been adopted in the study of the economics of capital (see Boulding, *op. cit.*, p. 189).

[23] Grauman, *op. cit.*; Schnore, *op. cit.* Status ascription is a process which ties some changeable characteristics to some fixed characteristics. Status achievement is a process which ties some changeable characteristics to other changeable characteristics.

heading come such non-biological characteristics of parents as ethnic origin, mother tongue, and place and date of birth. The last of these, which identifies cohort membership, is usually represented by age, which is, of course, an invariant function of time and in this sense a fixed variable. Other characteristics are subject to change throughout the course of an individual's life, such as educational attainment, marital status, and the various attributes associated with economic activities.

Now this distinction between changeable and unchangeable characteristics is an important convenience at an operational level, but it is scarcely defensible from the standpoint of the significance of such identifications for behavior, and it conceals a facet of almost all census questions which can be exploited by the demographer. Thus there is merely a distinction of degree rather than kind between inherited and acquired characteristics, since virtually all phenotypic characters manifest the interaction of genetic and environmental factors. From a sociological standpoint sex, race, age, and other "biological" characteristics are learned roles. Although the various types of non-biological identification that can be used to label a person at birth may be regarded as having a persistent influence on his lifetime behavior, their fixity is only a convenient approximation to a much more complex and dynamic reality.

There are several senses in which the changeable characteristics are fixed. Many of them endure for extended periods of time, and are frequently permanent within or beyond particular age limits. Thus educational attainment is by and large established during prematurity; marital status tends to be fixed for lengthy periods within each stage of the sequence of single, married, and widowed; labor-force participation for many involves a single entry early in life and a single departure late in life; religion and citizenship are fixed for life for the majority. Now it is not disputed that changes can and do occur in these variables. But the significant point for the student of the population as an aggregate is that these changes tend to occur within a narrow age range for most of the population.

This aspect of temporal persistence in most census characteristics is manifest in and enhanced by census practice. Thus it is accepted procedure to attempt to record those who habitually live in an area (including absentees and excluding transients) in order to get at "usual residence" —the place were a person "lives." A population is considered as consisting of inhabitants, a term that implies both spatial fixity and temporal endurance. Occupation and place of residence are among the characteristics that a person changes most frequently. But census procedures ordinarily involve the attempt to establish what these are usually rather than momentarily. This is a partial explanation for the fact that the census is primarily a classificatory rather than a measuring instrument. In the same way, migration is identified as only those changes of residence that carry some implication of permanency. In summary, population characteristics are for the most part the results of attempts to achieve relatively enduring labels by definitional devices, and changeable characteristics may be distinguished by the extent to which they manifest temporal persistance.

A further type of fixity in the realm of changeable characteristics concerns fixity of sequence. Several important population characteristics have high probabilities of change, but in relatively restricted ways. The classic case is age (which departs from its fixed sequence only through age misstatement, a form of intercohort migration). Reproductive parity and educational attainment in terms of years of schooling fall under the same heading. Likewise, specific marital statuses can only be changed, by law, into a limited number of particular other marital statuses. Some occupations can be arranged in career sequence, particularly if the earlier occu-

pation is in some sense apprenticeship for the later. Sometimes one variable is characteristically sequential with another, for example, the close relationship between certain levels of education and certain categories of occupation, which in turn are closely related to income levels.

The aspect of fixity of characteristics and sequences has been stressed here because it makes possible the application in analysis of the array of demographic techniques based on the concept of a population. The argument is, in a sense, a generalization of the implications of Grauman's observation that the principal relevance of the distinction between fixed and changeable characteristics is that population segments which have the same fixed characteristics can be treated by estimating methods analogous to those which are employed in estimating population totals.[24] It is the writer's view that the special contribution of the demographer to social analysis is focused on those items of individual information which can be thought of as defining quasi-populations, because they endure. Thus a characteristic may be viewed as an individual's residence over a period of time. The time interval has a beginning and an end for the individual—an entry into and an exit from that particular quasi-population—and within the interval the individual is exposed to the risk of occurrence of various events, in particular, that of departure from that quasi-population. It is at least operationally conceivable that not only an enumeration of the individuals within these quasi-populations at successive times can be obtained, but also a registration of entries and exits. Full utilization of the power of the population model would also require determination of the length of time each individual has spent within the quasi-population. The most commonly used interval is age, and it ordinarily serves as a surrogate for more precise interval determination. Age is the outstanding representative of a large class of measurements of the length of time elapsing since the occurrence of a cohort-defining event, but is a satisfactory substitute for the particular duration only to the extent that there is small variance in age at entry into the quasi-population in question.

To summarize this section, the cutting edge of methodology has been used to guard against the presumptuous position that the demographer is sole custodian of census materials. In the writer's view, the talents of the demographer are most usefully employed in considering such items of information about individuals as can be conceptualized as quasi-populations by virtue of their property of persistence through time. This perspective is designed not so much to inhibit the expansiveness of the demographer's statistical work as to suggest a program for extending his activities into areas in which his contribution is unique.

III. POPULATION PROCESSES

The position has been advanced that the criterion which identifies those situations in which the demographer has something special to offer is methodological rather than substantive. To exemplify further this viewpoint, the present section examines the applicability of the population concept in the study of the three processes that are clearly integral to the field of demography considered substantively—fertility, mortality, and migration—and to the closely associated process of nuptiality. The outcome of the presentation is that the basic population model is tailor-made for mortality analysis, plays an important but incomplete role in the measurement of nuptiality, has proliferated in several analytically advantageous ways in fertility research, and finally is appropriate for answering questions about one kind of migration but not about another.

The prototpe of statistical analysis in demography is the life-table. A cohort is taken from birth throughout the lifespan, with its numbers reduced age by age on the basis of the mortality rates, the ratios

[24] Grauman, *op. cit.*

of occurrences of death to person-years of exposure to the risk of death. The area of exposure in each age and time interval is reduced successively by occurrences which depend in turn on previous areas of exposure and their mortality rates. Whatever other subpopulation or quasi-population may be studied, the events that reduce membership must include mortality as well as the departures that are specific to the particular definition of membership. The elegance of the system of life-table functions has inspired the whole array of attempts to convert other processes, often substantively quite dissimilar, into analogues of mortality.

In the sphere of nuptiality analysis, the parallel with mortality is readily drawn and the basic population model successfully applied. Every person begins life single and most persons suffer "death" as a single person by experiencing the event of first marriage, an event that is irreversible provided a strict definition of the single state is maintained. Similarly the successive stages of married life may be studied as attrition processes, for example, the dissolution of marriage by divorce or widowhood. But the population model is restricted in its usefulness to the situation in which the event may reasonably be considered as occurring to an individual. Now marriage is in fact an event that occurs to two persons simultaneously, and the exposure to the risk of occurrence of marriage is a function not only of the personal characteristics of the man and woman involved, but also of the general state of the marriage market—the relative availability of spouses of either sex. This problem has proven completely intractable to conventional modes of demographic analysis.[25] This is the reason for specifying above that the basic population model is non-sexual or monosexual. The relationships between probabilities of marriage and the sex-age composition of the unmarried population are not expressible in terms of formal interconnections between occurrences and exposures. This is a clear-cut case of the need for measurements of properties of the aggregate in determination of probabilities of individual behavior, as discussed in more detail in Section IV.

In fertility research, one important improvement in methodology has been the extention and generalization of the basic notion of a cohort from its original signification of birth cohort to cohorts identified by common date of occurrence of other significant events. The variables that have been exploited in modern fertility measurement are number, age, marital status, marital duration, parity, and birth interval. These six variables may be grouped in three pairs, in order, each pair consisting of a status—which identifies quasi-population membership—and a time interval since acquiring that status—"age" within the quasi-population. The demographic characteristics pertinent to the act of parenthood are most succinctly identified as a series of time points: date of birth of the prospective mother; date of her marriage; date of birth of each preceding child; and date of current birth. The intervals, then, are the differences between pairs of successive time points. The statuses imply membership in various types of cohort: the birth cohort, the marriage cohort, the first parity cohort, and so forth. Temporal aggregation afresh on the basis of the most recent event in the reproductive history provides a mode of efficient analysis of the frequency and the time distribution of the next succeeding event.[26] The only formal problem in this sequence is the formation of marriage cohorts out of birth cohorts, as discussed in the preceding paragraph.

Migration is clearly the most complex demographic process to discuss from the standpoint of the basic population model. The problems that arise are discussed here first for external migration and then for inter-

[25] See my "Bisexual Marriage Rates" (paper read at the annual meetings of the Population Association of America, 1961).

[26] See my "La mesure des variations de la fécondité au cours du temps," *Population*, XI (January-March, 1956), 29–46.

nal migration. Immigration and emigration, the terms generally used in distinguishing the two directions of external migration, are on an equal footing with fertility and mortality as modes of entry and exit from the total population. This circumstance follows from the fact that the population is customarily defined in spatiotemporal terms: immigration and emigration represent the crossing of spatial boundaries just as fertility and mortality represent the crossing of temporal boundaries. The parallel may be extended to the conceptualization of emigration as a type of mortality. There are no unique difficulties of a formal kind in considering exposure to the risk of occurrence of emigration from the population, with a determination of the probabilities of emigration in each time interval, for members of successive birth cohorts, following the life-table format. Furthermore emigration, like mortality, is a process of exit from the total population, and therefore from every constitutent subpopulation of quasi-population.

When attention is turned to immigration, the analogy immediately dissolves. This may indeed be an important mode of addition of new members to the receiving population, but the events that constitute it do not occur to members of the receiving population. In contradistinction to fertility, the initiation of immigration is exogenous to the population being studied and can be built into the model only on an *ad hoc* basis because the exposure to the risk of immigration lies outside the defined population. For this reason, immigration research has not been able to exploit the measurement techniques that emanate from the basic population model. The characteristics of the population which yield various patterns of immigration are characteristics of the aggregate rather than of individuals within the aggregate. In this sense, immigration is a subdivision of the general study of ecosystem interchange, a branch of population theory which is much more developed for nonhuman than for human populations.[27]

The process of internal migration may be regarded on the one hand as a special application of the quasi-population concept, or on the other hand as the prototype of a different but related kind of population model. If the territory that defines a population is divided into subterritories, each with its own subpopulation, then the movement of an individual from one subterritory to another is formally analogous to passage from any one status to another. If the focus of interest is the subpopulation itself, then in-migration and out-migration are at that level analogous to the processes of immigration and emigration as discussed above for the total population. But if the total population within which the movements are occurring is the focus of attention, then it is more natural to speak of the movements not so much in terms of entry into and exit from particular subpopulations, as in terms of interstitial movement between subpopulations.[28] This way of describing the process places the subject within the reach of the theory of Markov chains, a type of mathematics that possesses great potentialities in demographic research. An initial distribution and a terminal distribution, termed column vectors, are related to one another by a square matrix of transition probabilities. These are the conditional probabilities of moving to a particular terminal location, given a particular initial location.[29]

[27] Boulding, *op. cit.*, chap. i.

[28] Similarly immigration and emigration may be considered as species of internal migration from the standpoint of the population of the world. Indeed the world population as a model has considerable theoretical convenience because, thus far at least, there is only one mode of entry and one mode of exit.

[29] The approach has been applied with success in solving some residual difficulties in stable population theory: see, e.g., D. G. Kendall, "Stochastic Processes and Population Growth," *Journal of the Royal Statistical Society*, XI (1949), 230–65; Alvaro Lopez, *Problems in Stable Population Theory* (Princeton, N.J.: Office of Population Research, 1961). Applications to the study of intergenera-

The basic population model and the transition matrix model are variants of a single formal system, which have their own special advantages for two different categories of problems. The basic concept of a population has been characterized as spatiotemporal. Accordingly, two types of changes may be distinguished: metabolism, or replacement in time, characterized by the processes of fertility and mortality; and migration, or replacement in space. An emphasis on metabolic transformation leads to a preference for the model of entry, exposure, and exit, as discussed above. An emphasis on migratory transformation leads to a preference for the transition-matrix model. The two types of models share one important feature. If the matrix of transition probabilities is held fixed, it may be shown that the column vectors move toward an equilibrium state which represents the latent structural propensities of the processes characterized by the matrix. This is, of course, a precise analogy to stable population theory. Both mathematical models direct analytic attention away from the consequent structures and toward the determinant processes. But a distinction of degree or emphasis remains. In considering the various census characteristics in the preceding section, it became clear that some of them were more adaptable to the quasi-population concept than others, and that the degree of adaptability hinged on the frequency of entry and exit, or, said otherwise, on the degree of temporal persistence. It seems reasonable to propose that those characteristics that are too variable from time to time to be usefully conceptualized in quasi-population terms can be accommodated methodologically within the transition matrix approach. As an alternative mode of division of labor between the two models, it may be suggested that the transition-matrix approach is more suitable for comparative cross-sectional analysis, which focuses on short-run period-by-period changes in the population structure, while the population approach is more suitable for the study of behavior associated with the life-cycle, which focuses on long-run cohort-by-cohort transformation. But these should be considered as tentative and probably premature forays beyond a fecund methodological frontier.

IV. MACROANALYSIS AND MICROANALYSIS

The study of population comprises not only a system of formal relations but also various systems of substantive relations between parameters of the population model and other variables.[80] Now the parameters of the population model are expressed as concrete phenomena rather than as analytic abstractions. In its substantive component, demography is an observational proto-science, and the concrete objects of observation may be examined through various frames of reference and from various analytic perspectives. The study of systems of analytic relations between population parameters and other variables lies within the purview of the various abstract sciences. The term "analytic relations" is used here to convey the sense of appraisal of probabilistic covariation within a particular frame of reference rather than analysis in the literal sense of decomposition.

Various consequences follow from the

tional occupational mobility have been less successful because the column vectors can be uniquely specified neither in temporal location nor in constituents. See, e.g., S. J. Prais, "The Formal Theory of Social Mobility," *Population Studies*, IX (July, 1955), 72–81, and his "Measuring Social Mobility," *Journal of the Royal Statistical Society*, Series A, CXVIII (1955), 56–66; Judah Matras, "Comparison of Intergenerational Occupational Mobility Patterns: An Application of the Formal Theory of Social Mobility," *Population Studies*, XIV (November, 1960), 163–69, and his "Differential Fertility, Intergenerational Occupational Mobility, and Change in the Occupational Distribution: Some Elementary Interrelationships," *Population Studies*, XV (November, 1961), 187–97.

[80] Lorimer (*op. cit.*, p. 165) has asserted that the concept of "pure demography" is an illusion except as the skeleton of a science.

circumstance that the parameters of the population model are defined concretely. (1) Population studies may be located within the realm of any of the abstract sciences. In particular, the subject straddles the biological and social sciences. The term "science" is used to mean an abstract area of empirical inquiry determined by a particular orientation and frame of reference, as distinct from proto-scientific approaches to concrete phenomena in all their aspects, like geography, ethnography, and, as generally defined, demography. (2) The professional demographer is almost always identifiable also as a substantive specialist with competence in a particular science. The circumstance that most American demographers are also sociologists is somewhat fortuitous, but does help to explain the tendency to define demography by contiguity as a field within sociology. That this is an incomplete view is indicated by the array of different kinds of scientists found in the International Union for the Scientific Study of Population, and even in the Population Association of America. (3) The diffuseness of substantive interest in questions defined by the population model and the narrow limits of the formal core of the discipline are sufficient to explain why it does not have the dignity of academic autonomy as a department of university instruction. For convenience, demography is housed within particular other departments, the choice being to some extent historical accident and varying considerably from country to country.[31] (4) The concreteness of content and the sophistication of the special methodology account for the continued tolerance of the demographer as a resident alien within different disciplines. The data with which the demographer works are grist for almost everybody's mill, and the models he employs are adaptable to a wide variety of situations, although the former type of usefulness to colleagues has been much more widely exploited than the latter. Indeed, the neglect of demographic concepts by non-demographers is one justification for the present piece. The demographer's work, in other words, yields problems for investigation by various scientific disciplines and provides an orientation for the solution of these and other isomorphic problems. (5) Under the circumstances, the demographer is likely to participate in interdisciplinary research—because the limits of his subject extend into the provinces of various disciplines—and he is useful as a channel of scientific communication between otherwise disparate orientations. (6) Finally, the concrete definition of the subject makes the demographer more immediately useful than the abstract specialist in the realm of policies directed toward practical, as distinct from intellectual, problems.

The writer has described elsewhere an example of the way in which a concrete object of the demographer's attention may have its various aspects allocated among different abstract disciplines.[32] Research on fertility can be divided into the contribution of substantive analysis in the biological realm—using data about fecundity and fertility regulation to explain observed fertility—and the contribution of substantive analysis in the realm of the social sciences—using data about individuals and groups to explain observed fertility regulation (and even to some extent fecundity). Within the latter realm it is useful to distinguish between psychosocial and sociocultural research, or—to identify more precisely the point to be discussed—between microanalytic and macroanalytic inquiries. The relationships between these two levels of inquiry deserve attention because they are of peculiar relevance to the demographer and because the concept of a population contributes to their clarification.

Much of the discussion of analytic

[31] David V. Glass (ed.), *The University Teaching of Social Sciences: Demography* (Paris: UNESCO, 1957).

[32] See my "Fertility," in Hauser and Duncan (eds.), *op. cit.*, pp. 400–436.

strategies in fertility research has consisted of assertions of the relative importance of microanalytic and macroanalytic levels of inquiry for the explanation of fertility. Thus Vance has called macroanalytic explanations inadequate because they fail to specify the ways in which macrovariables are translated into individual motivations.[33] In their commentary on this assertion, Hauser and Duncan have labeled the psychosocial variables as superficial, and proposed that the deep-seated macroanalytic causes underlie them.[34] The difficulty of adjudicating competing claims like these is that the criteria of relative success differ. If the individual is the unit of analysis, then success is measured by explanation of variance among individuals; if the population is the unit of analysis, then success is measured by explanation of variance among populations. The issue has not been confined to the field of fertility. Several influential demographers have decried the circumstance that most migration analysis ignores the study of the motivations behind particular individual movements by its macroanalytic focus on net migration.[35] Schnore has taken the opposite stand concerning sociological interest in mobility.[36] He has expressed concern that the majority interest in the correlates of individual behavior means short shrift for macroanalytic inquiries into interdependencies of population composition and social structure. In the field of mortality research, the microanalytic approach is winning by default, because almost no student of the subject seems interested in asking macroanalytic questions.

A final controversy deserves mention. The variables in individual correlation are descriptive properties of individuals; the variables in ecological correlation are descriptive properties of populations (although they are computed by deriving summary indexes of the properties of individual members of the respective populations). Robinson has asserted correctly that individual correlations cannot be inferred from ecological correlations, and has asserted incorrectly that the purpose of ecological correlations must be to discover something about the behavior of individuals.[37] This debate has special pertinence for the demographer because of one characteristic feature of the population concept. Given the definition of a population as an aggregate of members, it appears superficially that the characteristics of the population are merely derivative from the characteristics of individuals by summation. The situation is in fact much more complex than that. Just as the properties of individual members may be used in aggregate form as properties of the population, so the properties of a population may be used as properties of its individual members.[38] The macroanalytic level of inquiry consists of propositions or statements of relationships among the properties of the population as the unit of reference. The microanalytic level of inquiry consists of propositions

[33] Rupert B. Vance, "The Development and Status of American Demography," in Hauser and Duncan (eds.), *op. cit.*, pp. 286–313.

[34] *Op. cit.*

[35] Donald J. Bogue, "The Quantitative Study of Social Dynamics and Social Change," *American Journal of Sociology*, LVII (May, 1952), 565–68; Vance, *op. cit.*; C. Horace Hamilton, "Some Problems of Method in Internal Migration Research," *Population Index*, XXVII (October, 1961), 297– [illegible]

[36] Schnore, *op. cit.* [illegible]

[37] W. S. Robinson, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review*, XV (June, 1950), 351–57; H. Menzel, "Comment on Robinson's 'Ecological Correlations and the Behavior of Individuals,'" *American Sociological Review*, XV (October, 1950), 674. See also L. A. Goodman, "Ecological Regressions and Behavior of Individuals," *American Sociological Review*, XVIII (December, 1953), 663–64; O. Dudley Duncan, and B. Duncan, "An Alternative to Ecological Correlation," *American Sociological Review*, XVIII (December, 1953), 665–66.

[38] J. A. Davis, J. L. Spaeth, and C. Huson, "A Technique for Analyzing the Effects of Group Composition," *American Sociological Review*, XXVI (April, 1961), 215–25.

or statements of relationships among the properties of the individual as the unit of reference. In general it is invalid either to transform a proposition about populations into a proposition about individuals or to transform a proposition about individuals into a proposition about populations. The relationship among individual characteristics, expressed as a regression equation linking individual variables, will generally have different parameters from one population to another. Now most sociological theory is pitched at the microanalytic level and therefore requires a test based on observations of individuals. This does not imply that macroanalytic theory is a lesser breed of theorizing, nor that it is merely derivative and a temporary substitute employed for the sake of convenience.

The question of the relationships between macroanalysis and microanalysis is important in current economic thought. Most theories in economics (as in sociology) are microtheories, while most empirical descriptions contain measurements of macrovariables which are functions, such as averages, of microvariables. The parameters in the macroanalytic regression equations are weighted averages of the parameters in the microanalytic regression equations because the former system is dependent not only on the latter but also on the composition of the population.[39] One prominent direction of resolution of this and other problems which exploits the magnitude of the latest computers is the microanalysis of socioeconomic systems, an attempt to generate aggregate properties from properties of individuals.[40] As a general rule, the theoretical systems of the economists have not encompassed the problems of differences between systems *qua* systems through time or space.[41]

There is another respect in which the microanalytic and macroanalytic levels of inquiry differ in character, and it leads to an important example of the utility of the population concept. The properties of distributions of variables are specific to populations rather than to individuals. An important question about a population is how to explain the differential distributions of various populations in terms of some individual-specific characteristic. Now microanalytic relationships are of the form: If an individual has characteristic A, then he has a probability p of having another individual characteristic B. This may be used to predict the distribution of B, given the distribution of A, but it does not answer the question: What determines the distribution of A? The approach that uses the basic population model emphasizes the events of acquiring and losing some characteristic. From the standpoint of the individual, movement into and out of categorical locations is placed in the perspective of the life-cycle; from the standpoint of the population, the distribution by categories is viewed as a consequence of processual parameters of fertility and mortality, using these terms in the broad as well as the narrow sense. More specifically, attention is directed to the movement from one structure to another by means of two questions: "Given this kind of exposure, what is the probability of the occurrence of departure from exposure?" and "Given departure from exposure, what is the consequence for subsequent exposure?" Thus this approach places emphasis on changes through time. More precisely, the emphasis is on long-run time, or time as the biology of evolution considers it, as distinct from short-run time, or time as used in the equations of physics. The latter is the analogy

[39] H. Theil, *Linear Aggregation and Economic Relations* (Amsterdam: North-Holland Publishing Co., 1954).

[40] Guy H. Orcutt, Martin Greenberger, John Korbel, and Alice M. Rivlin, *Microanalysis of Socioeconomic Systems* (New York: Harper & Bros., 1961).

[41] The problems of distinction between analyses of individuals and of aggregates are not confined to the social sciences. For a brief view of the parallel dilemma in physics see William [illegible], *An Introduction to Probability Theory and Its Applications* (2d ed.; New York: John [illegible], 1957), I, 356.

for the study of covariation of individual and population characteristics; the former is the analogy for the study of population dynamics.

In the area of conjuncture between the macroanalytic and microanalytic approaches to the study of behavior, the cohort as a population element plays a crucial role. It is a device for providing a macroscopic link between movements of the population and movements of individuals. The conceptual gap between individual behavior and population behavior is provided with a convenient bridge, in the form of the cohort aggregate, within which individuals are located and out of which the population as a function of time is constructed from the sequence of cohort behavior patterns. Thus the cohort is a macroanalytic entity like the population, but it has the same temporal location and pattern of development as the individuals that constitute it. It seems to the writer that the analysis of cohort structures and processes is a valuable intermediary between the analysis of individual behavior and the analysis of population behavior, in attempting to increase the possibilities of cross-fertilization. The concept of a population, which is closely allied with the concept of a society, is brought closer to the concept of an individual, when the latter is viewed as a member of a cohort aggregate which is in turn a constituent of a population. Thus one avenue is provided in sociology for the perplexing questions of the relationships between the individual and the society.

To conclude this section, a note seems worthwhile concerning the partnership of demography and human ecology, a partnership manifest in the central position within each discipline of the concept of a population and in the frequency with which particular scientists have interests in both areas. Human ecology may be characterized as the study of social organization as a property of a population in interaction with its environment.[42] The characteristic method of this study is employment of the spatiotemporal orientation to provide dimensions for the observation and measurement of organization. The concrete definition of a population within both demography and human ecology implies that the two principal definitional axes of each are space and time, and that each discipline will have an expansive view across various spheres of learning. To some extent there has been a division of labor between the ecologic and the demographic orientations: the former has focused more on variations in space, and the latter more on variations in time. The central concept of the ecologist has been the community, defined minimally by spatial co-occupancy. To some extent it is possible to make the parallel assertion that the central concept of the demographer is the cohort, defined minimally by temporal co-occupancy.[43] The approaches converge in orientation and bring the demographer and the human ecologist together as the ecologist begins to ask dynamic questions and the demographer concerns himself with spatial distribution. The parallel between the cohort and the community is not to be pressed too far because the community is viewed by the ecologist as capable of being considered a self-sufficient societal organism, whereas the cohort is for the most part a statistical plural with common characteristics stemming from its definitional basis of temporal location but without the integration implicit in various community properties.

V. SOCIAL CHANGE FROM A DEMOGRAPHIC PERSPECTIVE

The purpose of this section is to present some ways in which the concept of a population may contribute to the analysis of social change. The first type of contribution is related to the definition of change. It is common in discussions of the topic to con-

[42] Hawley, "Human Ecology," *International Encyclopedia of the Social Sciences*, forthcoming.

[43] It is interesting to observe that the word "cohort" originally had a spatial referent, because it identified a type of community. This etymological tie is manifested in its kinship with words like "garden" and "girdle."

fine the term "social change" to transformation of the social structure, in contradistinction to the patterned sets of phases in the life-cycles of individuals and other relatively invariant systems of action and interaction.[44] Two contributions to this distinction may be drawn from stable population theory. In the first place, structural transformation is caused by a discrepancy at any point of time between the extant structure and the processes which are responsible for creating that structure. In the second place, life-cycle changes for individuals can be summarized by various indexes of cohort behavior as a function of age (or other appropriate interval). The definition of social change prompted by these considerations is the modification of processual parameters from cohort to cohort. Thus social change occurs to the extent that successive cohorts do something other than merely repeat the patterns of behavior of their predecessors. Given the far greater availability of structural than of processual data of all kinds, knowledge of the nuances of interdependency of period and cohort functions of fertility, mortality, and age distribution (or their analogous forms, if a quasi-population concept is employed) is likely to be essential in research on social change.

The proposed definition of social change is incomplete because it does not distinguish long-run from short-run change. Statistical contributions to the separate measurement of each for quantitative materials have been unimpressive because the distinction to be drawn does not actually hinge on the length of time elapsing but on the consideration that such changes have different determinants, and this can only be supplied by a person with knowledge of the content rather than the form of the data. Again the basic population model provides one direction of resolution of the difficulty. If functions of process are examined for a series of cohorts over their age spans, two distinct types of changes may be observed. One of these is the manifestation of a period-specific event or situation which "marks" the successive cohort functions at the same time, and thus at successive ages of the cohorts in question. Frequently such manifestations take the form of fluctuations, in the sense that a counteracting movement occurs subsequently which erases the impact of the situation in the eventual summary for the cohort. The other type of change is characterized by differences in functional form from cohort to cohort other than those betraying the characteristic age pattern of the period-specific event. With full recognition of the incompleteness of the view, it is suggested that among the various sense of "short run" and "long run," an important part of the distinction can be captured by statistical operations designed to segregate the period-specific and cohort-specific variations as functions of age and time. As a not entirely parenthetical remark it may be suggested that one contrast between demography and other types of quantitative sociology has been the emphasis of the former on long-run change and of the latter on short-run or no change.[45]

Throughout this essay, attention has been focused almost exclusively if implicitly on the level of the total population and its less tangible partner, the society. But the concepts introduced are clearly applicable with little modification to the other levels of social organization. In particular the demographic approach provides some methodological resources for a dynamic approach to organizational structure. Any organization experiences social metabolism: since its individual components are exposed to "mortality," the survival of the organization requires a process of "fertility." The problem of replacement is posed not only for the total organization but also for every one of its differentiated

[44] T. Parsons, "Some Considerations on the Theory of Social Change" (1960). (Mimeographed); Hawley, "Change and Development," in *Human Ecology* (New York: Ronald Press Co., 1950), p. 319.

[45] Cf. Boulding, *op. cit.*, p. 26; Lorimer, *op. cit.*, *passim.*

components.[46] The ineluctability of social metabolism is from one view a problem that any organization must solve in the interests of continuity and from another view a continual opportunity for adaptation and change.[47] Furthermore, a plausible case can be made that the processes of "fertility" and "mortality" provide revealing insights into the present character of an organization as well as predictions of its future shape. As a final note on the demography of organizations, it is evident that they themselves can be treated as individuals within a population of organizations of like type, to approach the changing structure of the larger society from another viewpoint.

It is probably true that many sociologists view population data with less than excitement because these seem to provide distributive descriptions of aggregates rather than structural information about groups. This perspective ignores the interdependency which must exist between the functioning of organizations and the demographic characteristics of the aggregates of members of these organizations. The institutional structure rests on a population base, in the sense that particular functions are dependent for their performance on the presence of particular categories of persons. The most elementary recognition of the point comes in the commonplace research practice of distinguishing three types of variables—dependent, independent, and control variables—with demographic data falling under the third heading. The implication is that the composition of the population plays a necessary role (almost tautologically so), but not a sufficient one, as a set of constraints on the degree and direction of change in the institutional structure.[48] Now abstracting from the population composition through the use of the control technique may be a useful practice in the static analysis of covariation, but these parameters become variables as time passes and the questions of social change arise. For analogous reasons, economic theory was able to progress without demographic variables only so long as economists failed to raise questions about economic development. Inquiry into the relationships between population composition and institutional structure promises large rewards for the person interested in developing a dynamic theory of society.

Finally the population model offers a strategy for helping to resolve one of the most frustrating methodological issues in the study of social change. Two modes of conceptualizing and describing change may be distinguished, termed loosely the qualitative and the quantitative. The former mode ordinarily appears as an approximately ordered sequence of discrete complexes which somehow replace or displace or merge with their temporal neighbors. Analysis relies on before-and-after comparisons or at the most on some variant of the idea of stages. With such conceptualizations it is most difficult to achieve operational precision, let alone statistical data. The latter mode of the study of change most commonly yields a precisely dated series of measurements of one or another particular element of a qualitatively homogeneous type. Precision of observation is achieved at the cost of qualitative richness. In some ways the demographic approach is a combination of these two procedures. The concept of a population suggests the examination of a succession of overlapping stages, based on elements of various qualitative (categorical) types, quantified in

[46] A penetrating early contribution to this discussion, which seems to have been ignored, is P. A. Sorokin, and C. Arnold Anderson, "Metabolism of Different Strata of Social Institutions and Institutional Continuity" (Comitato Italiano per lo studio dei problemi della popolazione [Rome: Istituto Poligrafico dello Stato, 1931]). Cf. Georg Simmel, "The Persistence of Social Groups," *American Journal of Sociology* (1897–98), trans. Albion W. Small; abridged in Edgar F. Borgatta and Henry J. Meyer (eds.) *Sociological Theory: Present-Day Sociology from the Past* (New York: Alfred A. Knopf, Inc., 1956), pp. 364–98.

[47] Ryder, "Cohort Analysis," *op. cit.*

[48] Cf. Hawley, *op. cit.*

terms of the frequency of each and the temporal distribution of individuals within the type, and the progression of aggregate indexes for the total population. A satisfactory dynamic theory of society requires a frame of reference that can establish propositions relating quantitative changes in "inputs and outputs" to the organizational transformations that they manifest and induce.[49] One such frame of reference is the concept of a population.

The case for the demographic contribution to the study of social change can easily be overstated. Clearly there are alternative procedures of great promise which have no particular connection with the population model. For example, there are many aggregate data of major significance for structural transformations such as the content of material and normative technology, and there are compositional data based on units of observation such as roles or norms rather than individuals, which require different kinds of model. But at the very least, population change provides a reflection of social change, conceived in any way, a reflection that deserves a place in efficient research because its data are well defined and well measured and its methodology is sophisticated.

VI. CONCLUSION

In this essay the demographer has been characterized as an agent for a particular type of model, the use of which implies particular kinds of measurements and particular directions of substantive inquiry. The influence the population concept might have on the shape of social analysis is threefold. In the first place, the demographer's mode of conceptualization never strays far from the mathematical in substance if not in language. This emphasis implies not only quantification but also persistent attention to some of the necessary components of explanation of societal behavior. Second, the demographic approach is both aggregative and distributive. The basic model is macroanalytic in form, and inclines the student toward a view of social systems in their totality. Nevertheless the model is so designed that it offers a convenient confrontation with some central issues of theorizing at different levels of organized reality. In particular the cohort provides an aggregative format within which the phases and facets of the individual life-cycle are imbedded, and through which the events experienced by the individual may be translated into the population processes that shape population structures. Finally the questions that are of central interest to the demographer are by definition dynamic. He is forced by his methodology to ask not so much about the association of characteristics as about the correlates of changes in characteristics, or, in his terms, about the perpetual interplay between occurrence and exposure. The central place of time in the demographic schema is most evident in the conceptualization of structure as a consequence of evolving process. Now these emphases all require qualifications, and particularly warnings, about what they neglect, but the same is true of any model. In the long run the utility of any approach to research is determined by the test of survival as measured by the fruits of inquiry in its image. To this writer the concept of a population has a high probability of high fertility.

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[49] T. Parsons, and Neil J. Smelser, "The Problems of Growth and Institutional Change in the Economy," in *Economy and Society* (Glencoe, Ill.: Free Press, 1956), pp. 246–94.

RELIGIOUS OBSERVANCE AND FAMILY FORMATION IN ISRAEL: SOME INTERGENERATIONAL CHANGES[1]

JUDAH MATRAS

ABSTRACT

Data are presented relating to intergenerational change in religious observance and in family-formation atterns of Jewish women in Israel. Change in extent of religious observance is almost always in the irection of diminished observance and is more frequent among Oriental immigrant and Israeli-born omen than among European immigrant women. Among Israeli-born and European immigrants high lucation is associated with intergenerational stability, whereas among Oriental immigrant women it is ositively associated with intergenerational *change* in extent of religious observance. Less than half the omen reported having considered "desired family size" or having practiced contraception. Both conderation of "desired family size" and practice of contraception are negatively associated with religious oservance; but "non-observant" daughters of "observant" mothers are as likely to report family planning actices as are "non-observant" daughters of "non-observant" mothers.

I. INTRODUCTION

This paper presents data relating to inrgenerational changes in extent of reliious observance and in patterns of family rmation in Israel. Aside from their bearg upon social change in Israel generally, e present data also indicate differences tween changes occurring in new immiant and in veteran resident subgroups, spectively. The extent to which patterns change characteristic of new immigrants id of old settlers, respectively, and of the fferent ethnic origin groups, have tended converge or to diverge is of particular terest with reference to Israel's problems absorption of immigrants.

The extent to which the Jewish commuty in Palestine and Israel was, or is, reious or irreligious has been the subject much speculation, guesswork, argument, id very little systematic knowledge. Comunities of religious Jews in Palestine date om the Middle Ages, and it is usually esumed that the entire Jewish population Palestine prior to the modern Zionistspired immigration was very orthodox. In e Jewish community of pre-independence lestine, both pro-Zionist and anti-Zionist religious organizations and political parties were active and, in addition, the non-religious (but not at all necessarily anti-religious) Zionist parties always claimed many religious adherents.

In the 1949 elections to the first Israeli *Knesseth* (Parliament; the date of Israel's independence is May 14, 1948), the religious political parties polled altogether 13.9 per cent of the vote.[2] All knowledge of the Oriental (Asian, African, and Middle Eastern) Jewish communities strongly suggested that these were traditional communities in many respects and, in particular, were characterized by relatively strict religious observance. As a result it was anticipated that, in subsequent elections to the *Knesseth,* the religious parties would surely gain additional support from among the Oriental immigrants. The religious parties were doomed to disappointment, and the nonreligious parties elated, when in the 1951 elections the religious parties polled only 12.5 per cent of the vote. The religious parties did make some gains in subsequent elections, but never polled as much as 16 per cent of the total vote despite very farreaching changes in the size and composition of the Jewish population of Israel.[3]

[1] This paper is based in part upon materials first esented in J. Matras, "Israel: Absorption of imigrants, Social Mobility, and Social Change" npublished Ph.D. dissertation, Department of ciology, University of Chicago, August 1962).

[2] Israel Central Bureau of Statistics, *Statistical Abstract of Israel,* No. 13-1962, (Jerusalem: Government Printer, 1962), Sec. X, Table 1, pp. 522-23.

[3] *Ibid.*

The actual extent of religious observance in the Jewish population of Israel remains a matter of conjecture.

Data from a survey of maternity cases in Israel have permitted some comparison of the extent of religious observances among the major groupings, by geocultural origin and length of residence in the Jewish population of Israel.[4] In general European-born and Israeli-born women were much less likely to report strict observance of religious regulations and prescriptions than were women born in Asia, Africa, or the Middle East (hereinafter denoted "Oriental" immigrants). Among immigrant women born in Europe, the new immigrants (those immigrating in 1948 or later) did not differ with respect to extent of religious observance from the veteran residents (those immigrating prior to 1948). But among Oriental immigrant women, the new immigrants were much more likely to report strict observance than were veteran residents, and veteran residents were more likely to report non-observance. The present paper draws upon data from the same source but focuses directly upon the extent and directions of intergenerational *change* in religious observance and upon the implications of such changes for patterns of family formation.

II. DATA AND CLASSIFICATION BY EXTENT OF RELIGIOUS OBSERVANCE

The data derive from a survey of Jewish maternity cases carried out in Israel between August, 1959 and March, 1960.[5] The survey was conducted in 23 hospitals in Israel (in which some 97 per cent of all births to Jewish mothers in 1958 took place) and included *all* women giving birth during a 60-day period in Tel Aviv-Jaffa, during a 42-day period in Jerusalem, and during 12-day periods in hospitals in other places. The women were interviewed by nurses, ordinarily within the first 3 or 4 days after delivery.

Demographic details on the questionnaire included age, place of birth, father's place of birth, year of immigration to Israel, year of marriage, and present place of residence. The questionnaire also covered socioeconomic items including the number of school years completed, present and past employment status, husband's occupation, and number of dwelling rooms. In addition, complete pregnancy histories were obtained including year of each conception, how each conception terminated, and method of contraception, if any, used prior to each pregnancy.

Almost all the women were interviewed about their own religious observance. The women in the sample who stated that they were not religious or not at all observant of religious prescriptions and traditions were classified as "non-observant." Those who indicated that they *were* observant of religious traditions were classified as "observant" or as "partially observant," according to whether they stated that they did or did not observe the religious ritual bath traditions. The division between "observant" and "partially observant" is made on the basis of observance or non-observance of the ritual bath traditions, because these are associated with more general religious regulation of marital sexual relations. However, it should be noted that in the case of Oriental women, observance of the ritual bath traditions is not always accompanied by religious regulation of sexual relations and does not always represent as high a degree of orthodoxy as it does for women born in Israel or in Europe-America.[6]

[4] Results of this survey are reported in R. Bachi and J. Matras, "Contraception and Induced Abortions among Jewish Maternity Cases in Israel." *Milbank Memorial Fund Quarterly*, Vol. LX, No. 2 (April, 1962); J. Matras and C. Auerbach, "On Rationalization of Family Formation in Israel," *Milbank Memorial Fund Quarterly*, Vol. LX, No. 4 (October, 1962); and R. Bachi and J. Matras, "Family Size Preferences of Jewish Maternity Cases in Israel," *Milbank Memorial Fund Quarterly*, forthcoming.

[5] See sources in n. 4.

[6] A much more detailed description of this classification is given in Matras and Auerbach, *op. cit.*, Appendix.

About three-fourths of the women interviewed in Jerusalem (those interviewed in the last 30 days of the 42-day survey period there) and some women interviewed in the largest of the Tel Aviv maternity wards were asked about their mothers' religious observance as well. These women almost always knew whether or not their mothers were or had been at all observant and whether or not the observant mothers were or had been observant to the extent of following the ritual bath rules or were only "partially observant."

TABLE 1

JEWISH MATERNITY CASES IN JERUSALEM AND TEL AVIV, ISRAEL, 1959–60, BY PLACE OF BIRTH INDEXES OF CHANGE IN EXTENT OF OWN RELIGIOUS OBSERVANCE RELATIVE TO THAT OF MOTHER*

Maternity Cases: Place of Residence and Place of Birth	N	Percentage Distributions							
		Religious Observance of Mothers				Religious Observance of Maternity Cases			
		Total	Observant	Partially Observant	Non-observant	Total	Observant	Partially Observant	Non-observant
Jerusalem: Total	582	100	69	22	9	100	37	42	21
Born in Israel	198	100	66	24	10	100	36	37	27
Born in Europe or America	94	100	48	27	25	100	38	25	37
Born in Asia or Africa	290	100	77	20	3	100	37	51	12
Tel Aviv: Total	180	100	47	31	22	100	23	37	40
Born in Israel	54	100	41	31	28	100	19	33	48
Born in Europe or America	39	100	33	28	39	100	20	41	39
Born in Africa or Asia	87	100	56	32	12	100	30	34	36

* Source: Surveys of Jewish maternity cases in Israel, 1959–60, reported in the two articles by Bachi and Matras cited in n. 4.

III. INTERGENERATIONAL CHANGES IN EXTENT OF RELIGIOUS OBSERVANCE

The distributions by extent of own religious observance and mother's religious observance are given in Table 1 for maternity cases interviewed in Jerusalem and in Tel Aviv–Jaffa. It is seen that the mothers of both the Jerusalem and Tel Aviv–Jaffa women are reported as more religious than the women themselves. In Jerusalem almost 69 per cent of the maternity cases reported their mothers "observant," although only 37 per cent reported themselves "observant"; although 21 per cent of the maternity cases reported themselves "non-observant," only 9 per cent reported their mothers as "non-observant." The same relationships hold for the Tel Aviv–Jaffa women, except that fewer of both maternity cases and their mothers are reported "observant" and more are reported "non-observant."

In Table 2 it is seen that 59 per cent of the Jerusalem maternity cases and 60 per cent of those in Tel Aviv–Jaffa were neither more nor less religious than their mothers. But while about two-fifths reported themselves *less* religious than their mothers, almost no one reported herself more observant than her mother. Thus the intergenerational change in extent of religious observance took place almost exclusively in the direction of diminishing religious observance.

Data not reproduced here indicate that the more observant the mother, the more likely is the daughter to be less observant than the mother. Of the daughters of "ob-

servant" mothers, 53 per cent in Jerusalem and 46 per cent in Tel Aviv–Jaffa reported themselves "observant" as well; but 47 per cent in Jerusalem and more than half (54 per cent) in Tel Aviv–Jaffa reported themselves *less* religious than their mothers—the majority of the latter being "partially observant" (and *not* "non-observant") in both Jerusalem and Tel Aviv–Jaffa.[7]

By contrast, among the daughters of "partially observant" mothers, the majority remained "partially observant." But of different ethnic origin groups, pronounced differences between European and Oriental immigrant women are evident in the data cited above. European immigrant daughters of "observant" mothers were much more likely to report themselves also "observant" than were Oriental immigrant daughters of "observant" mothers. The Israeli-born daughters of "observant" mothers reported change only slightly less frequently than did the Oriental immigrant women. Among women in Jerusalem reporting their mothers "observant," about

TABLE 2

PERCENTAGE OF MATERNITY CASES WHO ARE AS RELIGIOUS AS, MORE RELIGIOUS THAN, AND LESS RELIGIOUS THAN THEIR MOTHERS*

	N	Percentage Distribution			
		Total	As Religious	More Religious	Less Religious
Jerusalem: Total	582	100	59	1	40
Born in Israel	198	100	60	2	38
Born in Europe or America	94	100	77	1	22
Born in Asia or Africa	290	100	54	0	46
Tel Aviv: Total	180	100	60	3	37
Born in Israel	54	100	59	2	39
Born in Europe or America	39	100	84	3	13
Born in Asia or Africa	87	100	51	3	46

* Source: Same as Table 1.

those changing, almost all changed in the direction of "non-observance" rather than in the direction of greater observance than their mothers. Of daughters of mothers who were "non-observant" almost all were themselves "non-observant"; no one at all reported herself "observant" and only one each in Jerusalem and in Tel Aviv–Jaffa reported herself "partially observant."[8]

Turning now to a comparison of the half of the Oriental immigrants and of the Israeli-born, but only one-fifth of the European immigrant women, reported themselves either "partially observant" or "non-observant." In Tel Aviv–Jaffa just under two-fifths of the European-born daughters of "observant" mothers reported themselves less religious than their mothers; but well over half of the Oriental and Israeli-born women whose mothers were "observant" reported themselves either "partially observant" or "non-observant."

In Jerusalem and Tel Aviv–Jaffa, 23 per cent and 16 per cent, respectively, of the European immigrant women reported changes in the extent of religious observance relative to their mothers. Of the Oriental immigrants, almost half reported

[7] Detailed tables are presented in Matras, "Israel: Absorption of Immigrants . . . ," *op. cit.*

[8] The generation-to-generation shift in the direction away from "observance" may be somewhat understated in the data from a sample of maternity cases. Evidently women who are *not* characterized by a lesser extent of religious observance than their mothers are also more likely to have children and hence to appear in a sample of maternity cases.

such changes; and two-fifths of the Israeli-born maternity cases did so.[9] That these reported changes are virtually all in the direction of *diminishing* religious observance has been noted before and seems abundantly clear from the data of Table 2.

Some tentative hypotheses about length-of-residence effects are possible on the basis of the Tel Aviv data. The numbers of both European and Oriental veteran resident maternity cases are small; nevertheless some differences seem evident (Table 3). In the case of European immigrants, there is no basis for inferring great

[9] The delta (Δ) index (the Index of Dissimilarity) may be computed using the data from Table 1

$$\Delta = \tfrac{1}{2} \sum_{i} | a_i - b_i | ,$$

where a_i = proportion of mothers in the ith religious-observance category, and b_i = proportion of daughters in the ith religious-observance category and may be interpreted as the *minimum* proportion of daughters who would have to change from their mothers' categories in order to yield the observed differences between mothers' and daughters' *distributions* by religious-observance categories.

	Proportion Changing (P)	Index of Dissimilarity (Δ)
Jerusalem: Total	0 406	0.318
Born in Israel	.404	.298
Born in Europe or America	.234	.117
Born in Asia or Africa	.462	.403
Tel Aviv: Total	.394	.234
Born in Israel	.408	.222
Born in Europe or America	.154	.128
Born in Asia or Africa	0.494	0.264

In both Jerusalem and Tel Aviv-Jaffa, the Δ-index is relatively small for European women, indicating relatively small differences between the distributions of mothers and daughters by extent of religious observance. For both Israeli-born and Oriental immigrant groups, the Δ-indexes are always higher than for European-born women, and higher in Jerusalem than in Tel Aviv–Jaffa. For all groups, the proportions of women who actually changed are higher than the theoretical minimum proportions changing implied by the Δ-indexes.

differences in extent of intergenerational change in religious observance between veteran settlers and new immigrants, the main conclusion for both groups being that they are characterized by relative generation-to-generation stability.

The distribution of the Oriental veteran settler maternity cases by extent of religious observance differs radically from that of their mothers ($\Delta = 0.644$), and in fact 86 per cent of them reported change relative to their mothers—all in the direction of diminished religious observance. By contrast, the distribution of Oriental *new* immigrant maternity cases differed from that of their mothers not nearly so radically ($\Delta = 0.232$) and less than half of the women reported changes in extent of religious observance relative to their mothers. Moreover, not all the change was in the direction of diminished religious observance. Although less change was reported by Oriental new immigrants than by Oriental veteran settlers, it is worth noting that the Oriental new immigrants nevertheless reported more intergenerational change in extent of religious observance then did either new immigrants or veteran settlers from Europe.

A number of additional comparisons are possible using the data for the Tel Aviv–Jaffa maternity cases. Table 4 indicates change in extent of religious observance relative to mother by number of school years completed. [For Israeli-born and European daughters of "observant" mothers, the likelihood of themselves remaining "observant" is *higher* for those with post-primary education than for those with only primary education.] Generally, the likelihood of changing relative to mothers' religious observance is evidently *less* for Israeli-born and European immigrant women with post-primary education than for those with only primary education; and although each individual index or measure depends upon a small number of cases, the same result appears for all measures. Thus half the women born in Israel who had completed only one to

TABLE 3

FOREIGN-BORN JEWISH MATERNITY CASES, TEL AVIV, ISRAEL, 1959–60, BY PLACE OF BIRTH AND LENGTH OF RESIDENCE: INDEXES OF CHANGE IN EXTENT OF OWN RELIGIOUS OBSERVANCE RELATIVE TO THAT OF MOTHER*

	N	Proportion Changing (*P*)	Index of Dissimilarity (Δ)
Born in Europe or America:			
Veteran residents	12	0.250	0 250
New immigrants	27	.111	.111
Born in Asia or Africa:			
Veteran residents	14	.857	.644
New immigrants	73	0.410	0 232

* Source: Same as Table 1.

TABLE 4

JEWISH MATERNITY CASES, TEL AVIV, ISRAEL, 1959–60, BY PLACE OF BIRTH AND EDUCATION (AND, FOR ORIENTAL WOMEN, EMPLOYMENT): INDEXES OF CHANGE IN EXTENT OF OWN RELIGIOUS OBSERVANCE RELATIVE TO THAT OF MOTHER*

	N	Proportion of Total Changing (*P*)	Index of Dissimilarity (Δ)
	Born in Israel		
Years of school completed:			
1–8	26	0.500	0.308
9 or more	27	.333	.148
Total	53	0.415	0.223
	Born in Europe or America		
Years of school completed:			
1–8	15	0.200	0.200
9 or more	24	.125	.084
Total	39	0.154	0.128
	Born in Asia or Africa		
Years of school completed:			
None	15	0.333	0.267
1–8	57	.492	.263
9 or more	13	.533	.399
Total	85	.471	.288
Employment:			
Never employed	29	.310	.208
Employed prior to marriage	29	.517	.344
Employed after marriage	30	.567	.400
Total	88	0.466	0 296

* Source: Same as Table 1.

eight years of school reported some change in extent of religious observance relative to their mothers, but only a third of those with nine or more school years completed did so. For women born in Europe, one-fifth of those with one to eight years of school reported changes compared to one-eighth reporting changes among women completing nine or more years of school.

For Oriental immigrant maternity cases, the relationship between level of education and intergenerational change in religious observance is reversed and is a direct relationship. Of the maternity cases whose mothers were reported "observant," a third of those with *no* education at all, 58 per cent of those with at least some elementary education (one to eight years of school), and two-thirds of those with at least some secondary education (nine or more years of school) reported themselves in different religious-observance categories from that of their mothers. More generally, whereas a third of the women having no education reported change relative to their mothers, about half of those completing one to eight years of school and more than half of those completing nine or more years of school reported such change.[10]

Almost all of the Tel Aviv maternity cases born in Israel or in Europe had been employed at some time, and a large majority had been employed after marriage. But of the women born in Asia or Africa, only about a third had been employed after marriage and another third had never been employed at all. Table 4 also shows intergenerational change in religious observance for the Oriental immigrants in Tel Aviv–Jaffa by past employment status.

Of the maternity cases whose mothers were "observant" and who themselves were never employed, more than two-thirds reported themselves "observant" as well and none reported herself "non-observant." Of women with "observant" mothers who *had* been employed at some time, only one third remained "observant," half of the rest being "partially observant" and the other half "non-observant." More generally, the dissimilarity index is highest for daughters who had worked after marriage ($\Delta = .400$) and lowest for daughters who had never worked ($\Delta = .208$), the index for daughters who had worked prior to marriage only being close to but less than that for daughters working after marriage ($\Delta = .344$). Finally, a similar progression is evident in the percentage of each group reporting *any* change relative to mothers' categories, the never-employed group reporting change least frequently, more than half of the employed-before-marriage group reporting changes, and almost three-fifths of the employed-after-marriage group reporting intergenerational changes.

It has already been suggested (see n. 8) that using data from a sample of maternity cases tends to overstate the likelihood that daughters of "observant" mothers will remain "observant," since the sample bias favors inclusion of such women to the extent that a high degree of religious observance is associated with high fertility. For very much the same reason—but one generation removed—the distribution by extent of religious observance of *mothers of maternity cases* overestimates the proportion "observant" among the mothers, since it is actually a distribution of *daughters* by religious observance of their mothers.

Bearing these limitations in mind, it is possible to summarize the materials on intergenerational change in extent of religious observance presented thus far. Among maternity cases studied in Jerusalem and in Tel Aviv–Jaffa, about 60 per cent reported themselves in the same religious observance category as their mothers, and almost all the remaining 40 per cent reported themselves *less* re-

[10] More detailed data for maternity cases classified by extent of mother's religious observance are presented in Matras, "Israel: Absorption of Immigrants . . . ," *op. cit.* A similar finding is reported in E. Katz and A. Zloczower, "Ethnic Continuity in an Israeli Town," *Human Relations*, Vol. XIV, No. 4 (1961).

ligious than their mothers. About half of the daughters of "observant" mothers reported themselves less religious than their mothers and about a third of the daughters of "partially observant" mothers reported themselves "non-observant."

Intergenerational change in religious observance is very much more frequent among Oriental immigrants and Israeli-born women than among European immigrant women. Among the Oriental immigrant women there are evidently very marked length-of-residence effects, the veteran settlers being characterized by much more frequent change than the new immigrants. Among Israeli-born and European immigrants, post-primary education is evidently negatively associated with intergenerational change. But among Oriental immigrants those who completed some post-primary education had the highest rates of intergenerational change and those attending no school at all had the lowest rates. Finally, among Oriental women previous employment was positively associated with a high frequency of intergenerational change, and women who had never been employed outside their homes were characterized by lowest rates of change. Thus although the European-born "observant" sectors appear to be relatively stable, it seems clear that—at least in Jerusalem and Tel Aviv–Jaffa—the "observant" or religious sectors of the Israeli-born and Oriental immigrant population groups are experiencing large-scale "defection" to the "partially observant" and "non-observant" camps.

Especially the Oriental immigrant population (and the Israeli-born population of Oriental origins as well) is evidently undergoing changes in extent of religious observance on a very broad scale, and there are many questions about the meaning of these changes. One possibility is that religious observance is simply another socioeconomic attribute, negatively correlated with indexes of socioeconomic status and change in which is closely related to changes in economic and educational opportunities, as reflected in data presented here. Another possibility, of more far-reaching implications, is that the changes in religious observance represent more profound secularization trends or perhaps even the breakup and end of traditional Oriental Jewish society.

In interpreting changes among the categories "observant," "partially observant," and "non-observant," it is important to bear in mind that from the point of view of traditional religion—including Orthodox Judaism—there is no distinction between "sacred" and "secular" in social behavior; rather, society is a religious society and there are religious prescriptions for all manner of activities, behaviors, and social relationships.[11] One behaves in a given situation—say, a family, business, or community relationship—according to precepts set down in religious writings or traditions not as a matter of social custom nor as a matter of business, social, or economic strategy for maximizing benefits or gratifications, but rather as a matter of religious and moral duty.

Thus a change from "observant" to "partially observant" or "non-observant" implies a contraction of the "sacred" realm in its very *recognition* of any "secular" realm at all. The distinction between "sacred" and "secular" realms conceded, the expansion of the "secular" and complementary contraction of the "sacred" associated with changes in extent of religious observance imply an across-the-board change in the content of all social roles included in the area or sphere changing from "sacred" to "secular," the religious dimension of the role in question being dropped.

A second type of consequence of change in the extent of religious observance is the corresponding change in extent of participation in social systems explicitly "re-

[11] Cf. E. Marmorstein, "Religious Opposition to Nationalism in the Middle East," *International Affairs* (1952), abridged and reprinted in J. M. Yinger, *Religion, Society, and the Individual* (New York: Macmillan Co., 1957).

ligious," for example, synagogues, religious schools, ritual baths, festivals, etc. For the "non-observant," the entire range of "religious social systems" represents either a memory or a set of occasionally frequented places of worship, gatherings, celebrations, and ceremonies marking vital events, etc. For the "partially observant" there may be active participation in synagogues, etc., and religious or traditional social positions and status are typically accepted as legitimate. For some "observant" groups, the "secular" realm is itself new, a product of recent events, so wrapped up are these groups in the sacred and religious. For both "observant" and "partially observant" groups, religious and traditional social systems are important loci of status; *change* in extent of religious observance ordinarily implies change in relative participation in "religious" and in "secular" social systems, as well as change in the relative importance of "religiously conferred" and of "secularly conferred" status.

IV. INTERGENERATIONAL CHANGES IN PATTERNS OF FAMILY FORMATION

By far the most interesting aspect of intergenerational changes in patterns of family formation is the rationalization of family formation—the change from no intervention or control over number of spacing of births to conscious and deliberate intervention in family formation, occurring especially among the Oriental immigrant population and among traditional and religious groups in the European immigrant and Israeli-born populations. Some of the data on maternity cases gathered in Jerusalem have been examined elsewhere[12] in a preliminary attempt to describe and analyze these changes, and only the main conclusions are recapitulated here, in an attempt to relate these family formation trends to the changes in religious observance discussed earlier.

Of women in Jerusalem reporting some practice of family limitation, about three-fourths reported that their mothers had had four or more live births, and about half reported that their mothers had had seven or more live births. Just under half the mothers of the maternity cases reporting practice of contraception were "observant" and another two-fifths were "partially observant."[13] These data, combined with the presumption that neither the Oriental Jewish communities nor the religious European or Palestinean Jewish communities had been characterized by widespread practice of family limitation in the past led to the inference that the present generation of young adults (represented by the maternity cases) is characterized by rationalization of family formation. "Rationalization" has reference to the *change* from unplanned family formation to introduction of strategies and tactics in family formation.

It was possible to document the contention that the women in Jerusalem reporting practice of contraception are—in this generation—accepting and applying the very fact and legitimacy of strategic and tactical considerations in family formation, in radical departure from the previous generation's relegation of family formation to "sacred" realms not amenable to their own manipulation and control. However, *not all* the women are characterized by such a "radical departure," and a major problem is that of the nature of the sorting process: why do some make this "radical departure" while others do not?

The major variables in the process of rationalization of family formation were held to be (*a*) extent of exposure to small-family values and goals, (*b*) degree of compatibility of small-family values and goals with other values and goals, and (*c*) accessibility to means of implementing these goals.

With reference to the "exposure" variable, two points were made: there are some elements of the Jewish population

[12] Matras and Auerbach, *op. cit.*

[13] *Ibid.*, Table 2.

of Israel still essentially isolated from small-family values, or to whom the very idea of conscious intervention to control number or spacing of births is almost inconceivable. But these are comprised primarily of new immigrants from Islamic countries who typically speak and understand no Hebrew, are illiterate in any language, and are more or less cut off from the major facets of social and economic interaction in Israel; and these population elements are passing the fertile ages and represent a rapidly diminishing proportion of all new family formation.

Besides these elements permanently isolated from small-family values, there is a "family cycle effect," consisting of *relative indifference* to questions of family size during the earliest years of marriage, a pattern which is very common among young couples especially in the Oriental community and in the religious and traditional European and Israeli-born population groups. Thus there is often a period of zero-exposure to small-family values which ends after the birth of several children leads to the onset of individual family-scale Malthusian problems.

Given exposure to small-family values and goals, it does not follow that all couples accept them. In general, the "non-observant" women do all eventually practice family limitation as do the "partially observant" women born in Israel or Europe. But the "observant" women of all places of birth and the "partially observant" of Asian or African birth *may* or *may not* report family limitation practices, depending in part upon whether they accept or reject these goals and values. For those accepting the small-family values and goals, actual implementation is not always possible. In particular, Oriental couples appear to be characterized by relative absence of husband-wife communication, discussion, and consensus on the topic of family size as well as on other topics.

Women interviewed in the maternity case surveys were not asked directly about their "ideal" or "desired" number of children but rather were asked first whether or not they had ever considered the question of a "desired number" of children; those answering affirmatively were then asked, "how many?" Of all the maternity cases (in the country as a whole), no less than 60 per cent indicated that, in fact, they *had never* considered the idea of a "desired number" of children. There was sharp ethnic and socioeconomic differentiation in percentages reporting ever having considered number of children desired. Of women born in Israel or Europe, 79 per cent and 72 per cent, respectively, reported having considered the number of children desired, whereas among women born in Asia or Africa, 82 per cent said that they *had not* ever considered the number of children desired. Women born in Asia or Africa reported practice of contraception much less frequently (25 per cent) than European and Israel-born women (64 per cent and 61 per cent, respectively).

In Jerusalem and Tel Aviv–Jaffa both consideration of desired number of children and practice of contraception were reported with more frequency than in the countrywide sample, but in Table 5 it is seen that the ethnic differentiation was repeated. The same table also indicates that differentiation in family-planning behavior by extent of religious observance; in almost all cases, family planning was inversely related to extent of religious observance.

Returning momentarily to Table 1, it is readily seen that the "partially observant" and "non-observant" groups must include large proportions of daughters of "observant" mothers. Assuming that—to the extent that family limitation was at all practiced in the previous generation—the inverse relationship between family-planning behavior and extent of religious observance is likely to have obtained for the mother's generation as well, the point is again made that the current generation of maternity cases practicing contraception includes in large measure offspring of mothers *not* prac-

ticing contraception and is hence characterized by "rationalization of family formation."

It might seem reasonable to expect that among "partially observant" and "non-observant" daughters of "observant" mothers family-planning practices would be less frequently reported than among daughters of "partially observant" or "non-observant" mothers, that is, that there is a first-non-Orthodox-generation effect which would appear among women not themselves "observant" but daughters of "observant" mothers. However, this turns out *not* to be the case to any noticeable degree. The percentages reporting ever considering number of children desired and ever practicing contraception among women in each religious observance category by religious observance category of their mothers were computed, but are not shown. In any given religious-observance category, the percentages of maternity cases reporting family planning do not differ spectacularly, or even in any noticeably consistent fashion, by religious observance of the mothers. Thus "non-observant" women whose mothers were also "non-observant" do not appear to be more likely to practice contraception than are "non-observant" women who *changed* to "non-observance," that is, are daughters of "observant" or "partially observant" mothers.

TABLE 5

JEWISH MATERNITY CASES, JERUSALEM AND TEL AVIV, ISRAEL, 1959–60, BY PLACE OF BIRTH AND EXTENT OF RELIGIOUS OBSERVANCE*

	Extent of Religious Observance (Per Cent)			
	Total	Observant	Partially Observant	Non-observant
Per cent reporting having considered desired number of children:				
Jerusalem: Total	34	(5)	37	59
Born in Israel	40	(6)	53	55
Born in Europe or America	44	(9)	(42)	77
Born in Asia or Africa	26	(3)	30	(43)
Tel Aviv: Total	55	(16)	61	65
Born in Israel	61	(20)	72	69
Born in Europe or America	75	(38)	88	80
Born in Asia or Africa	35	(8)	40	55
Per cent reporting previous practice of contraception:				
Jerusalem: Total	39	(8.4)	40	72
Born in Israel	48	(8.3)	54	76
Born in Europe or America	51	(17.4)	58	77
Born in Asia or Africa	28	(2.8)	31	57
Tel Aviv: Total	66	34.1	70	82
Born in Israel	67	(30.0)	56	88
Born in Europe or America	69	(37.5)	69	86
Born in Asia or Africa	64	(34.7)	80	74

* Source: Same as Table 1. Percentages based upon fewer than 15 cases in numerator are inclosed in parentheses.

Thus, as far as extent of intervention to control number or spacing of births is concerned, there are immediate effects of intergenerational changes in the extent of religious observance, and the entire sphere of family formation is affected. More generally, the change is not simply a change in "religious" behavior, for example, adherence to ritual bath tradition, dietary laws, etc., but rather extends to

other social spheres. The percentages who ever considered desired number of children or ever practiced contraception may be viewed as indexes of the legitimacy, range, and scope of human initiative, manipulation, and control over increasing spheres of activity and increasing numbers of social systems, and these indexes are here seen to be related to extent of religious observance and to changes in the extent of religious observance.

It should be noted that important trends in rationalization of family formation take place in part independently of change in the extent of religious observance. This is evident in the percentages of "observant" and "partially observant" women reporting practice of contraception, and some of these trends have been discussed elsewhere. As far as rationalization of family formation in Israel is concerned, it is important to note that it takes place concomitantly with change in the extent of religious observance *and* in the apparently increased legitimacy—within the religious groups and without apparent changes in "extent of religious observance—of intervention in family formation at least to the extent of postponement of marriage. Thus, on the one hand the religious subsystems are themselves increasingly "permissive" and agreeable to intervention in and rationalization of certain spheres previously held strictly "sacred"; and on the other hand, there is a steady stream of "defectors" from the religious subsystems themselves. Both these trends have very far-reaching implications for virtually every facet of social, economic, and political structure and activities in Israel.

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URBAN-RURAL DIFFERENCES AS A FUNCTION OF THE DEMOGRAPHIC TRANSITION: EGYPTIAN DATA AND AN ANALYTICAL MODEL

JANET ABU-LUGHOD

ABSTRACT

Data from contemporary Egypt refute almost all the generalizations about urban-rural demographic differences that have been developed from the study of industrialized countries. However, if these deviations are viewed as functions of the temporal sequence of the demographic transition from a preindustrial to a postindustrial equilibrium, then the discrepancies revealed through cross-cultural comparisons can be reconciled and even predicted.

Within Western industrialized countries certain differences have been noted in the demographic structures of urban and rural areas. Early in the present century, when these were first being probed, the observed variations were emphasized and urban areas were considered to differ *sui generis* from rural ones. More recently, as urban culture has spread out from the metropolitan centers to encompass more and more of the rural hinterlands, and as rural values, patterns, tastes, and standards tend increasingly to approximate those set in cities, many of the differentials hitherto considered inviolate have begun to blur. The wide spread between urban and rural fertility rates commonly observed several decades ago in the United States has been narrowing precipitously, and further diminution is anticipated. Similarly, differences between urban and rural mortality rates, infant death rates, and longevity have been vanishing.

From the opposite side evidence accumulating on the characteristics of urban and rural populations in underdeveloped regions frequently contradicts the generalizations that have long prevailed in urban-rural sociology. "Expected" differences often fail to materialize and, in some cases, reverse differentials have been observed. This point is clearly illustrated by a comparison between certain generalizations elaborated through the study of industrialized countries, particularly the United States, and data available from one newly industrializing country, Egypt.[1] After identifying the discrepancies we shall consider how these disparate sets of facts might be placed within a common framework of analysis.

PROPOSITION I

Urban fertility is lower than rural fertility, and the typical urban family is smaller than the rural. *Corollary:* The larger the city the lower the fertility. (Differences are decreasing.)[2]

Egyptian evidence.—The association be-

[1] Efforts to test the generality of these propositions using the comparative statistics assembled in various issues of the United Nations *Demographic Yearbook* appear to this writer highly dubious. Countries whose conditions are most likely to contradict the generalizations have statistics most in need of careful adjustment. In contrast to many other industrializing nations, Egypt has fairly accurate data over a long period of time. Decennial censuses have been taken since 1887 and registration of births and deaths has been mandatory (though not enforceable) since the late nineteenth century. Even so, however, figures have had to be adjusted and interpreted, a procedure possible *only* when the investigator has detailed knowledge of statistical peculiarities and local conditions. For this reason we have advisedly eschewed random and gross comparative statistics.

[2] See, for example, O. D. Duncan and A. J. Reiss, Jr., *Social Characteristics of Urban and Rural Communities, 1950* (New York: John Wiley & Sons, 1956); and R. Freedman *et al., Family Planning, Sterility and Population Growth* (New York: McGraw-Hill Book Co., 1959), pp. 309–17.

tween urban life and low fertility has been observed so frequently that urbanization has seriously been suggested as a "tool" of population control. In addition, an entire theory concerning the "changing functions of the family" has been formulated to explain the decline in fertility associated with urbanization. It is therefore particularly significant that in Egypt *urban and rural fertility patterns are substantially the same.* Nor is this a recent phenomenon. Both Cleland,[3] utilizing data from 1906 to the early 1930's, and Kiser,[4] using the Census of 1937, concluded that there appeared to be no urban-rural fertility differential in Egypt. El-Badry computed the average number of children per married woman in 1947 (3.65 for urban as compared with 3.66 for rural) and concluded that there was no "excess of rural fertility over that of urban areas," even when the metropolis of Cairo was isolated.[5]

Investigation of more recent data fails to identify any basic change in this situation. For at least two generations the crude birth rate of Egypt has remained remarkably constant at about 43/1000. The recorded crude birth rate of Cairo has ranged between 50 and 55/1000 in recent decades while the rate for rural areas served by health bureaus has been in the vicinity of 45/1000.[6] While differential underregistration makes it impossible to compare urban and rural fertility directly through birth rates, Cairo, which benefits from the fullest reporting, has a rate so close to the maximum found anywhere that it is difficult to conceive of the rural rate being higher, even if there were full reporting. Nor do fertility ratios reveal significant differences between urban and rural areas. In 1952, the rural fertility ratio was 599 while urban centers had ratios ranging between 592 and 610.[7] Even more direct evidence is available from a recently completed survey of 2,000 urban, almost 700 semiurban and over 3,000 village women in Egypt, which measured fertility by "children ever born to completed families." While some differences were found between urban and rural Christian families, within the Muslim majority (over 90 per cent of the population) the difference between the number of live children ever born to rural women and the number ever born to urban women was too small to be statistically significant.[8]

Thus far, therefore, the expected relationship between fertility and place of residence has not appeared in Egypt, at least for the Muslim majority. Differences in age,

[3] W. W. Cleland, *The Population Problem of Egypt* (Lancaster, Pa.: Science Press, 1936), particularly Table IV, p. 39.

[4] Clyde Kiser, "The Demographic Position of Egypt," in *Demographic Studies of Selected Areas of Rapid Growth* (New York: Milbank Memorial Fund, 1944), pp. 97–122.

[5] M. el-Badry, "Some Aspects of Fertility in Egypt," *Milbank Memorial Fund Quarterly,* January, 1956, pp. 22–43.

[6] Vital rates in areas served by health bureaus are released quarterly by the Egyptian Ministry of Statistics and Census. See various issues of the *Quarterly Return of Births, Deaths, Infectious Diseases, Marriage and Divorce.* Health bureaus now cover all urban and many rural areas, accounting for about one-third of the total population. Underregistration in rural areas served by health bureaus is still serious. Underregistration *outside* health bureau districts is alarmingly high.

[7] Computations ours from the Quarterly Return. A ratio of children to women fifteen to forty-five years has been used since the Egyptian Census age categories do not break at twenty and modal and median age at marriage for Muslim females is below twenty.

[8] See Hanna Rizk, "Fertility Patterns in Selected Areas in Egypt" (unpublished Ph.D. dissertation, Princeton University, 1959). A summary of findings appears in her "Social and Psychological Factors Affecting Fertility in the United Arab Republic," *Marriage and Family Living,* XXV (February, 1963), 69–73. Other basic sources on Egyptian population are: M. el-Badry, "Some Demographic Measurements for Egypt Based on the Stability of Census Age Distributions," *Milbank Memorial Fund Quarterly,* July, 1955, pp. 268–305; S. H. Abdel-Aty, "Life Table Functions for Egypt Based on Model Life-Tables and Quasi-Stable Population Theory," *Milbank Memorial Fund Quarterly,* April, 1962, pp. 350–77; A. al-Dali, "The Birth Rate and Fertility Trends in Egypt," *Egypte contemporaine,* October, 1953, pp. 1–12; G. Marzouk, "Fertility in the Urban and Rural Population of Egypt," *Egypte contemporaine,* January, 1957, pp. 27–53.

duration of marriage, and religion are sufficient to account for observed variations.

In the absence of a clearly defined difference between urban and rural fertility it is not surprising that an association between fertility and city size should also be conspicuously absent. While certain industrial cities, particularly in the fast-growing Canal region, seem to have higher fertility rates than older, more diversified centers both smaller and larger, these appear to be functions of their youthful populations, a result of high migration rates, rather than of their size.[9]

PROPOSITION II

General death rates are lower in rural than urban areas, although cities have somewhat lower rates of infant mortality. (However, where finer breakdowns have been analyzed in the most recent period, the mortality advantage of rural areas has almost entirely disappeared.)

Egyptian evidence.—While fertility differences have not yet appeared in Egypt there can be little doubt that rather dramatic differences between urban and rural mortality rates have become apparent in recent years. These differences are in the opposite direction from those predicted in Proposition II. *Urban mortality rates in Egypt are now substantially below those in rural areas.* This is a totally new phenomenon dating back little more than a decade and a half.

Cities have always been notoriously unhealthy environments. When the "great killers" were still at large, the urban population, because of its greater density and its removal from subsistence agriculture, was much more vulnerable than the population on the land. Until fairly recently this was still true in Egypt. The first mortality decline of the modern period occurred during the second half of the nineteenth century when Egypt experienced an agricultural revival and a consequent expansion in the carrying capacity of the land. It was the rural areas that benefited most from this decline. Flood and famine, the major Malthusian checks in rural areas, were coming under control; their urban counterparts of epidemics and disease remained rampant.

It was not until the closing years of the century that the major causes of urban mortality were attacked. Smallpox vaccination became mandatory in 1890; the nuclei of municipal water systems were installed in Cairo and Alexandria by 1870 and gradually extended; services such as collection of refuse and night soil were commenced, and food and beverage standards were imposed. By the turn of the century urban mortality rates began to decline, reaching a level comparable to the rural rate by the 1920's. By 1937, mortality rates in urban and rural areas were still substantially the same, although the rates for the largest cities may have been lower.[10] This situation prevailed up to 1946, a date that can be established rather precisely.[11]

[9] An inherent bias introduced by Egyptian methods of estimating postcensal population bases in cities may also be responsible, since the method tends to ignore migration.

[10] See Kiser, *op. cit.*

[11] Differential underregistration of deaths makes a direct comparison between urban and rural mortality rates ill-advised. For example, Weir found in 1948 that the recorded death rate in a village containing the health bureau office was 32/1000, while two villages only a mile away had rates of 19 and 23 respectively. A third village some three miles from the office had a recorded death rate of only 12. More graphic proof of the need to adjust and correct recorded rates would be difficult to find. See J. Weir, "An Evaluation of Health Sanitation in Egyptian Villages," *Journal of the Egyptian Public Health Association,* Vol. XXVII (1952).

With this caution in mind, we have taken the liberty of using published rates merely as the starting point of our own estimates, which are as follows: During the nineteenth century the mortality rates in the largest cities ranged between 45 and 50/1000 while the average rural rate was only about 40/1000. For serial data on Cairo and national rates since 1875 see M. Clerget, *Le Caire: Étude de géographie urbaine et d'histoire économique* (Cairo: Schindler, 1934), II, 24.

During the twentieth century up to 1946, the overall death rate of the country was apparently on a plateau of 30–35/1000. See detailed estimates by M. el-Badry in "Some Demographic Measure-

Between 1945 and 1947, the Egyptian death rate dropped sharply. In the earlier year the crude death rate, corrected for underreporting, was about 33/1000. Two years later it was about 25/1000 (our corrected estimate). This decrease has persisted steadily since 1947 and we would now estimate Egypt's crude death rate at no more than 18–20/1000.[12] With this drop came a rather marked divergence between urban and rural rates, with the former now substantially below the latter.[13]

The decline in infant deaths has been even more extreme than total mortality. From a recorded infant mortality rate of 174/1000 live births in 1932, the Egyptian rate declined to 118/1000 by 1956, the major change occurring between 1945 and 1947.[14] While corrections for underreporting elevate the earlier figure to over 200, inflate the 1945 rate to perhaps 196, and require an upward adjustment of at least 25 per cent for the 1956 rate,[15] still the magnitude of the decline offers impressive testimony to the efficacy of antibiotics in controlling the devastations of infant enteritis, colitis and respiratory infections, the major causes of death in Egypt's population under one year. The urban infant mortality rate is now significantly below the rural. Despite the tendency for infant deaths (and the births preceding them) never to reach the official registers in rural sections, even the *recorded* urban rates are below the rural. In 1956–57, for example, the Cairo rate was 115/1000. Other urban areas had an uncorrected rate of 117/1000 while, in the grossly underreported rural areas served by health bureaus, the recorded infant mortality rate was 125/1000. Any adjustments in these admittedly inaccurate rates can only serve to increase the observed differential in favor of urban areas.

In summary we may conclude that accompanying the rapid decline in mortality since 1946 has been the emergence of a marked differential between the major cities and the remaining rural areas. The gap between the two has been widening and, for the immediate future, no reversal of this trend can be foreseen. Both crude death rates and infant mortality rates are substantially lower in urban centers.

PROPOSITION III

As a corollary of the first two propositions, the excess of births over deaths is

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ments . . . ," *op. cit.*, p. 303. The rural rate had ceased to decline before 1920, although the decline in the urban mortality rate continued steadily. The urban rate was probably somewhat below the rural by the 1940's.

[12] Official government statistics suggest a stable death rate between 1920 and 1945 of about 28/1000, a drop to 21/1000 by 1947, and a current rate below 15/1000.

[13] Only indirect evidence can be used to show that, at least since the "break" in the 1940's, the mortality situation in the major cities has become increasingly superior to that in rural areas. In order to use the admittedly inaccurate recorded rates certain assumptions must be made: first, that urban underregistration is less extreme than rural underregistration; and second, that while registration has become more complete in recent years, the "difference in degree" of underregistration in urban and rural areas has remained fairly constant. If these assumptions are correct, any decrease in the difference between urban and rural death rates may be interpreted as a change in their relative positions, *even if* the actual values cannot be precisely determined. In the late 1930's uncorrected rural mortality rates were consistently 8–10/1000 below the uncorrected urban rates. By 1956, this difference had almost entirely disappeared (the rural rate was only 1/1000 below the urban). If urban underregistration is between 2–3/1000 and underregistration in rural areas served by health bureaus is about 10–11/1000 (see above), a "guestimate" of 1956 true rates would be 16/1000 in the largest cities, as compared to a rural mortality rate in the vicinity of 22–24/1000.

[14] Various issues of the United Nations *Demographic Yearbook* and computations (ours) from the *Quarterly Return* . . . cited earlier.

[15] The five-year moving average for health bureau areas declined from 296/1000 live births in 1908 to 216 by 1930. See Cleland, *op. cit.*, p. 54. Kiser suggests 198/1000 for health bureau areas in 1939, but this may be too low. El-Badry, using Weir's formula to correct for underregistration, estimated an infant mortality rate between 200 and 240 for the same period. We would estimate that, by 1957, Egypt had a corrected infant mortality rate of about 150/1000 with the urban rate down to 135–40 and the rural rate above the national one.

higher in rural than urban areas, with the rate of natural increase declining precipitously with city size. Urban growth must therefore come primarily from migration.

Egyptian evidence.—As we have shown, birth rates in rural and urban Egypt are approximately equal while mortality rates are now significantly lower in the major cities. Therefore, there can be little doubt that the Egyptian urban rate of natural increase is considerably higher than the rural rate. In fact, natural increase has become the major, if still overlooked, source of population growth in the largest cities. Migration has so often attracted the attention of analysts that it is assumed to be, if not the *only* source of growth, at least the most significant one. There are now excellent reasons to suspect that since the 1940's natural increase has accounted for at least half of the growth recorded for Egypt's major cities and for possibly as much as three-fifths in the 1950's. This unprecedented phenomenon deserves careful study since it clearly deviates from all accepted urban theory.

The shift in the relative importance of migration and natural increase for Egypt's urban growth can be illustrated indirectly by comparing Cairo's rate of growth with the natural increase rate of the country.[16] Between 1800 and 1848, the population of Egypt increased by about 35 per cent while Cairo grew by only 20 per cent. Since urban and rural birth rates were roughly comparable and there was no out-migration from the capital, Cairo's lower rate of growth must be attributed to deficient natural increase, that is, higher mortality. Between 1848 and 1897—the opening phase of modern development—Egypt's population increased by 130 per cent while the capital city grew by only 90 per cent. Since this was a period of healthy migration to the capital, the figures suggest an even greater discrepancy between urban and rural rates of natural increase, brought about by a decline in rural mortality rates in the face of stable urban rates.

During the course of the present century this situation was gradually but totally reversed. The installation of a drainage-sewerage system in Cairo during World War I was translated almost immediately into a 5/1000 decrease in the crude death rate, making natural increase rates in the capital and the country comparable for the first time in history. Between the two world wars Egypt's demographic situation seemed to have become stabilized. With birth rates slightly lowered due to an increase in the age at marriage and with death rates apparently on a plateau, the country's rate of increase slowed to little more than 1 per cent per year. During this depressed period, natural increase in Cairo possibly outstripped that of the country but the difference was not substantial. It was not until World War II set in motion the drastic drop in mortality—a drop most precipitous in the largest cities—that the current situation came about in which natural increase in Cairo now amounts to 32/1000/year in contrast to 23–25/1000 in rural areas. For the first time, capital-city growth is coming primarily from an excess of births over deaths.

While this is an unprecedented phenomenon, one cannot believe that Egypt is simply a unique case. A similar change may now be occurring in other overpopulated, underindustrialized countries, although better data than are currently available would be necessary to substantiate this. We would hypothesize that the largest Indian cities, for example, now have higher natural increase rates than prevail in their rural hinterlands, despite arguments to the con-

[16] Because Egypt has peculiarly low rates of in- and out-migration, it is possible to take total population growth as equivalent to natural increase. In the analysis that follows the raw data, the criticisms of them, and the corrections and extrapolations that underlie the conclusions have had to be omitted. Statistical substantiation is available from the author and will appear in a forthcoming volume on the evolution and organization of the city of Cairo.

trary.[17] If this should prove to be the case, current theories must be revised to take into account the new element that has been introduced in industrializing countries by the postwar tumbling of mortality rates.

PROPOSITION IV

Urban and rural areas differ significantly in the age compositions of their populations. The percentage of population in the

TABLE 1

AVERAGE PERCENTAGE OF YOUNG AND MIDDLE-AGED PERSONS IN EGYPTIAN COMMUNITIES OF DIFFERENT SIZES, 1947*

Size Class	Percentage of Total Population (Class Average)	
	Under 15 Years	15-50 Years
Entire country	39.3	45.0
Cities 500,000 plus	37.1	52.1
100,000-499,999	39.2	50.1
50,000-99,999	39.5	49.3
30,000-49,999	40.2	48.8
20,000-29,999	39.5	48.7

* Computations based on separate returns of the 1947 Census for the fifty-seven urban places having 20,000 or more inhabitants.

"productive years" is highest in the largest cities and declines smoothly with size of place; rural areas, on the other hand, contain concentrations of the very young and the very old. Differential fertility and mortality as well as selective migration create this situation.

Egyptian evidence.—A similar but much less marked gradient is found in Egypt for which migration alone appears to be responsible. Table 1 illustrates this. The magnitude of the observed differences is relatively small, and within the size classes the exceptions are so numerous that one must conclude that the differential is far less developed than in the United States. Only the very largest cities deviate significantly, a fact that must be attributed to the Egyptian pattern of migration which tends to bypass smaller cities and towns and be directed almost exclusively toward the largest cities.[18]

PROPOSITION V

Urban and rural populations are characterized by different sex ratios. Females predominate in cities whereas males outnumber females in rural farm and non-farm areas.

Egyptian evidence.—So accustomed have we become to the female excess in American cities that it shocks us to learn that in rapidly developing countries the reverse is much more common. In Egypt it is the cities that have positive sex ratios while the rural areas have ratios of under 100. This is clearly illustrated in Table 2.

TABLE 2

SEX RATIOS BY COMMUNITY SIZE, EGYPT, 1947*

Size of Place	Males per 100 Females
Country total	98
Cities over 100,000	103
Cities 50,000-99,999	101
Cities 20,000-49,999	97
Villages 10,000-19,999	99
Villages 5,000-9,999	97
Places under 5,000	95

* Computations ours from data presented in United Nations *Demographic Yearbook 1955*.

Differences in sex ratios must be traced always to selective migration. Table 3 demonstrates the close relationship between

[17] In a recent book on urbanization in India several very reliable demographers concluded, on the basis of census data up to 1951, that it was highly unlikely that urban growth in India could ever come from an excess of births over deaths. See Kingsley Davis, "Urbanization in India," particularly pp. 5-6; and D. Bogue and K. Zachariah, "Urbanization and Migration in India," particularly pp. 27-28, in Roy Turner (ed.), *India's Urban Future* (Berkeley: University of California Press, 1962). The next census may provide an unexpected turn of events.

[18] This phenomenon has been identified and substantiated in J. Abu-Lughod, "Urbanization in Egypt: Present State and Future Prospects," forthcoming.

Egyptian patterns of migration and the resultant sex ratios in sending and recipient communities.[19] Note that within the youngest age class—that least likely to be imbalanced by selective migration—males and females are quite evenly matched. Rural-to-urban migration, concentrated after fifteen years of age, produces a major dislocation noticeable particularly in the middle years of life, with the sex ratios highest in the large, fast-growing communities.

TABLE 3

AVERAGE SEX RATIOS FOR EGYPTIAN URBAN COMMUNITIES, BY SIZE, CLASS, AND REGIONAL LOCATION, 1947*

SIZE AND TYPE OF COMMUNITY	RATIO OF MALES PER 100 FEMALES IN AGES			
	No.	Under 15	15-49	50 Plus
Receiving:				
500,000 plus	2	98.0	105.5	106.6
100,000-499,999	5	98.6	110.6	105.8
50,000-99,999	9	98.0	101.0	104.0
Sending:				
30,000-49,999	15	100.0	97.8	105.0
20,000-29,999	26	99.8	95.4	98.8
Total	57			
All fast-growing	10	100.3	111.8	106.1
Slow-growing in:				
Upper Egypt	24	100.0	91.7	108.0
Lower Egypt	23	98.9	96.6	90.5

* Computations ours from special data processed for the fifty-seven Egyptian communities with populations of 20,000 or more in 1947.

As can be seen, fast-growing cities, regardless of location, have the highest ratios. Among the remaining cities, however, regional location is of critical importance, reflecting the cultural differences between Upper and Lower Egyptian migration patterns. When people from the Delta (Lower Egypt) migrate to the cities, they often move as a nuclear family; men migrating alone rarely return to their community of origin once having gone to the city. The reverse is true for the typical Upper Egyptian migrant. First, migration is almost exclusively a male matter, and second, migrants frequently return to their families in the village afer decades of only intermittent contact. While these differences have been identified elsewhere, the sex ratio differentials revealed in Table 3 give added credence to them.[20]

Thus, with respect to sex ratios the urban-rural differential is found to be as extreme in Egypt as it is in the United States but in the opposite direction. This is due to differences in selective migration which in turn are related to the types of employment opportunities available in the cities

[19] The age categories of under 15, 15–49 and 50-plus have been selected because of their sensitivity in Egypt. The majority of Egyptian migrants move by the age of twenty; and only 10 per cent of the Egyptian population is fifty or more years of age.

[20] See J. Abu-Lughod, "Migrant Adjustment to City Life: The Egyptian Case," *American Journal of Sociology*, July, 1961, particularly p. 27. Subsequent publication of the preliminary results of the 1960 Census permits an analysis of post-1947 changes. Postwar migration from Upper Egypt differed from earlier movements in that the sexes were quite evenly balanced. The 1947 sex ratio of Upper Egyptians in Cairo was 400; this has been reduced to 129, as computed from the 1960 Census, representing an important departure from a pattern of very long duration.

of each and to the cultural determinants of labor-force participation.[21]

PROPOSITION VI

To summarize the rather complex data for the United States, there is a moderate relationship between size of place and the marital status of the population, if age composition is standardized. Larger cities contain greater proportions of single, widowed, and divorced adults. The reverse is true for smaller towns and rural non-farm areas, while farm regions contain more single men and more married women than a standard population.[22]

Egyptian evidence.—The inconclusive data for Egypt indicate that both the married and the divorced states are more common in the larger communities while single status is more frequent in rural communities, although the lack of age standardization throws doubt on some of these conclusions. In 1947, 39 per cent of Cairo's male and an equal per cent of her female population (total) was married, as compared to the national rate of 38 per cent for each sex. The annual marriage rate shows a slightly greater discrepancy. For example, in 1957, the *Quarterly Return* recorded 17.7 marriages per thousand Muslims within the entire country, while the comparable rate in Cairo was 20.2 marriages. Judgment on these points must be reserved, however, since differences in age distribution, age at marriage, and incidence of remarriage make it impossible to interpret these figures in a meaningful way.[23]

While the variations in nuptiality are so slight that they might be dissipated entirely by a more refined analysis, this is hardly true with respect to the incidence of divorce. According to the 1947 Census, some 1.7 per cent of the males sixteen years of age and older and fully 3.1 per cent of the adult females in Cairo were listed as divorced, as compared to 0.9 and 1.3 per cent, respectively, in the country as a whole. Within recent years the annual divorce rate for Cairo Muslims[24] has fluctuated between 9–12/1000, according to various issues of the *Quarterly Return*, although the comparable rate for the entire country has varied between 3.5–6.5/1000 each year. Thus the Cairo rate has been consistently two to three times higher than the national rate, indicating a significant differential.

We may tentatively suggest that, with the exception of specific industrial centers that recruit an unattached male labor force, urban areas and rural areas in Egypt are roughly comparable in the percentage married. On the other hand, urban life does appear to affect the frequency of divorce. Given the legal ease with which a Muslim union may be dissolved, social and familial sanctions play the major role in minimizing divorce within the closed village society; when these become less effective (although hardly inoperative) in the city and as urban heterogeneity encourages more exogamous matches, divorce rates increase. Cairo's higher rate is due more to these factors than to any selective principles of migration.

[21] The low rate of participation of Egyptian women in the *urban* labor force is evident from the following figures computed from the unpublished 1957–58 sample Labor Force Survey of Egypt. Only 3 per cent of all females five years of age and older were employed in "urban-type" occupations, i.e., in manufacturing, commerce, government, services, etc. By contrast, almost half of the male active labor force was engaged in "urban-type" occupations.

[22] See Duncan and Reiss, *op. cit.*, Table 15, p. 71.

[23] The higher nuptial rate of Cairo appears to be due more to age composition than other factors. In addition, remarriage is more common in Cairo than elsewhere in the country, which would serve to inflate the nuptial rate. On the other hand, the median age at marriage is somewhat higher in Cairo than elsewhere (21.6 as contrasted with under 20 for Muslim females); but since these figures include women entering second and third unions as well as first, they are unreliable.

[24] Muslim figures are used exclusively in this analysis since the major Christian sect in Egypt prohibits divorce except under very unusual and unlikely circumstances.

THE EGYPTIAN DEVIATIONS—ACCIDENTAL OR SYSTEMATIC?

When six commonly accepted propositions on the demographic differences between urban and rural areas are tested in a country at an entirely different stage of the demographic-industrial revolution, we find that on almost every point the Egyptian data deviate more or less from the "expected." The deviations are not capricious but bear a systematic relation to each other and to the historic and cultural context of their appearance.

Furthermore, Egypt's experience cannot be dismissed merely as a unique or exceptional case. Many of the Egyptian "deviations" are shared by other countries at similar stages of modernization and may even have characterized Western countries several centuries ago. While it remains outside the scope of this article to provide proof for these statements, one can point to suggestive evidence. A recent Indian study indicates an absence of the urban-rural fertility differential in that country.[25] Since many newly modernizing countries experienced the same drastic decline in mortality after World War II[26] that Egypt did and for the same reasons, it is not unlikely that they are experiencing also a divergence between their urban and rural mortality rates by this time. Studies have appeared that indicate the presence in many Asian countries of the same age and sex differences as those identified in urban and rural Egypt.[27] In short, there is congruence to the deviations.

[25] Edward Driver, in *Differential Fertility in Central India* (Princeton, N.J.: Princeton University Press, 1963), found no significant differences between urban and rural fertility patterns in the sample studied.

[26] See George Stolnitz, "The Revolution in Death Control in Non-Industrial Countries," in *Annals of the American Academy*, CCCXVI (March, 1958), 94–101.

[27] See, for example, the findings summarized in UNESCO, *Urbanization in Asia and the Far East*, ed. P. Hauser (Calcutta: Research Centre on the Social Implications of Industrialization in Southern Asia, 1957), pp. 123–25.

A PROPOSED MODEL FOR ANALYSIS

The theory of the demographic transition offers a useful conceptual framework within which both the generalizations derived from industrialized countries and those applicable to modernizing countries may find their place.[28] On the basis of that theory and the expanding body of knowledge relating to the dynamics of demographic change, it is possible to construct a hypothetical model in which specific urban-rural differentials (or their absence) can be predicted at various stages of the transition.

The model presented below is consciously derived from the Egyptian case, although it has been modified by data from other areas and has wider application and, hopefully, more general value. No model can hope to be equally relevant to all cases. Exceptions and necessary modifications will be discussed in a final section.

Assumptions.—Beginning with the simplest proposition, one may state: The demographic transition from a preindustrial equilibrium (a function of high birth, crude death, and infant mortality rates leading to a relatively stable, young, and small population) to a postindustrial equilibrium (a function of relatively low birth rates, very low infant mortality rates, and moderate mortality rates leading to a relatively stable, older, and larger population) takes place over time (X) *in a given and predictable temporal sequence* that affects certain variables before others and certain social and geographic subgroups of the population before others. Therefore, at any particular point in time, *observed differences in the characteristics of urban and rural populations can be explained with reference to the temporal sequence.*

[28] In a field in which theory seems to be converging very rapidly, no claim to originality can be justified. The basic framework provided by P. Sorokin, Amos Hawley, and particularly Frank Notestein has shaped much of the author's thinking. A parallel approach which appeared after this article was framed is found in D. Cowgill, "Transition Theory as a General Population Theory," *Social Forces*, March, 1963, pp. 270–74.

A second proposition must be appended if the remaining variations observed in individual case studies are to be accounted for, namely:

Both (*a*) the *technologies of birth and death control* extant at the historic epoch during which a country's transition is effected and (*b*) the *cultural-social system of the society* undergoing the transition will modify the rates at which specific changes take place and may even, in exceptional cases, lead to minor revisions in the temporal sequence itself. *The temporal sequence of the transition* may be summarized in terms of several non-discrete phases.

PREINDUSTRIAL OR FEUDAL AGRARIAN

1. The economy is based upon subsistence agriculture or, if under colonial administration, an exported agricultural surplus, and the total population is therefore "relatively" small.

2. Crude birth rates are high and fluctuate little from year to year except when unusual circumstances force postponements of marriage or when economic conditions permit a lowering of the average age at marriage. While there may be a relationship between economic status and size of family (due to survivorship), a gross urban-fertility differential is extremely unlikely.

3. Crude death rates are high and are furthermore subject to violent periodic fluctuations in which the small natural increase gains of many years may be totally wiped out. Because of unsanitary conditions, cities will usually have much higher mortality rates than rural areas.

4. Natural increase will be higher in rural than urban areas, and urban growth will depend heavily upon continual replenishment through migration. Selective migration will lead to marked age and sex differences.

5. Only a small percentage of the population can reside in cities[29] which serve chiefly as the locus of concentrations of religious and military power, as central places for a very circumscribed hinterland, and/or as "break-in-bulk" points for wider trade. The urban population consists of a very small elite, a somewhat larger and fairly stable artisan-merchant class, and a massive "floating" population, unskilled and of rural origin, which is culturally undifferentiated from the peasant class except in terms of often temporary place of residence. The rural population is organized along feudal lines.

6. There is little dominance of the city over the hinterlands except in tax and conscription matters. Cultural values of the urban elite—including those affecting fertility and mortality—are transmitted neither to the urban proletariat nor to the peasants.

EARLY TRANSITION

1. The first variable affected when a society begins the demographic transition (which may or may not coincide with economic reorganization) is mortality. Death rates decline but at a rate that has varied significantly during different epochs of "death control technology." At least three major periods must be distinguished:

Prior to about 1850.—Very gradual decline in death rates, more pronounced in rural than urban areas. In countries beginning the transition during this epoch—for example, England and Wales—the differential between urban and rural mortality rates inherited from the preindustrial phase not only persisted but became even wider.

1850–1946.—Moderately rapid decreases in mortality were achieved chiefly through advances in epidemic control, public health regulations, and environmental sanitation. Both urban and rural mortality rates declined, the latter often earlier but more gradually than the former. The differential inherited from the preceding period usually grew no wider and may actually have de-

[29] See G. Sjoberg, *The Preindustrial City* (Glencoe, Ill.: Free Press, 1960); also E. Lampard, "The History of Cities in the Economically Advanced Areas," *Economic Development and Cultural Change*, III (January, 1955), 81–136.

creased. A distinction must be made between indigenous development and imposition of death-control technology. France during the nineteenth century illustrates the first development; Egypt from 1890 provides an example of the second.

After 1946.—Mortality rates may fall quite rapidly in response to widespread use of DDT, antibiotics, etc. The decline will probably be even faster in urban than rural areas. Depending upon the preexisting gap between urban and rural rates and upon the extent of urban-to-rural diffusion, the mortality differential may disappear entirely or even be reversed so that the lowest death rates may be found in the largest cities[30] Most so-called underdeveloped countries are entering the transition stage under conditions of this type.

2. Fertility rates remain constant or may actually increase somewhat if prosperity encourages a lowering of the marriage age.[31] The lack of a noticeable urban-rural fertility differential persists.

3. The "population explosion" experienced at this stage results from the progressively larger gap between stable fertility and declining mortality. Whether this explosion will be more marked in urban or rural areas is totally dependent upon the direction and degree of the mortality differential.

4. The pressures created by the population increase can be relieved through (*a*) emigration, (*b*) premature rural-to-urban migration, or (*c*) land expansion through conquest of reclamation of marginal territory, or some combination of all three. Europe depended primarily on the first; Egypt, Japan, Korea,[32] etc. have depended more upon the second; while Mexico and many South American countries have found the third alternative practicable. The chances that urbanization and industrializa- will develop apace are greater where the first and third alternatives are available; where only the second is feasible, the time lag between premature urbanization and commensurate industrial development will be greater.

5. Since much of the pressure is relieved through migration from rural-to-urban areas, rather marked differences between the age and sex compositions of urban and rural areas should appear at this time. Selective migration on a large scale modifies the age and sex structures of both sending and receiving areas, the net effect being a function of (*a*) the degree and consistency of the selection and (*b*) the ratio of migrants to base population. The *specific* differentials that will be found, however, depend upon the cultural patterns of a country and no single model will suffice for predictive purposes. Most countries will develop uniform age differentials since in all countries youth tends to be more mobile than age. With respect to sex ratios, on the other hand, culture will be determinative. Attitudes toward and opportunities for employment of women outside the home are the key variables affecting urban-rural sex ratios.

INDUSTRIALIZATION AND THE TRANSITION PROPER

Once the economy has passed the "taking-off" point and the processes of industrialization have become firmly and irreversibly established, hitherto inert variables begin to change.

1. A decline in fertility appears but is not distributed at random in society. It begins within certain socioeconomic and cultural groups and diffuses according to a fairly predictable sequence, a phenomenon that facilitates prediction of the specific urban-rural differentials that reach maxi-

[30] This is because modern techniques of death control are more readily available in urban areas and acceptable to urbanites, while folk techniques persist more vigorously in rural areas.

[31] This is suggested by William Petersen in "The Demographic Transition in the Netherlands," *American Sociological Review*, June, 1960, pp. 334–47. Confirmation can be found in the Egyptian data.

[32] See, for example, T. O. Wilkinson, "The Pattern of Korean Growth," *Rural Sociology*, XIX (March, 1954), 32–38.

mum visibility during this phase of the transition. As was necessary in the case of mortality, one must distinguish several epochs in the technology of birth control:

Prior to circa 1875.—During the eighteenth and much of the nineteenth century fertility could be only imperfectly controlled by inadequate techniques. The very gradual rates at which fertility declined in Western countries undergoing the demographic transition at that time must be attributed in part to inefficiency rather than lack of motivation.

From 1875 to the present.—During this epoch fertility was rendered increasingly subject to human control through the development of improved, though scarcely perfected, methods of contraception. This reduced the time interval required by an industrializing society (for example, Japan) to make the transition from high to low fertility, indicating that faulty techniques rather than values had constituted the major deterrent during the earlier period.

After 1964.—A third epoch in the history of fertility control, one that may revolutionize birth rates as dramatically as the advances of the 1940's revolutionized death rates throughout the world, appears to be imminent. A technological breakthrough in the provision of a simple, non-repetitive, inexpensive, and highly effective method of birth control will make it possible for societies now entering the second stage of the demographic transition to transform their fertility rates within a considerably briefer period of time than has ever before been recorded. Again, although motivation must be present, this writer believes that the latent demand for effective birth control is so great that all our current projections will prove to have been overly pessimistic, once this third epoch of fertility control is entered.[33]

2. The rural-urban differences that appear during this stage of the transition, however, result more from the sequence of the fertility decline than from its speed, although the more rapidly over-all fertility declines, the briefer the period during which these differentials will be apparent. The following model of sequence is composite and cultural differences will lead to minor variations.

a) Fertility declines *first* within the newly emerging professional and managerial class that is "created" by industrialization and concentrated in the cities.[34] This class, characterized by upward mobility and possession of skills rather than land, has most of the factors that, in numerous studies, have been linked to family-planning motivation and birth-control effectiveness. It has incorporated most completely the values of "rationalism" and ends-means into its *Weltanschauung;* it tends to experience the greatest discrepancy between its level of expectation and its income; it tends to delay marriage until later in life; and members tend to select wives from higher educational levels. A lower fertility is therefore to be expected.

b) Only after industrialization has begun to undermine the traditional bases of prestige characteristic of the preindustrial phase (such as land, religious role, even "foreignness") will the older aristocracy begin to take its cues from the new upper-middle class. Only then and quite gradually will this group experience a downward trend in fertility.

c) Emulation will lead members of the

[33] The high latent demand for birth control in Egypt's urban areas is confirmed by a recent study conducted by the Alexandria Institute for Social Work, as reported in *Al-Ahram* (Cairo), August 11, 1963, p. 4. Within a relatively poor area of Alexandria, 96 per cent of the wives and 84.5 per cent of the husbands interviewed favored family limitation. The ideal size of family was placed overwhelmingly at three children, which appears to be a developing universal ideology found equally in such places as Puerto Rico and Indianapolis!

[34] El-Badry, analyzing differential reproduction in Egypt in 1947 by occupation of father, found that only one occupational class—comprised of engineers, doctors, officers, and technicians—had fewer children. No other occupational differences were statistically significant. See his "Some Aspects of Fertility in Egypt," *loc. cit.*, pp. 35–39.

urban lower-middle class to adjust their fertility patterns next. This group, through its white-collar employment in the government and corporate bureaucracies, is maximally exposed to the new professional-managerial elite with whom it can identify more readily than with the older aristocracy. This makes it likely that they will incorporate the new values, although not without perhaps a lengthy and painful conflict.

d) In descending order of exposure and capacity to "identify" are the urban artisan class, the semirural or migrant city dweller, and finally, the peasant. Declines in group fertility can be anticipated in this order. *Note that the sequence begins with urban subgroups and terminates with rural groups*. Therefore, urban-rural fertility differentials will appear to develop along a U-shaped curve, minor at the point when the fertility transition begins, maximal toward the middle, and negligible at the terminal phases of the transition.

3. The simultaneous urbanization of the country exaggerates the urban-rural differentials created by the sequence of fertility decline. Urbanization serves to increase membership in classes likely to be experiencing a fertility decline while it reduces membership in those classes least affected by the new fertility patterns. At this point significant changes are occurring in (*a*) the role of cities in the national economy; (*b*) the percentage of the population engaged in non-agricultural pursuits; and (*c*) the relationship between cities and their hinterlands. These changes have important implications for urban-rural differentials.

a) During the period of "incipient" industrialization, particularly in densely populated regions, the urban population may expand to include one-fifth or more of the total without any alteration in the role of cities in the economy. Urban growth results more from pressures expelling people from rural areas than from urban opportunities attracting them. Once industrialization becomes established, however, urban growth attains a momentum of its own with a basis in economic viability. If cities can offer sufficient industrial employment opportunities, social mobility and cultural assimilation of migrants will be encouraged, which will lead to a more rapid decline in over-all urban fertility rates.

b) This expansion in the industrial sector, accompanied by the gradual substitution of rational farming methods for the rural horticulture formerly practiced, leads to a *shift* away from farming in the long run, even if this is temporarily disguised by "underemployment" in densely populated countries such as India and Egypt. The net result is an increase in the percentage of the labor force engaged in urban-type occupations and thus an increase in the number of persons "exposed" to conditions and values favoring lowered fertility.

c) The third change that occurs affects the relationship between cities and their hinterlands. One major morphological difference between Western industrial cities and their counterparts in pre- and semi-industrial areas is that, whereas the former are surrounded by a wide zone of transitional uses, the latter tends to terminate abruptly with almost no density gradient, as if they were surrounded by invisible walls. This physical fact is matched by the less obvious sociological fact that the city's influence ends almost as sharply as its physical plant.[35] This makes it unlikely that urban-nurtured values and patterns will have any impact on rural persons. With industrialization and more particularly with the proliferation of the transport and communication networks prerequisite to industrial growth comes a radical expansion in the city's sphere of influence that is manifested physically in the development of a transitional suburban ring and sociologically in the increased capacity of the city to affect economic

[35] One of the few studies of this phenomenon is Richard Ellefson, "City-Hinterland Relationships in India," in Roy Turner (ed.), *op. cit.*, pp. 94–115.

conditions, aspirations, and ways of life in an ever widening hinterland.

It is significant that the first two changes in the nature of the city tend to increase the demographic contrast between urban and rural areas while the last tends, in the long run, to destroy it. In the history of Western urbanization the first two operated initially and only at a much later stage did the third factor enter to mitigate their effects. A major difference between the Western model and one appropriate for societies currently undergoing the transition, then, is that in the latter all three processes are occurring simultaneously, *which may prevent the emergence of as extreme an urban-rural contrast as developed in the nineteenth-century West.*

4. Since urbanites still constitute a minority of the population, the over-all birth rate of a country will appear to be declining only very gradually during the first stages of the fertility transformation, although the urban-rural fertility differential will be progressively widening. Once urban patterns diffuse to rural areas the over-all crude birth rate will drop rapidly, but by the time this occurs, the demographic differences between urban and rural populations will have already begun the process of coalescence that terminates within the succeeding (and for the present, terminal) stage of the demographic transition.

5. Differential mortality plays only a minor role during this phase. Its impact is chiefly upon the changing age structure, a phenomenon that becomes critical during the next stage.

POSTINDUSTRIAL SOCIETY: A NEW TAKING-OFF POINT?

The basic characteristic of a postindustrial society is that urbanization and industrialization have become so pervasive that their presence or absence within any given geographic subarea is culturally "irrelevant." Just as preindustrial cities absorbed their dominant ethos from agrarianism, so postindustrial rural enclaves derive theirs from urbanism. In neither instance can simple criteria such as size of community, density of settlement, etc., serve as reliable indices to values or "ways of life." And just as preindustrial societies maintained a relatively stable demographic balance *in the absence of major urban-rural differences,* the postindustrial society appears to develop its own equilibrium despite urban-rural uniformities. This period is characterized by:

1. Relatively stable birth rates comparable in so-called urban and so-called rural areas, with significant intraregional variations due primarily to ecological specialization within the metropolitan complex. From the very limited experience to date it appears that fertility rates may dip dangerously low before stabilizing at a more moderate level.

2. Relatively stable death rates equivalent in urban and rural areas with regional and intraregional differences due almost exclusively to age, sex, and income differentials. Again from limited experience, it appears that the death rate reaches an abnormal low before the effects of an aging population appear and then rises slightly to a fairly stable level.

3. Moderate population growth rates are dependent upon economic conditions that encourage or inhibit planned fertility. Natural increase, once at the mercy of mortality fluctuations in the preindustrial era, becomes almost solely a function of fertility.

4. Continuance of selective migration not to compensate for urban deficits but to allow for a constantly increasing degree of conurbation. Rapidly growing regions will have younger populations and ostensibly higher birth and lower death rates; declining regions will have older populations, lower birth and higher mortality rates, but these variations will not necessarily be related to the degree of urbanism per se.

5. Relation of existing mortality and fertility differentials more to socioeconomic status than to place of residence. That

mortality decreases with higher socio-economic status has already become apparent; evidence is now accumulating to suggest a future positive association between income and fertility during the post-industrial period. Sweden has recorded such an association for some time, and data from the United States appear to be moving in the same direction.

APPLICATION OF THE MODEL AND SOME LIMITATIONS

Application of the above model to the Egyptian case is relatively simple since the model has, in part, been derived from the data. Briefly, one can summarize it as follows: prior to 1850, Egypt's demographic condition was preindustrial. Toward the end of the nineteenth century she entered the initial stage of the transition, as evidenced by the decline in rural mortality which was translated immediately into rapid population growth more marked in rural than urban areas. The industrialization stimulated during World War II seems to have propelled Egypt into the opening phases of the established transition, since fertility in the limited class of urban professionals and technicians now shows a significant deviation from the high fertility norm. It appears that this norm has already lost ideological support and one can therefore anticipate that an urban-rural fertility differential—conspicuously absent up to the present—will shortly appear in Egypt. As Egypt advanced along the demographic transition, certain urban-rural differentials were found to increase while others decreased or were reversed. Thus the "deviations" of Egypt from the patterns found in industrialized countries and from patterns that previously prevailed within Egypt itself can be accounted for within the framework of the demographic transition.

To what extent can this model be used to analyze the urban-rural differences found in other countries and regions? Only slight modifications would appear to be required to adjust the model to other relatively overpopulated and newly industrializing countries of Southeast Asia and the Middle East. "Horizontal" comparisons, that is, comparisons of observed urban-rural differences within these countries *at the same phase* of the demographic transition, would permit us to develop and refine the generalizations hypothesized in the model and, further, would permit us to trace the *residual* variations due to technology and culture.

The model in its present form would not be applicable either to the African case or, perhaps, to certain parts of Latin America.[36] Since the transition in Africa is basically from tribal to urban organization without a significant intervening period of settled agrarianism, a separate model must be developed. Basic modifications would also be required for the countries of Central and South America, since the ethnic cleavage between urban and non-urban subgroups is so deep and prolonged that observed urban-rural differences there may merely reflect separarate cultural strains.

SMITH COLLEGE

[36] Several studies of Mexican fertility seem to refute predictions that would follow from our model. The South American situation is well documented in Kingsley Davis and Ana Casis, "Urbanization in Latin America," *Milbank Memorial Fund Quarterly*, XXIV (April, 1946), pp. 186–207.

RESIDENTIAL DISTRIBUTION AND STRATIFICATION, 1950–1960[1]

EUGENE S. UYEKI

ABSTRACT

A replication in space and extension in time of the Duncans' analysis of the relationship between occupational stratification and residential distribution in Chicago, 1950, was undertaken by using Cleveland data for 1950 and 1960. The rank-order correlations between Chicago and Cleveland for 1950 for indexes of segregation, dissimilarity, centralization, and low-rent concentration range from .88 through .97. The ρ's for Cleveland for 1950 and 1960 for the indexes range from .93 for low-rent concentration through .95 for the others. Not only are the patterns similar, but the magnitudes of the index values are also quite similar for both intercity and intracity comparisons.

INTRODUCTION

This article reports on a replication in space and extension in time of the Duncans' study of residential distribution and occupational stratification,[2] which was undertaken for Chicago for 1950. The results reported below are for what is essentially the Cleveland Standard Metropolitan Area (SMA) for 1950 and 1960.[3] The Duncans have demonstrated the close relationship between social distance and spatial distance for one large American metropolis. Wilkins has shown this relationship, though differing at certain points, for eight medium-sized American cities.[4] Cleveland, although about one-fourth the size of Chicago, shows many similarities with the latter on several characteristics which are considered to have some effect on residential distribution of the population in urban areas. A confirmation in Cleveland of the findings for Chicago will support the applicability of this general approach to comparative urban social patterns of large American cities.

Cleveland and Chicago are both cities fronting on the Great Lakes. Each is built up on flat land with a river merging with the lake near the city center. Both began their period of rapid growth during the 1850's with decreasing growth around the turn of the century and actual population decline in the central city in the last decade. The industrial breakdowns for the two cities are similar: both have diverse

[1] I would like to thank the Case Research Fund for a grant which made part of this analysis possible and D. Atkins, L. Saban, and C. Applebaum for their statistical assistance. Acknowledgment is also made for the assistance of the staff and the use of the facilities of the Andrew R. Jennings Computing Center.

[2] Otis Dudley Duncan and Beverly Duncan, "Residential Distribution and Occupational Stratification," *American Journal of Sociology,* LX (March, 1955), 493–503. For other applications of the same method see Otis Dudley Duncan and Stanley Lieberson, "Ethnic Segregation and Assimilation," *American Journal of Sociology,* LXIV (January, 1955), 364–74; Arthur H. Wilkins, "The Residential Distribution of Occupation Groups in Eight Middle-sized Cities of the United States in 1950" (unpublished Ph.D. dissertation, University of Chicago, 1956); and Otis Dudley Duncan and Beverly Duncan, *The Negro Population of Chicago: A Study of Residential Succession* (Chicago: University of Chicago Press, 1957). Also see Arnold S. Feldman and Charles Tilly, "The Interaction of Social and Physical Space," *American Sociological Review,* XXV (December, 1960), 877–84.

[3] The 1950 census tracts are all those listed for Cleveland and Adjacent Area, 1950. This tracted area encompasses 96.7 per cent of the population in the Cleveland SMA for 1950.

[4] The study by Wilkins, *op. cit.,* e.g., shows a greater regularity in the predicted directions for dissimilarity of occupational distribution for an average of eight middle-sized American cities (Hartford, Syracuse, Columbus, Indianapolis, Richmond, Atlanta, Memphis, and Fort Worth) in 1950. The patterns of dissimilarity for Cleveland and Chicago, as will be noted below, are more similar to each other than to the pattern for the eight medium-sized cities. This is suggestive of differences in residential patterns of the various occupation groups between medium-sized (340,000–675,000) and large (1,500,000) cities.

manufacturing, including heavy industry such as primary metals, professional services, and public administration. Both cities have large areas of older housing and have urban renewal projects finished or under way (Table 1).

In 1950 Cleveland and Chicago had quite similar proportions of their populations in the various categories of racial and ethnic status (Table 2).[5] Moreover, as may be seen in Table 1, in 1950 the occupational composition of the male work force in the two metropolitan centers was virtually identical; only 3.6 per cent of the Chicago work force would have to shift major occupation group for the Chicago and Cleveland occupational distributions to correspond perfectly.

Finally, the socioeconomic status of the several occupation groups, as indexed by the proportion non-white and median income, is much the same in the two cities. Laborers are the only group in which the percentage of non-white workers differs by as much as 1 percentage point; the proportion non-white is 3.7 points higher in Cleveland than in Chicago. Sales workers

TABLE 1

SELECTED CHARACTERISTICS OF MALE EXPERIENCED CIVILIAN LABOR FORCE BY MAJOR OCCUPATION GROUPS IN CLEVELAND METROPOLITAN AREA, 1950 AND 1960, AND CHICAGO METROPOLITAN AREA, 1950

Major Occupation Group	Cleveland 1950			Difference, Chicago 1950 minus Cleveland 1950			Change, Cleveland 1960 minus Cleveland 1950		
	Per Cent Distribution	Per Cent Non-White	Median Income	Per Cent Distribution	Per Cent Non-White	Median Income	Per Cent Distribution	Per Cent Non-White	Median Income
All	100.00	9.0	$3,281	0.0	0.3	$ 43	0.0	2.2	$2,290
Professional, technical, kindred workers	9.6	2.0	4,363	−0.2	0.7	24	2.7	1.5	2,999
Managers, officials, and proprietors, except farm	11.1	2.4	4,712	0.4	−0.2	119	−0.6	−0.2	3,538
Sales workers	7.4	2.3	3,395	−0.3	0.5	303	0.8	0.7	2,430
Clerical and kindred workers	8.9	6.4	3,148	1.2	1.0	− 16	0.0	4.1	2,055
Craftsmen, foremen, kindred workers	24.0	5.0	3,514	−1.3	−0.1	134	−0.8	1.8	2,516
Operatives and kindred workers	24.3	11.5	3,101	−1.9	0.9	14	−0.1	4.8	2,118
Service workers, except private household	6.4	22.4	2,486	1.8	0.6	149	−0.5	3.2	1,500
Laborers, except farm and mine	8.3	31.1	2,484	0.2	−3.7	96	−1.5	3.2	1,462

Source: *1950 Census of Population, Characteristics of the Population*, State Tables 35 and 78; *1960 Census of Population, General Characteristics*, State Table 74, and *Detailed Characteristics*, Table 124.

TABLE 2

RACIAL AND ETHNIC STATUS OF POPULATIONS OF CLEVELAND AND CHICAGO

	Per Cent	
	Cleveland	Chicago
White	89.5	89.0
Negro	10.4	10.7
Other races	0.1	0.3
Foreign-born white	13.0	12.8

[5] U.S. Bureau of the Census, *U.S. Census of the Population: 1950, Vol. II, Characteristics of the Population*, Part 35, *Ohio*, and Part 33, *Illinois* (Washington, D.C.: Government Printing Office, 1952), Table 34.

are the only group for which median income differs by as much as $150; the median income for Chicago sales workers was $303 greater than for Cleveland sales workers. There is one inversion in rank order; craftsmen-foremen in Cleveland had a higher median income than did sales workers whereas the reverse is true for Chicago.

Given these striking similarities, then, in Cleveland we should expect to find not only a fairly close relationship between social distance and spatial distance, but also a pattern the details of which resemble closely those of the Chicago pattern. Otherwise, some factor other than topography, growth history, racial-ethnic and occupational composition, industrial breakdown, and relative status of socioeconomic groups must have a powerful influence on socioeconomic differentiation.

COMPARISON OF CLEVELAND AND CHICAGO IN 1950

The computational procedures for the various indexes describing the spatial distribution of occupational groups have been described in Duncan and Duncan and therefore are not detailed here.[6] The nature of the indexes will be briefly indicated. The index of dissimilarity measures the difference in areal distribution between two occupation groups. The index of segregation measures the difference in areal distribution between one occupation group and all the remaining occupation groups. The index of low-rent concentration measures the extent to which one occupation group compared with the remaining occupation groups is concentrated in areas with a low median monthly rent of tenant-occupied dwelling units. The index of centralization measures the extent to which one occupation group compared with all the remaining occupation groups is concentrated in areas located near the center of the city.

There are some differences between this study and the Duncans' which relate primarily to size and number of zone sectors. The Chicago study had five sectors and zones radiating out and varying in size from 1-mile intervals for the first 14 miles to 2-mile intervals after that, for a total of 104 zone-sector segments. The Cleveland analysis is based on five sectors also but the zones radiate out from the central business district at intervals of approximately 2¾ miles for the first four zones and the last zone includes all of the remaining tracted areas for the 1950 Census. There are twenty-one zone-sector segments for Cleveland.[7]

The index of residential segregation for each occupation group relative to all other groups appears in Table 3. The rank-order correlation coefficients for the indexes for Cleveland and Chicago for 1950 are as follows: (1) by zone-sector segment, .88; and (2) by census tracts, .96. Both are significant at the 0.01 level or greater.[8] The difference in index value is no more than 3 points by tract and no more than 5 points by zone-sector segment. The only index which is out of line is that for service workers for Cleveland, which is less than that for operatives—its immediately preceding category. Except for this, however, the Cleveland data show the U-shaped distribution hypothesized by the Duncans, that is, with the upper and lower parts of the occupational categories being more segregated than those in the middle ranges.

The index of residential dissimilarity between each occupation group and every

[6] See Duncan and Duncan, "Residential Distribution and Occupational Stratification," *op. cit.*, and Otis Dudley Duncan, "Urbanization and Retail Specialization," *Social Forces*, XXX (March, 1952), 267–71.

[7] Because of low population densities, the west and southwest sectors have four zones and the southern sector has three. In each case, all of the remaining census tracts were added to the last zone in the sector.

[8] For a one-tailed test, a ρ of .83 or larger is significant at the 0.01 level and a ρ of .64 or larger is significant at the 0.05 level for an N of 8.

other occupation group appears in Table 4.[9] The rank-order correlation coefficients between the twenty-eight index values for Cleveland and Chicago, 1950, are .97 based on census tracts and .93 by zone-sector segments.[10] A closer analysis of the spatial patterns between the two cities is possible by rank-ordering the indexes for each of the major occupation groups in its comparison with all the other groups.[11] Of the possible fifty-six comparisons between the cities (ignoring a difference of 0.5 which indicates a tied rank), there are fifteen comparisons which are out of line, with only six involving differences of more than one rank. All six occur in comparisons involving a "blue-collar" group: craftsmen, operatives, or laborers. The pattern for the clerical group is marred by a single inversion of one rank; and the rank-ordering of dissimilarity for the top three white-collar groups is duplicated perfectly between the two cities. Moreover, the numerical differences between the Cleveland and Chicago indexes of dissimilarity for pairs of occupations are small. For twenty-four of

TABLE 3

INDEX OF RESIDENTIAL SEGREGATION OF EACH MAJOR OCCUPATION GROUP FOR EMPLOYED MALES: CLEVELAND 1950 AND 1960, AND CHICAGO 1950

	Cleveland 1950		Difference, Chicago 1950 minus Cleveland 1950		Change, Cleveland 1960 minus Cleveland 1950 (by Zone-Sector Segments)
	By Census Tracts	By Zone-Sector Segments	By Census Tracts	By Zone-Sector Segments	
Professional, technical, kindred workers	33	22	−3	−1	−1
Managers, officials, and proprietors, except farm	31	21	−2	−1	2
Sales workers	27	18	2	2	2
Clerical and kindred workers	12	8	1	1	−1
Craftsmen, foremen, and kindred workers	18	9	1	5	1
Operatives and kindred workers	23	19	−1	−3	−1
Service workers, except private household	21	16	3	4	−1
Laborers, except farm and mine	33	25	2	4	6

Source: Chicago data from Duncan and Duncan, "Residential Distribution and Occupational Stratification," *op. cit.*, Table 2.

[9] The indexes of segregation and dissimilarity for Cleveland in 1950 were computed by census tracts and also by zone sector segments. The numerical values may be seen in Tables 3, 4, and 6. Like the Chicago indexes, the values are larger by census tracts than by zone sectors, though the ordering remains the same.

The product-moment correlation coefficient between the two sets of indexes of segregation for Cleveland in 1950 is .93 and the regression equation is $s = .74\,t - 1.1$, where s stands for zone-sector segment and t for census tract. The coefficient for the two sets of dissimilarity indexes is .95 and the regression equation is $s = .79\,t - 2.26$. The comparable Chicago coefficients relating the indexes computed by zone-sector segments and by census tracts are .96 for the segregation indexes and .98 for the dissimilarity indexes. The difference of .03 in the coefficients of the two cities is probably a result of the grosser grid utilized in the Cleveland analysis, which averaged 17.5 census tracts to each zone-sector segment compared with 10 for Chicago. These results tend to confirm the Duncans' conclusion that for these purposes using a larger area may be an efficient way of ordering data.

[10] For a one-tail test, a ρ of .45 or larger is significant at the 0.01 level and a ρ of .32 or larger is significant at the 0.05 level for an N of 28.

[11] E.g., in Cleveland in 1950 the professional group is most similar in residential pattern to the managerial and sales group, so its comparison with each is assigned rank 1.5; it is least similar to laborers, so the professional-laborer comparison is assigned rank 7.0. In Chicago in 1950 the professional-managerial comparison is ranked 1.0, the professional-sales comparison 2.0, and the professional-laborer comparison 7.0.

these twenty-eight comparisons, the difference is less than 3 percentage points. In three cases, the difference is in the 5–7 point range; and in the intercity comparison for craftsmen versus service workers, a difference of 11 points is observed. The lesser dissimilarity in residential distribution between craftsmen and service workers in Cleveland may reflect the fact that only one-eighth of the male service workers in Cleveland are janitors and sextons as compared with one-fifth in Chicago.

The ρ for the relation between the indexes of centralization for Cleveland and Chicago in 1950 is .90 (Table 5). As in the Chicago distribution, the clerical group is out of line with the other white-collar groups and the craftsmen-foremen group is the most decentralized of the blue-collar occupational categories.. The largest discrepancy between the two cities is the greater centralization of laborers in Cleveland. Cleveland does not lack industrial suburbs which ought to be a decentralizing

TABLE 4

INDEXES OF DISSIMILARITY IN RESIDENTIAL DISTRIBUTION AMONG MAJOR OCCUPATION GROUPS, FOR EMPLOYED MALES, BASED ON CENSUS-TRACT GRID, CLEVELAND AND CHICAGO, 1950*

MAJOR OCCUPATION GROUP	MAJOR OCCUPATION GROUP							
	A	B	C	D	E	F	G	H
A. Professional, technical, kindred workers	......	0	2	− 1	− 2	− 2	2	2
B. Managers, officials, and proprietors, except farm	13	......	0	− 7	− 2	− 2	1	2
C. Sales workers	13	13	......	3	3	2	1	5
D. Clerical and kindred workers	29	35	24	......	3	0	1	3
E. Craftsmen, foremen, kindred workers	37	35	32	13	......	0	11	2
F. Operatives, kindred workers	46	43	40	21	17	......	5	1
G. Service workers, except private household	39	39	37	23	24	21	......	3
H. Laborers, except farm and mine	52	50	49	35	33	24	25	......

* Above the diagonal: difference, Chicago 1950 minus Cleveland 1950; below the diagonal: Cleveland 1950.

Source: Chicago data are from Duncan and Duncan, "Residential Distribution and Occupational Stratification," *op. cit.*, Table 3

TABLE 5

INDEXES OF CENTRALIZATION AND OF LOW-RENT CONCENTRATION OF EACH MAJOR OCCUPATION GROUP FOR EMPLOYED MALES, CLEVELAND 1950 AND 1960, AND CHICAGO 1950

MAJOR OCCUPATION GROUP	CENTRALIZATION			LOW-RENT CONCENTRATION*		
	Cleveland 1950	Chicago 1950 minus Cleveland 1950	Cleveland 1960 minus Cleveland 1950	Cleveland 1950	Chicago 1950 minus Cleveland 1950	Cleveland 1960 minus Cleveland 1950
Professional, technical, kindred workers	−22	8	−2	−74	42	41
Managers, officials, and proprietors, except farm	−21	9	−4	−53	23	13
Sales workers	−15	10	−4	−25	0	− 8
Clerical and kindred workers	1	4	2	− 2	− 7	3
Craftsmen, foremen, and kindred workers	− 2	− 6	−1	7	4	− 1
Operatives and kindred workers	14	− 4	6	20	9	6
Service workers, except private household	17	4	−2	8	− 1	5
Laborers, except farm and mine	25	−18	8	14	18	18

* The Chicago index for low-rent concentration includes employed females.

Source: Chicago data are from Duncan and Duncan, "Residential Distribution and Occupational Stratification," *op. cit.*, Table

attraction as in Chicago. Whatever the reason for the greater centralization of laborers, the Cleveland data show a better fit with the Burgess hypothesis of upward gradients in socioeconomic status as one moves out from the center of the city to its periphery.

On the index of low-rent concentration, the ρ for the Cleveland-Chicago comparison is .95. (The Cleveland indexes are for "employed males," whereas the Chicago indexes are based on "total employed persons.") The magnitude changes are in the same direction for both the Cleveland and Chicago indexes, showing the sharp break between the white-collar and the blue-collar occupational categories. However, in Chicago the professional and managerial groups are less concentrated in higher rental areas and the laborers are more concentrated in low rental areas than the same groups in Cleveland.

TEMPORAL STABILITY IN RESIDENTIAL PATTERNS

A remarkable similarity in residential patterns for two metropolitan centers differing in size but similar in growth history, industry distribution, and socioeconomic characteristics of their populations has been shown at one point in time. The question of the persistence of residential patterns over time is answered by comparing the Cleveland materials for 1950 and 1960. To make the comparison rigorous, the same spatial area has been used in 1960 as in 1950.[12] The data summarized below point to the fact that in a number of respects the Cleveland of 1960 differs from the Cleveland of 1950 more than Chicago and Cleveland differed in 1950.

There have been some population disruptions resulting from urban renewal and highway building in Cleveland during this time. Most of this building has been concentrated on the eastern side of Cleveland. As a matter of fact, the two urban renewal projects—one public and the other private—are both concentrated largely within the southeastern sector. Highway construction has been largely adjacent to the central business district. Over-all, the greatest disruption is probably associated with lower-income families.[13]

There have been some changes in population composition in the Cleveland area during the decennial period. There is a noticeable decrease in the white population (from 89.5 per cent in 1950 to 85.4 per cent in 1960), a decrease in foreign-born whites (from 13.0 per cent in 1950 to 9.7 per cent in 1960), and an increase in Negroes (from 10.4 per cent in 1950 to 14.3 per cent). As may be seen in Table 1, the change in occupational composition is relatively small; 3.5 per cent would have to shift [their occupation] within the 1950 occupational distribution to produce the 1960 occupational distribution. The largest difference is for professional workers who increased by 2.7 per cent.

There are several shifts worth noting in the status of the several occupation groups in Cleveland over the decade. The changes in the proportion non-white over the range "operative to craftsmen to clerical" should be noted. The percentage point increases were 4.8 for operatives, 1.8 for craftsmen, and 4.1 for clerical. As indexed by proportion non-whites, it would appear that the "status" of clerical relative to craftsmen slipped during the decades. The statistics of income point to the same fact. In terms of 1950 income, the clerical workers were the lowest of the white-collar group and below the top manual-worker group of craftsmen; by 1960, they had fallen below the operatives.

The ρ for the degree of residential segregation by zone-sector segments during the two time periods is .95, which suggests a high degree of stability. As may be seen

[12] There were 206 census tracts in Cleveland and 143 in suburban areas in 1950. As the same tracts were used for the 1960 analysis, 20 census tracts in the 1960 SMSA were not used.

[13] Seymour Z. Mann and Charlotte B. Smart, *Land Use and Planning in the Cleveland Metropolitan Area* (Cleveland: Cleveland Metropolitan Services Commission, 1959).

in Table 3, the largest difference is the greater degree of segregation of laborers in 1960. No other index change is larger than 2 in value.

The ρ for the index of dissimilarity by zone-sector segments for 1950 and 1960 is .95. Perhaps the most interesting change shown in Table 6 is the greater dissimilarity of the laborers to the top three white-collar groups and also to the craftsmen. There are fourteen inversions in rank order between the two time periods. However, there are only three where the rank-order difference is more than 1; two of these involve clerical workers and the other craftsmen.

For 1950 and 1960, the ρ for the index of centralization is .95. The greatest change is the greater centralization of the laborers. In addition, managerial workers have become more decentralized than professional workers, and clerical workers more centralized.

For 1960, the index of centralization was computed by the five sectors radiating out from Public Square into the metropolitan area. The pattern for the total area is reproduced in three of the five sectors—namely, the west, south, and southeast. The southwest sector still shows the professional workers as the most decentralized, and the eastern sector shows a greater centralization of service workers than the other sectors. A possible explanation of the situation in the southwest sector is the continuity of the earlier pattern of greatest decentralization of professional workers. For the outlying areas in the southwest sector are not the high prestige suburban areas, and it is probable that managerial workers have not moved there in such proportions as elsewhere. A partial explanation for the high centralization of service workers in the eastern sector (as well as the southeast) is the inclusion of Negro areas in the central city within these sectors since the second greatest percentage of Negro workers is to be found in this major occupation group.

TABLE 6

INDEXES OF DISSIMILARITY IN RESIDENTIAL DISTRIBUTION AMONG MAJOR OCCUPATION GROUPS, FOR EMPLOYED MALES, BASED ON ZONE-SECTOR SEGMENT GRID, CLEVELAND, 1950 AND 1960*

Major Occupation Group	Major Occupation Group							
	A	B	C	D	E	F	G	H
A. Professional, technical, kindred workers	......	2	0	1	− 3	− 1	− 2	8
B. Managers, officials, and proprietors, except farm	5	......	− 1	4	2	1	1	9
C. Sales workers	5	7	......	3	3	0	1	8
D. Clerical and kindred workers	19	19	16	......	1	− 4	− 3	3
E. Craftsmen, foremen, kindred workers	22	21	18	9	......	2	0	9
F. Operatives, kindred workers	33	33	31	18	15	......	− 3	− 4
G. Service workers, except private household	32	32	29	18	19	14	......	1
H. Laborers, except farm and mine	38	39	37	27	24	21	16	......

* Above the diagonal: change, Cleveland 1960 minus Cleveland 1950; below the diagonal: Cleveland 1950.

The ρ for the index of low-rent concentration for 1950 and 1960 is .93. Managerial workers are concentrated most highly in the higher rental areas, surpassing professional workers, and the latter are equaled by sales workers. Laborers have become more concentrated in low-rent areas during the decennial period.

CONCLUSIONS

One finding of this study is the great similarity in residential distribution of the major occupation groups for Cleveland and

Chicago in 1950. There are some minor inversions in rank order among the indexes. A majority of the inversions involve comparisons of craftsmen with another occupation. Interestingly enough, the sole inversion in the ranking of occupational groups by income also involves the craftsmen group. In sum, not only are the patterns and the differences in the two cities similar, but the magnitude of the intergroup differences are also similar.

The other important finding demonstrates the great stability of residential patterns of the major occupation groups in Cleveland from 1950 to 1960. We cannot account in any systematic way for the small changes that have occurred. With respect to nearly every index of residential distribution, laborers have become more differentiated from the other groups; in both absolute and relative terms, the increase in income for laborers over the decade was less than for any other group.

These results demonstrate that the details of socioeconomic residential differentiation in Chicago are almost perfectly reproduced in Cleveland, a smaller city, but one whose growth history, industrial structure, and socioeconomic stratification are similar; and that there is strong persistence in the pattern of socioeconomic differentiation within a city over the span of a decade. Finally, the results suggest that changes in residential pattern tend to occur for groups whose relative socioeconomic status is changing.

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SOME EFFECTS OF CLOSE AND PUNITIVE STYLES OF SUPERVISION[1]

ROBERT C. DAY AND ROBERT L. HAMBLIN

ABSTRACT

An experiment based on a two-by-two factorial design was conducted to test hypotheses involving the relationship of four supervisory styles to aggressive feelings and actions of subordinates. The supervisory styles were arrayed on two continuums: the close versus general and the punitive versus non-punitive. Close as compared with general supervision produced significant increases in aggressive feelings toward the supervisor and in indirect aggression toward the supervisor through lowered productivity, an *insignificant* increase in verbal aggression toward the supervisor, and an increase of borderline significance in aggressive feelings toward co-workers. Punitive as compared with non-punitive supervision produced significant increases in indirect aggression through lowered productivity and in verbal aggression, but no significant increases in aggression toward co-workers. The relationship between close supervision and aggressive feelings appears to be mediated by the self-esteem of the subordinate; an increase in aggressive feelings occurred only in subjects having low self-esteem. Finally, the combined effect of the close and punitive supervision dimensions, for both aggressive feelings and indirect aggression, was not a simple function, but was less than would be predicted on the basis of additive assumptions.

INTRODUCTION

Close supervision was originally isolated and studied as a style or dimension of supervision by teams of researchers connected with the Survey Research Center at the University of Michigan and led by Daniel Katz.[2] In an early study of female workers in a large metropolitan insurance firm, Morse reported that workers subjected to a close supervision style were less satisfied with the supervisor's ability to handle people, less satisfied with the reasonableness of her expectations, and generally less satisfied with the rules she enforced.[3] These findings suggest a specific hypothesis that aggressive feelings are instigated by close supervision. Katz and Kahn reported finding this relationship between close supervision and aggressive feelings of workers in a tractor plant.[4] Furthermore, if lowered productivity is taken as a form of retaliatory aggression toward the supervisor, data reported earlier by Katz and his associates[5] are also consistent with this hypothesis. Gouldner produced evidence, from an illuminating study of a gypsum factory, that further supports the close supervision–aggression hypothesis.[6]

In the present study, close supervision

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[2] D. Katz and R. L. Kahn, "Some Recent Findings in Human-Relations Research in Industry," in G. E. Swanson, T. M. Newcomb, and E. L. Hartley (eds.), *Readings in Social Psychology* (2d ed.; New York: Henry Holt & Co., 1952), pp. 650–65; D. Katz and R. L. Kahn, "Leadership Practices in Relation to Productivity and Morale," in D. Cartwright and A. Zander (eds.), *Group Dynamics* (2d ed.; Evanston, Ill.: Row, Peterson Co., 1960), pp. 554–70; D. Katz, N. Maccoby, G. Gurin, and Lucretia G. Floor, *Productivity, Supervision and Morale among Railroad Workers* (Ann Arbor: Survey Research Center, University of Michigan, 1951); and D. Katz, N. Maccoby, and Nancy C. Morse, *Productivity, Supervision and Morale in an Office Situation,* Part 1 (Ann Arbor: Survey Research Center, University of Michigan, 1950).

[3] Nancy C. Morse, *Satisfactions in the White Collar Job* (Ann Arbor: Survey Research Center, University of Michigan, 1953).

[4] Katz and Kahn, "Leadership Practices . . . ," *op. cit.*

[5] Katz and Kahn, "Some Recent Findings . . . ," *op. cit.*

[6] A. W. Gouldner, *Patterns of Industrial Bureaucracy* (Glencoe, Ill.: Free Press, 1954).

is conceptualized as one end of a continuum that describes the degree to which a supervisor specifies the roles of the subordinates and checks up to see that they comply with the specifications. However, there are two other points of this continuum worth noting. The opposite extreme to close supervision might appropriately be termed "anomic supervision," as it would involve no specifications (that is, no expectations or norms) and no checkups. Somewhere in the middle area of this theoretical continuum, the general style of supervision can be postulated; it involves a moderate number of specifications and checkups, at least enough to let the workers know what they are supposed to do. Thus, in close supervision the attempt is to structure completely the workers' behavior, and in general supervision, to structure it only to the point where the worker does not feel at a loss as to what to do; in anomic supervision no attempt is made at all to structure the behavior. Although it would have been interesting, anomic supervision was not included in the present investigation because of the limits of time.

To account for the relationship between close supervision and aggressive feelings or actual aggression, a softened version of the frustration-aggression hypothesis can be used,[7] since close supervision apparently is frustrating to the subordinate. To the extent that it is frustrating, then, the subordinate should be instigated to aggress against the supervisor as the agent of frustration and, in some cases, perhaps actually to translate his impulses into direct aggression, such as angry words, or indirect aggression, such as a conscious retaliatory slowdown in productivity.

However, an important point to grasp here is that close supervision is not in itself aggression. For to be aggression, a manifest intention in applying it would necessarily be to hurt or injure the subordinate. But as Gouldner has suggested, the manifest intention involved in using the close style of supervision is probably to increase productivity.[8] The supervisor may not even be aware that his close supervision is frustrating, thus producing psychological pain or injury. In terms of intention or awareness, the close style of supervision may be contrasted with a second style, "punitive" supervision, which involves the intentional, conscious use of aggression to gain the compliance of subordinates.

To the extent that a supervisor enforces work specifications or rules by aggressing against those subordinates who depart from or violate the rules, he is using a punitive style of supervision. When the punitive supervisor uses aggression (most often in the form of angry, ego-lacerating reprimands), he is attempting to reinforce the avoidance of behavior that violates work rules. Thus, he is usually aware that his aggression is painful to the subordinate, and is in effect saying to him, "I know this hurts, but it is your own doing. If you want to avoid it in the future, follow my rules." Because it is so painful, he is probably aware that his aggression instigates subordinates to counteraggress, but because of his authority to hire, fire, promote, or demote, he evidently also assumes ultimate victory in any aggressive exchange. Furthermore, since the workers are aware of his superior power, he counts upon their not wanting to start an aggressive exchange by counteraggressing.

Yet, unresolved tensions have a way of being channeled into more subtle forms of indirect aggression that are not easily detected or eliminated, as, for example, when workers channel their aggressive impulses into conscious, retaliatory slowdowns in production. If artfully practiced, this form of aggression can hurt the supervisor badly while making it most difficult for him to fix blame or take active corrective

[7] N. E. Miller, "I. The Frustration-Aggression Hypothesis," *Psychological Review*, XLVIII (1941), 337–42.

[8] *Op. cit.*

measures. This evidently happened with railroad section gangs studied by Katz and his associates, who found that foremen of low-producing gangs tended to use a punitive style of supervision.[9]

Thus far, we have postulated that aggression is the over-all result of both the close and the punitive styles of supervision because both styles, whether intentionally or not, are painful to subordinates. Specifically, we have mentioned two forms of aggression: angry words and a conscious, retaliatory slowdown in production. But these may not be the only manifestations of aggression that result from the frustrations inherent in the close and punitive styles of supervision. Negative emotions often become displaced, and consequently could magnify out of proportion the aggression that sometimes accompanies routine conflicts among workers. Furthermore, these emotions might also be displaced to magnify any incipient dissatisfaction with the work situation itself. Thus, in hypothesizing that both the close and the punitive styles of supervision result in increased aggression, we are actually predicting that they both result in (1) an increase in the amount of verbal aggression toward the supervisor, (2) a decrease in productivity, (3) an increase in verbal aggression toward co-workers, and (4) an increase in dissatisfaction with the work situation. However, in making our predictions we should note that Pepitone and Reichling found that relationships based on displacement are usually weaker than the others.[10]

Although the close supervision-aggression hypothesis has been generally supported in a number of investigations, evidently the strength of the relationship is quite variable. Again, if lowered productivity is taken as an indication of indirect aggression, the data reported by Katz and his associates in 1950 show that a strong relationship exists where the close versus general styles are used by second-line supervisors in their relations with section heads, but only a moderate relationship in relations between section heads and their subordinates.[11] Data from a second study in 1951 by Katz and associates indicate no relationship at all between close versus general supervision and worker aggression.[12] These variations seem to indicate that a third variable mediates the relationship between close supervision and aggression. In this instance, the mediator is probably a characteristic of the subordinate that influences the amount of frustration he experiences when he is subjected to close and perhaps punitive supervision. The rationale for suggesting self-esteem as the mediating variable is best understood in the context of Goffman's dramaturgical theory of social behavior.[13]

Using the theoretical metaphor extensively, Goffman views the behavior of persons in social contexts as a sequence of carefully guided performances serving to create a "front" or an impression. In attempting to create and maintain a satisfactory self-image, the individual tries to define the situation in such a way that he is able to guide and control the impressions that others obtain of him in the situation. The individual's concern, then, is to put his act over successfully, to maintain by various techniques a favorable, creditable self-image. Thus, in a bureaucratic situation the workers may be viewed as striving to project a self-image to the supervisor and to co-workers, "and the characteristic issue, the crucial concern, is whether it [the self-image] will be credited or discredited."[14]

[9] Katz, Maccoby, Gurin, and Floor, *op. cit.*

[10] A. Pepitone and G. Reichling, "Group Cohesiveness and the Expression of Hostility," *Human Relations*, VIII (1955), 327–37.

[11] Katz, Maccoby, and Morse, *op. cit.*

[12] Katz, Maccoby, Gurin, and Floor, *op. cit.*

[13] We wish to express our appreciation to Alvin W. Gouldner for suggesting Goffman's theory as the conceptual context in which to discuss self-esteem as a mediating variable (E. Goffman, *The Presentation of Self in Everyday Life* [Garden City, N.Y.: Doubleday & Co., 1959]).

[14] *Ibid.*, p. 253.

From the assumption that the workers are attempting to project a creditable self-image, it follows that the close and punitive styles of supervision would be frustrating for two reasons. First, the styles imply a lack of competence, a lack of skill on the part of the worker. Second, they imply a lack of motivation on his part to do the right thing or, in fact, a kind of malicious motivation to do the wrong thing. Thus, when a subordinate is subjected to either of these two styles of supervision, his self-image may be discredited severely. Furthermore, he may be able to do very little to change the situation so that he can create a more favorable impression.

However, and this is the critical assumption, not all subordinates may be equally concerned with maintaining the front, with presenting a creditable self-image. Specifically, our assumption is that some individuals have such strong, favorable self-images, such high *self-esteem,* that they are relatively *unconcerned* with impression management, whereas other individuals have such ambiguous, ambivalent self-images, such low *self-esteem,* that they are *highly concerned* with impression management, with maintaining a front and thus projecting a creditable self-image. If so, the amount of frustration an individual experiences when subjected to close or punitive supervision should vary inversely with his self-esteem. Furthermore, since the strength of the postulated relationship between either style of supervision and aggression should be a function of the amount of frustration experienced,[15] the strength of the relationship should also be a function of the self-esteem of subordinates. Perhaps a simpler statement is: The association between the two styles of supervision and the various aggression variables should be relatively weak among subordinates who have high self-esteem, but relatively strong among those with low self-esteem.

[15] Miller, *op. cit.*

METHOD

THE EXPERIMENTAL GROUPS

Twenty-four groups, each consisting of four women recruited from undergraduate classes and dormitories at Washington University, were used in the experiment. Controls were applied for age (17–19 years) and years of schooling (Freshmen and Sophomores).

THE EXPERIMENTAL SITUATION

At an appointed time each group arrived at the laboratory and was ushered into an experimental room designed to stimulate reasonably well an industrial work station. Here the subjects were given a pre-experimental questionnaire and then task instructions. After these were completed, each group worked at the task for a period of 40 minutes, and then completed a post-experimental questionnaire. The 40-minute experimental session included 10-minute periods with a supervisor (a trained member of the experimental staff) in the room and two intervening 5-minute periods during which the supervisor left the room for the expressed purpose of evaluating the workers' production. Her absence, however, was designed to give the subjects a chance to be alone and thereby some freedom to express any aggressive feelings toward the supervisor or the experiment. Her exits and entrances were timed precisely with a stopwatch that she held in her hand and that, in addition to accurate timing, provided a note of precision and authority. As in a factory setting, an impersonal buzzer was used to signal the beginning and end of the work period.

The task consisted of assembling models of molecules using pegs, springs, and various colored balls provided in Sargent Kits, which are often used in university chemistry classes. Drawings of elaborate, complicated molecular structures were provided as "blueprints" for molecule construction. These models seemed to be novel and complex enough to interest and involve

the subjects for the required 40-minute work period. The fact that the task was complex and naturally suited for assembly-line procedures contributed to making this a natural situation where various styles of supervision could be used. (In any experiment it is important that the manipulation not be external to the situation and thus relatively obvious to the subjects, who usually are interested in guessing "what they're after." The general assumption is, of course, that subjects cannot systematically fake behavioral effects unless the goals of the experimenters are obvious.) In addition to providing a natural environment for the manipulations, the kits for molecule construction afforded a rather simple but reliable quantitative measure of productivity.[16]

To simulate an industrial setting, the member of the experimental staff who took the role of supervisor was introduced simply with, "This is Miss Bradshaw, your supervisor during the work period." To heighten the impersonality of the situation, subjects were not introduced to the supervisor, but were addressed by numbers conspicuously displayed at each of their work stations. In addition, words such as "supervisor," "worker," "blueprint," "material bin," "work efficiency," "production unit," "subassemblies," and "production line" were used to convey the atmosphere of an industrial situation. However, in order to promote interaction, the situation was designed to be different from the usual production line in one important way: The subjects were stationed around an oval table that permitted each subject to view all co-subjects during the work period.

[16] Several considerations went into the decision to set up a production line. First, it was desirable to standardize and keep constant the over-all sequence of operations for all groups. To this end, subjects were given definite subtasks to perform; that is, the groups did not determine their own division or non-division of labor. Second, it was undesirable either for the final combinations of operations to be so inefficient or slow that tension would be generated by the task itself, or for each worker to be allowed to proceed to construct whole models by herself in relative isolation, with no interdependence with the others. Although the latter procedure would have maximized productivity in the time allotted to the task, intersubject conflict would have been virtually absent even in high-tension situations. Consequently, although the sequence of operations chosen was efficient enough to keep frustrations at a minimum, it included enough interdependence among subjects to make some conflict inevitable.

THE EXPERIMENTAL DESIGN

Technically, the experiment involved a two-by-two factorial design with high-low manipulations of the two independent variables, that is, the close and punitive styles of supervision. Using a table of random numbers, six of the twenty-four groups were assigned to each of the four "cells," as shown in Figure 1.

	Close Supervision	General Supervision
High Punitive Style	6 Groups	6 Groups
Low Punitive Style	6 Groups	6 Groups

FIG. 1

To operationalize the four styles of supervision, two lists of remarks were drawn up for use by the supervisor. To operationalize closeness of supervision, a set of clear, concise instructions (role definitions) was developed. In the general supervision situations, the eight most essential of these instructions were used by the supervisor to give a minimum definition of the situation. In the close supervision situations, forty instructions were used; also, certain amounts of obvious hovering and watching as well as repetitions of previous instructions were used as checkup techniques.

To operationalize the punitive style in both the close and general situations, a list of sarcastic, negative, status-deflating remarks was developed for the supervisor to use as punitive sanctions. In the high-punitive situation, she made forty such remarks; in the low-punitive situation, she made none at all.

Two members of the staff who observed verbal aggression by the subjects also counted the supervisor's punitive remarks. The experimenter kept a count of the instructions and checkups, and a system of lights informed the supervisor when she had given the required number of remarks for each situation.

Fourteen practice sessions were required to standardize and internalize the multiple facets of the supervisor's role.[17] All extraneous remarks had to be identified and inhibited, and important non-verbal gestures (facial and body) had to be standardized. She had to learn to recognize and "control" subjects who were skilled at becoming dependent on the supervisor through asking for support. To help the supervisor control and minimize support-giving, observers registered all supportive remarks by the supervisor during this training period. Finally, she had to practice giving instructions that were devoid of aggressive connotations.

MEASUREMENT

Self-esteem.—The measure of self-esteem used in this experiment was developed by de Charms and Rosenbaum[18] and was based in part on an earlier measure by Janis.[19] The subjects were instructed to choose an answer ranging from "strongly agree" through "strongly disagree" that best characterized their usual reactions. (In listing these and other items in this section, the numbering and order are appropriate to this presentation and consequently depart from the format used in the questionnaire.)

1. I feel capable of handling myself in most social situations.
2. I seldom fear my actions will cause others to have a low opinion of me.
3. It doesn't bother me to have to enter a room where other people have already gathered and are talking.
4. In group discussions I usually feel that my opinions are inferior.
5. I don't make a very favorable first impression on people.
6. When confronted by a group of strangers, my first reaction is always one of shyness and inferiority.
7. It is extremely uncomfortable to accidentally go to a formal party in street clothes.
8. I don't spend much time worrying about what people think of me.
9. When in a group, I very rarely express an opinion for fear of being thought ridiculous.
10. I am never at a loss for words when I am introduced to someone.

Agreement with items 1, 2, 3, 8, and 10 and disagreement with items 4, 5, 6, 7, and 9 probably indicate high self-esteem or self-confidence and little concern with the presentation of self, that is, with the management of the image presented to others. On the other hand, opposite responses to these items indicate low self-esteem and low self-confidence and a great deal of concern and anxiety about the presentation of self in everyday situations.

Aggressive feelings.—To measure covert aggressive feelings toward the supervisor, that is, those aggressive feelings which did not erupt into overt behavior, the following items were used:

1. How often did you become annoyed with the supervisor?
2. How often did you become irritated with the supervisor?
3. If you were to participate in this group again, how would you feel about having the supervisor replaced?

On the first two items, the subjects were asked to make responses on a six-point scale ranging from "continually" to "never"; for the third item, a seven-point scale was used ranging from "extremely favorable" to "ex-

[17] The supervisor's role demanded a person who could combine a certain ability to act with emotional stability, maturity, and general interpersonal insightfulness. The role was taken by a recent graduate in nursing who had a major in psychiatric nursing and whose past experience in therapeutic role-playing with mental patients and hospital-ward supervision constituted an excellent background for the job.

[18] R. de Charms and M. E. Rosenbaum, "Status Variables and Matching Behavior," *Journal of Personality,* XXVIII (1960), 492–502.

[19] I. L. Janis, "Personality Correlates of Susceptibility to Persuasion," *Journal of Personality,* XXII (1954), 504–18.

tremely unfavorable." The responses provided an estimate of the frequency with which each subject experienced aggressive feelings toward the supervisor during the work period.

Two sets of items similar to these were used to measure aggressive feelings toward co-workers and dissatisfaction with the task. The first set was identical with the above items except that the term "co-workers" was substituted for "supervisor." The response alternatives were the same except for the third item; here the subjects were asked to indicate the actual number of co-workers they would prefer to have replaced. In measuring dissatisfaction with the task, only the first two of the above items were used, but they were used twice, first with the term "molecules" and second with the phrase "job in general" substituted for the term "supervisor." Ranging from "continually" to "never," the response alternatives were the same as before.

Overt aggression.—As may be recalled, we assumed that overt aggression might be expressed directly as verbal aggression or indirectly as a conscious slowdown in production. Two rather complex measures thus were required.

After two female observers had reached a level of competence where they could reliably code verbal aggression as it occurred during the experimental-work period, they independently entered marks on forms each time a subject (*a*) antagonistically criticized the supervisor or used indirect sarcasm with definite negative content in reference to the supervisor; (*b*) antagonistically criticized or used sarcasm about her co-worker; or (*c*) antagonistically criticized or joked about the task or the experimental situation. In general, the observers were asked to perceive and evaluate each remark simultaneously along two dimensions: objective content and affective content. All remarks that were negative in objective meaning were counted as verbal aggression regardless of affective content. Remarks that were not objectively negative but tended to carry negative affective connotations were more difficult to categorize reliably. However, 85 per cent agreement of two observers was obtained throughout the experiment on items, not just cell totals.

The production-line arrangement of work required to encourage interdependence in interaction among subjects virtually precluded the possibility of taking accurate or even meaningful measures of each subject's production rate. Consequently, a measure based on the group's total production was used. This was calculated as the sum of the model components (the colored balls, pegs, and springs) completed per 40-minute work period, minus errors and omissions.

Factor analysis.—Data from each of the scales administered in the pre- and post-experimental questionnaires were factor-analyzed using the principal axis method. The obtained factor weights were used, together with standardized scores for each of the subjects, to obtain indexes for each of the above-mentioned dimensions[20] (Table 1).

RESULTS

Analysis of the results began with group data and the testing of basic hypotheses for the stable effects. Then individual scores and a smaller number of independent variables selected on the basis of the initial analysis were used to test the more complex hypotheses involving the mediating variable, that is, the psychological dimension of self-esteem.

THE BASIC HYPOTHESES TESTED WITH GROUP DATA

Results relevant to the basic hypotheses are presented in Tables 2 and 3. For all the basic hypotheses, the analysis-of-variance results in Table 2 gives significance as well as explained variance. In Table 3 the means are given where significant relationships were found.

From Tables 2 and 3 it is apparent that close supervision produced a significant and

[20] These procedures are outlined in detail in M. J. Hagood and D. O. Price, *Statistics for Sociologists* (New York: Henry Holt & Co., 1952), pp. 526–30.

TABLE 1

FACTOR ANALYSIS WEIGHTS* FOR PRE- AND POST-EXPERIMENTAL QUESTIONNAIRE SCALES

Scale	Item Number									
	1	2	3	4	5	6	7	8	9	10
1. Self-esteem	0.77	0.42	0.74	−0.50	−0.42	−0.75	−0.35	0.38	−0.65	0.58
2. Aggressive feelings, supervisor	.94	.96	.78	.....	.....	.....	.....	.....	.....	.....
3. Aggressive feelings, co-workers	.97	.95	.64	.....	.....	.....	.....	.....	.....	.....
4. Dissatisfaction, task	0.90	0.89	0.88	0.92	.....	.....	.....	.....	.....	.....

* These weights were extracted using the principal axis method. A separate analysis was done for each scale, and in each case the weights given are those obtained on the first factor.

TABLE 2

SUMMARY OF TWO-WAY ANALYSIS OF VARIANCE USING DATA TABULATED BY GROUPS

Dependent Variable	Close Supervision		Punitive Supervision		Interaction	
	F-Value*	Explained Variance†	F-Value	Explained Variance	F-Value	Explained Variance
1. Aggressive feelings, supervisor‡	10.3	.24	10.9	.25	3.0	.07
2. Aggressive feelings, co-workers	4.0	.16	0.0	.00	0.4	.02
3. Dissatisfaction, task	.6	.03	0.2	.01	0.1	.01
4. Verbal aggression, supervisor	1.8	.04	20.3	.48	0.9	.02
5. Verbal aggression, co-workers	2.8	.11	2.2	.09	0.1	.00
6. Verbal dissatisfaction, task	2.0	.09	1.4	.06	0.0	.00
7. Productivity	5.2	.17	4.2	.14	1.1	.04

* Results are significant at the 0.10 level if F is equal to or greater than 3.0; at the 0.05 level if F is equal to or greater than 4.4.

† The measure of explained variance is η.

‡ Using Bartlett's test, the variance of all dependent variables was tested for homogeneity. All variances were homogeneous except for this particular variable. To achieve homogeneity, the scale was transformed using the following formula: $\frac{1}{2}(X - 100/50)$. We wish to thank Keith Miller for helping us find this and other transformations used later.

TABLE 3

CELL MEANS FOR TWO-WAY ANALYSIS OF VARIANCE

Dependent Variable	Means			
	General Non-punitive Style	Close Non-punitive Style	General Punitive Style	Close Punitive Style
1. Aggressive feelings, supervisor*	0.73	1.14	1.15	1.27
2. Aggressive feelings, co-workers*	1.84	2.07	1.70	1.84
3. Dissatisfaction, task*	1.90	2.06	1.64	2.01
4. Verbal aggression, supervisor†	0	4.5	10.8	11.7
5. Verbal aggression, co-workers†	5.2	14.0	15.2	19.2
6. Verbal dissatisfaction, task†	9.3	17.8	16.5	24.5
7. Productivity‡	335	252	258	226

* These measures of aggressive feelings are based on standard scores which have little obvious meaning. However, the means are included here to give an idea of the relative magnitude of the effects of close and punitive supervision.

† All of these verbal measures are in terms of the number of aggressive or negative remarks.

‡ In terms of the number of correct connections of the pegs, springs, and balls completed during the work period.

large increment in aggressive feelings toward the supervisor. The data also indicate a moderate and near-significant increment in aggressive feelings toward co-workers. On the other hand, close supervision was not significantly related to dissatisfaction with the task, to verbal aggression against the supervisor or co-workers, or to verbal dissatisfaction with the task. Finally, the data indicate that close supervision results in a significant and rather substantial decrease in productivity.

Tables 2 and 3 also indicate that the punitive style of supervision resulted in a large, significant increment in aggressive feelings toward the supervisor. However, in this case the relationships between punitive supervision and aggressive feelings toward co-workers or dissatisfaction with the task are both small and insignificant, as are the relationships between punitiveness and verbal aggression toward co-workers or verbal dissatisfaction with the task. Unlike close supervision, however, punitive supervision resulted in a large, significant increase in verbal aggression toward the supervisor. Finally, it is evident that punitive supervision also resulted in a relatively large decrease in productivity—a decrease which, because of a small N, is of borderline significance.

Thus far the data have not supported all of the basic hypotheses. The results with respect to direct verbal aggression and the displacement of aggressive feelings toward co-workers and the task were variable. Consequently, the more detailed analysis will be limited to two dependent variables: aggressive feelings toward the supervisor and productivity.

TABLE 4

SUMMARY OF THREE-WAY ANALYSIS OF VARIANCE INVOLVING CLOSE SUPERVISION, PUNITIVE SUPERVISION, AND SELF-ESTEEM, USING DATA TABULATED IN TERMS OF INDIVIDUAL SCORES

Source of Variation	Aggressive Feelings, Supervisor*		Productivity	
	F-Value†	Explained Variance‡	F-Value	Explained Variance
1. Close supervision	9.3	.08	23.7	.17
2. Punitive supervision	11.3	.10	18.0	.13
3. Self-esteem§	0.3	.00	0.1	.00
Close style×punitive style	2.7	.02	4.0	.03
Close style×self-esteem	5 0	.04	1.1	.01
Punitive style×self-esteem	1.0	.01	0.7	.00
1×2×3	0.0	.00	0.7	.01

* Using Bartlett's test, the variance of this dependent variable was tested for homogeneity with negative results. The unhomogeneous variance was corrected using a log x transformation.

† Results are significant at the 0.10 level if F is equal to or greater than 2.79; at the 0.05 level if F is equal to or greater than 3.96.

‡ The measure used for explained variance is η.

§ Because of unequal cells, the approximate method for analysis of variance was that presented in Helen M. Walker and Joseph Lev, *Statistical Inference* (New York: Henry Holt & Co., 1953).

HYPOTHESES INVOLVING SELF-ESTEEM AND ANALYSIS IN TERMS OF INDIVIDUAL SCORES

The results relevant to the mediating hypotheses are found in Tables 4, 5, and 6. Table 4 gives the results of an analysis of variance, with significance levels and explained variance; Tables 5 and 6 show the means involved in the significant interactions. Before turning to these interactions, note that the relationships between punitive or close supervision, on the one hand, and aggressive feelings toward the supervisor or productivity, on the other, are much

stronger in this than in the preceding analysis. This reflects the difference between group and individual data—largely the difference between an N of 24 (groups) and an N of 96 (individual subjects).

In Table 4 the findings indicate a significant interaction between closeness of supervision and self-esteem with respect to feelings of aggression toward the supervisor, but not with respect to productivity. In Table 5, the means indicate that this significant interaction is precisely the one that was predicted. The relationship between close supervision and aggressive feelings toward the supervisor is much stronger among subjects with low than among subjects with high self-esteem. In fact, the difference between the means for subjects with high self-esteem is nil. In other words, the over-all relationship observed between closeness of supervision and aggressive feelings toward the supervisor is due primarily to the subjects with low self-esteem. Yet, at the level of indirect aggression apparently no difference existed, since the interaction between close supervision and self-esteem with respect to productivity involved very little variance and was insignificant as well. In the close supervision variations, the subjects with high self-esteem evidently engaged in indirect aggression through lowered productivity as readily as did those with low self-esteem. The difference apparently was in their emotional state; that is, whether or not aggressive feelings, perhaps anger, accompanied their decision to decrease productivity. However, it should be noted that this pattern could be an artifact of the group productivity scores.

TABLE 5

MEANS INVOLVED IN SIGNIFICANT INTERACTION BETWEEN CLOSE SUPERVISION AND SELF-ESTEEM WITH RESPECT TO AGGRESSIVE FEELINGS TOWARD SUPERVISOR

Means	Aggressive Feelings, Supervisor
General style, low self-esteem	1.54
Close style, low self-esteem	1.74
General style, high self-esteem	1.64
Close style, high self-esteem	1.66

In Table 4 it is apparent that the interactions between punitive supervision and self-esteem with respect to both aggressive feelings and productivity are insignificant. Apparently the experience of being aggressed against in the form of punitive supervision produces aggressive feelings as well as indirect aggression equally in subjects with high and low self-esteem. We can make this assumption with some confidence because the variance involved in the relevant interactions is very low and because we have seen that the measure of self-esteem is sensitive enough to detect rather precise effects.

Finally, are the effects of the two supervisory styles a simple additive function? If they are, the interactions between close and punitive supervision with respect to any of the aggression variables will be insignificant. It is apparent in Table 4 that the interaction between the close and punitive styles of supervision with respect to aggressive feelings toward the supervisor only approaches significance at the 10 per cent level. However, the interaction with respect to productivity is significant, even though a modest amount of variance is involved. This result, of course, implies something more than a simple additive effect with respect to productivity. Apparently the effects of punitive and close supervision with respect to productivity are less than would

TABLE 6

MEANS INVOLVED IN SIGNIFICANT INTERACTIONS BETWEEN CLOSE AND PUNITIVE SUPERVISION WITH RESPECT TO PRODUCTIVITY

Means	Productivity	
	No. of Units	Percentage Reduction
General style, non-punitive	84	100
Close style, non-punitive	63	75
General style, punitive	65	77
Close style, punitive	56	67

be expected on the basis of the effects of close supervision alone and punitive supervision alone. As can be noted in Table 6, close supervision by itself reduces productivity by 25 per cent and punitive supervision reduces it by 23 per cent. Together they do not reduce it by 48 per cent as would be expected if the effects were a simple additive function, but only by 33 per cent. Since the decrease in productivity, indicating as it does an increase in aggression, is less than might be anticipated, we will refer to the phenomenon apparent in this interaction as the dampened-increment effect.

This dampened-increment effect might have occurred for one of two reasons. First, productivity in the experimental situation might have been very difficult to reduce below some minimal level regardless of aggressive feelings or impulses to reduce it still further. Second, the aggressive feelings themselves might not have been additive. In other words, double frustration may not lead to double aggressive feelings, but to something much less than double. As can be seen from the first line in Table 3, the data are consistent with this latter interpretation. The scores indicating aggressive feelings toward the supervisor are increased (from .73) 41 points by close supervision alone and 42 points by punitive supervision alone. Together, however, they increase the score only 54 points, as compared with an increment of 83 points that would be predicted if the effects were not dampened. These data, showing as they do a dampened-increment effect, are remarkably consistent with the data in the previous paragraph which showed a similar effect with respect to productivity.

DISCUSSION

Over-all, the results present an interesting pattern that is laden with implications. The lack of support for a number of the hypotheses matters very little, as these involved displacement which usually vitiates the strength of aggressive phenomena. But the interesting thing is that in the close supervision situations a certain amount of displacement evidently did occur. The subjects by and large expressed more than usual aggressive feelings against one another as co-workers. It is this tendency to displace, plus the absence of verbal aggression toward the supervisor, which distinguishes the close from the punitive supervision situations. In the latter, the tendency to displace aggressive feelings was conspicuously absent and verbal aggression toward the supervisor conspicuously present. Why should such a difference obtain?

A number of explanations are possible, but the one that suggests itself involves the generic distinction between close and punitive supervision made in the theoretical section, that of intention. Our argument there was that with close supervision the *intention* is simply to increase production; the resulting psychological pain is unanticipated, unintentional, and possibly even an unknown consequence. Therefore, close supervision was characterized as frustrating rather than aggressive. On the other hand, with punitive supervision, the pain-producing activities are used intentionally because the pain presumably reinforces the desired avoidance responses. Since activities used with the intention of producing pain or injury are by definition aggression, we pointed out that punitive supervision is a form of aggression. Thus, at a more generic level, the close supervision-aggression hypotheses tested here are simply variants of the basic frustration-aggression hypothesis, whereas the punitive supervision-aggression hypotheses are basically aggression-aggression hypotheses. Phrased this way, these hypotheses may appear to be circular, but they are not. What really is meant is: "To the extent that A frustrates B, B will be instigated to aggress against A," and "To the extent that A aggresses against B, B will be instigated to aggress against A." In other words, genuine causal relationships are involved in the hypotheses because the hypotheses involve an exchange between two individuals.

If this distinction is valid, then the

difference in response patterns to close and punitive supervision may actually represent a more generic difference in response patterns to frustration and aggression. Evidently, when A either frustrates B or aggresses against B, the unvarying result is the instigation of aggressive feelings in B. Furthermore, if an indirect avenue of aggression is available, such as decreasing productivity, then in either case B will use this indirect aggression against A. However, a basic difference evidently arises at the level of direct verbal aggression. Although they recognized that the pain they felt was not intended, those subjects who were frustrated by the close supervision practices had a difficult time expressing their aggressive feelings directly at the verbal level, whereas those subjects who felt the aggression inherent in punitive supervision, perhaps because they recognized it as intentional, retaliated openly in kind. Apparently because the latter subjects were able to verbalize their aggressive feelings directly, they were not led into displacing the feelings as were the subjects who experienced the frustrations of close supervision.

Before concluding, we must pay tribute to the women who were subjects in this investigation. While their behavior was not predicted precisely, it was not entirely unpredictable, as we sometimes feared it would be. They probably reacted to the various styles of supervision in much the same way as men would have reacted; most differences probably would be a matter of degree. However, our empirical impression is that women suppress their tendencies to overt aggression, particularly verbal aggression, more than do men. Rather than express their negative feelings in words, they tend to express them more in nervous laughter, or alternatively to withdraw more than do men. In other words, if the experiment were duplicated using men, we think any change in results would be with respect to verbal aggression: verbal aggression would be much more frequent in the punitive situation, and a significant relationship might obtain between close supervision and verbal aggression.

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ECONOMIC DOMINANTS AND COMMUNITY POWER

A COMPARATIVE ANALYSIS[1]

DONALD A. CLELLAND AND WILLIAM H. FORM

ABSTRACT

A comparison of the community power positions of economic dominants in a satellite city and an independent city over a century shows both similarities and differences. In both communities they withdrew from elective political offices as business became integrated into national markets. Partial withdrawal from offices in civic associations occurred later. Economic dominants in the independent city were more often cited as public leaders or top influentials, and were involved more frequently in local issues and projects than those in the satellite city. The research points to the importance of community variables as affecting the pace of bifurcation of local economic and political orders.

INTRODUCTION

Three avenues to the study of American community power structure have received widest attention during the last decade. The earliest approach studied a single set of community influentials who allegedly made the major community decisions.[2] Adherents of this method have generally concluded that business leaders are the "ruling elite" or at least *primi inter pares* in the community power structure. The second method discerned the power structure by examining how specific persons and groups behaved in specific community issues and decisions.[3] Those using this technique have generally found a pluralistic system of decision-making. The third avenue has investigated the forces changing the character of persons holding positions of potential power.[4] Irrespective of approach, an ideological question has been persistent—whether the community is governed informally by an economic elite or whether the dominant pattern is political pluralism, a situation where decision-makers represent groups with differing interests.

One instructive way of posing this controversy is to ask what types of relationships characterize the stratification orders in American communities in the past and in the present.[5] More specifically, the sociological question is: To what extent has private economic power been translated directly into community or public power? Although R. O. Schulze did not formally place his research within the Weberian framework, operationally he did study the question we have posed by tracing histori-

[1] We are grateful to Professor James B. McKee for a critical reading of the manuscript.

[2] The tradition of Robert S. Lynd and Helen Merrill Lynd, *Middletown in Transition* (New York: Harcourt, Brace & Co., 1937); C. Wright Mills, *The Power Elite* (New York: Oxford University Press, 1956); Floyd Hunter, *Community Power Structure* (Chapel Hill: University of North Carolina Press, 1953), and many others.

[3] E.g., Robert A. Dahl, "Equality and Power in American Society," in *Power and Democracy in America*, ed. William V. D'Antonio and Howard J. Ehrlich (Notre Dame, Ind.: University of Notre Dame Press, 1961); Nelson W. Polsby, "The Sociology of Community Power: A Reassessment," *Social Forces*, XXXVII (March, 1959), 232–36; Linton C. Freeman *et al.*, "Local Community Leadership," *Syracuse College Paper No. 15* (Syracuse, N.Y.: Syracuse University, 1960); Edward C. Banfield, *Political Influence* (New York: Free Press of Glencoe, 1961); and many others.

[4] Robert A. Dahl, *Who Governs?* (New Haven, Conn.: Yale University Press, 1961); Constance Green, *Holyoke, Massachusetts* (New Haven, Conn.: Yale University Press, 1939); Thorstein Veblen, *Absentee Ownership* (New York: Viking Press, 1939); and the works of R. O. Schulze cited in n. 6.

[5] In the framework of Max Weber as explicated in "Class, Status and Power," in *From Max Weber: Essays in Sociology*, ed. and trans. Hans H. Gerth and C. Wright Mills (New York: Oxford University Press, 1946).

cally the place of economically powerful figures in the public life of Cibola.[6] The study reported here attempts to replicate his investigation in a different type of community, which we shall call "Wheelsburg."

Schulze's findings upheld his hypothesis that as a city grows from an isolated, self-contained entity to an urbanized community "increasingly involved and interrelated in the large social complex," its sociopolitical power structure changes from a monolithic one dominated by persons possessing great economic power to a bifurcated structure comprising "two crucial and relatively discrete power sets, the economic dominants and the public leaders."[7] Economic dominants were defined as "those persons who occupy the top formal statuses in the major economic units within the community area,"[8] and public leaders (or top influentials) as those who, in the opinion of community "knowledgeables," exercise major influence and leadership in community affairs.[9]

Schulze tentatively explained the dissociation of economic dominants from local political-civic affairs by the following three trends:

(*a*) the establishment by a growing number of locally-owned industrial units of direct supplier relationships with a small number of large, non-local manufacturing plants; (*b*) the subsequent introduction into the local economic system of an increasing number of branch plants of large, absentee-owned corporations; and (*c*) the concomitant dissolution of the extensive networks of interlocking directorates and officerships which had formerly served to link significant numbers of local economic dominants within the community.[10]

These trends have also occurred in Wheelsburg, but to a more limited degree. The greatest variation between Cibola and Wheelsburg is in the first factor, because in Wheelsburg many local supply plants were established to serve the local automobile firms.

COMPARISON OF THE COMMUNITIES

The two communities differ significantly in a number of ways. For most of its history Cibola was a small independent town. It is now a satellite city of approximately 20,000 inhabitants located just beyond the Standard Metropolitan Area of a large midwest industrial center containing more than 3,000,000 people. The five largest of its eight major industrial plants were absentee-

[6] Robert O. Schulze, "Economic Dominance and Public Leadership: A Study of the Structure and Process of Power in an Urban Community" (microfilmed Ph.D. dissertation, University of Michigan, 1956); "The Role of Economic Dominants in Community Power Structure," *American Sociological Review*, XXIII (February, 1958), 3–9; "The Bifurcation of Power in a Satellite City," in *Community Political Systems*, ed. Morris Janowitz (Glencoe, Ill.: Free Press, 1961), pp. 19–80.

[7] "The Bifurcation of Power . . . ," *op. cit.*, pp. 21–22.

[8] *Ibid.*, p. 21. For Schulze's operational criteria for determining economic dominants and public leaders see *ibid.*, Appendixes A and B, pp. 73–75. Essentially the same criteria were utilized to identify the dominant economic units (and consequently economic dominants themselves) in the two cities. Number of employees, capital worth, and assessed valuation were used as measures. However, since Wheelsburg is a much larger city than Cibola, the minimum figures for cutoff points were necessarily larger. In Cibola the only dominant economic units were manufacturing plants, banks, and savings and loan companies. In Wheelsburg a wider variety of economic units was included in the dominant group, e.g., department stores, utilities, and insurance companies. In addition to the heads of the major economic units, all who were on the board of directors of two or more of the major economic units were also identified as economic dominants.

[9] As suggested by Hunter, *op. cit.* The "knowledgeables" who were interviewed in the two studies differed somewhat. Schulze's knowledgeables were the heads of local voluntary associations. This research relied on the nominations of fourteen high-ranking officials from seven institutional sectors of the community (mass communication, business, union, welfare, education, government, religion). David A. Booth and Charles A. Adrian compared the results of the method used by Schulze with the simpler method we employed, and found almost identical results (see their "Simplifying the Discovery of Elites," *American Behavioral Scientist*, V [October, 1961], 14–16).

[10] Schulze, "The Role of Economic Dominants . . . ," *op. cit.*, p. 6.

controlled. Cibola is an extreme example of a city that "has felt the full impact of the metropolitan drift of American life."[11] A period of rapid expansion began during World War II with the establishment just outside the city's boundaries of a gigantic war-production plant which employed over 40,000 workers at its peak. After the war the economic instability of absentee-owned companies occupying this plant caused wide and rapid fluctuations in the local labor force. Consequently, during the 1940's the community experienced rapid fluctuation and high turnover in population. At the time of Schulze's study employment at the main plant had leveled off at 9,500 as it became tied securely to the motor vehicle industry.

Wheelsburg is located about 60 miles west of Cibola. It is an independent city of over 100,000 dominating a metropolitan area with a population of approximately 180,000. Like Cibola, its economy is based primarily on motor vehicle production. In fact, the same motor vehicle company is the largest single employer in both communities. In Wheelsburg the company employs nearly 15,000 workers. However, significant sections of Wheelsburg's labor force are employed in state government and in a nearby state university. Wheelsburg's period of most rapid industrial and population growth occurred earlier than Cibola's, between 1900 and 1920. This growth largely reflected the success of locally owned automobile and supplier plants. Since 1920 Wheelsburg's growth has been moderate and steady even with the large invasion of absentee-owned companies. Such companies came earlier to Wheelsburg, but entered and grew more gradually than in Cibola.

Currently, thirteen of the twenty non-financial dominant economic units are absentee-controlled.[12] Unlike Cibola, (*a*) Wheelsburg's major firms have been fairly stable operations, (*b*) the vast majority of its labor force has always been employed within the city limits, (*c*) very few of its economic dominants have lived beyond the city's contiguous suburbs, and (*d*) the city is removed from the influence of a large competing metropolis. Wheelsburg, then, is a much more stable and "normal" type of community setting in which to test Schulze's hypothesis.

Following Schulze's method closely, we tested his main hypothesis by (1) reconstructing the formal participation patterns of economic dominants over the past century in the political and civic activities of the community; (2) ascertaining the representation of current economic dominants among public leaders, that is, in the "reputational" power structure; and (3) analyzing the role of current economic dominants in specific community issues and programs.

ECONOMIC DOMINANTS AS POLITICAL AND CIVIC LEADERS

In Wheelsburg, as in Cibola, the proportion of economic dominants who occupied high local governmental offices declined dramatically over the century. Data in Tables 1 and 2 reveal that in both communities prior to 1900 the economic dominants were highly represented in local government. The comparable percentages in each table are virtually identical. Although the twentieth century ushered in a sharp decline in the proportion of economic dominants holding public office in both communities, this decline was sharper in Wheelsburg than in Cibola. Moreover, in both cities, but especially in Wheelsburg, the offices held by economic dominants have been increasingly appointive rather than elective. Indeed, no economic dominant has

[11] Schulze, "The Bifurcation of Power . . . ," *op. cit.*, p. 24.

[12] An absentee-controlled company is defined as one having a majority of its board of directors living outside of the local community. In both Cibola and Wheelsburg, slightly less than 50 per cent of the dominant economic units were absentee-controlled—five of eleven units in Cibola and thirteen of twenty-seven units in Wheelsburg. In both cities, all of the financial units (three and seven, respectively) were locally owned.

served as mayor since 1899, or as councilman since 1932.

The trend of these developments in Wheelsburg may be seen more clearly by examining the data in terms of twenty-year periods. A precipitous decline in public officeholding by economic dominants occurred in the 1900–1920 period, with relatively little change thereafter. However, there has been a continuing change in the type of office held. In each succeeding twenty-year period, fewer of the economic dominants who held office were elected. Increasingly, they have come to hold ad-

TABLE 1

ECONOMIC DOMINANTS SERVING IN PUBLIC OFFICE IN WHEELSBURG AND CIBOLA

Period	No. of Economic Dominants	Per Cent			
		In Public Office	In Elective Office	On Governing Body	In Highest Public Office
1823–60:					
Wheelsburg...	*	.....	.....	.....	.....
Cibola........	12	83	83	75	50
1860–1900:					
Wheelsburg...	44	73	64	57	30
Cibola........	21	81	67	57	33
1900–1940:					
Wheelsburg...	80	25	11	4	0
Cibola........	43	26	16	12	5
1940–59:					
Wheelsburg...	71	14	0	0	0
Cibola........	31	23	13	10	3

* Wheelsburg was not incorporated until 1859.

Source: Cibola data, see Schulze, "The Bifurcation of Power . . . ," *op. cit.*, pp. 37–38.

TABLE 2

OFFICES HELD BY POLITICALLY ACTIVE ECONOMIC DOMINANTS IN WHEELSBURG AND CIBOLA*

Period	No. of Politically Active Economic Dominants	Per Cent		
		In Elective Office	On Governing Body	In Highest Public Office
1823–1860:				
Wheelsburg.........	.....	.....	.....	.....
Cibola............	10	100	90	60
1860–1900:				
Wheelsburg.........	32	88	78	41
Cibola............	17	88	71	41
1900–1940:				
Wheelsburg........	20	45	15	0
Cibola............	12	64	45	18
1940–1959:				
Wheelsburg.........	10	0	0	0
Cibola............	7	57	43	14

* "Politically active" refers to economic dominants holding any appointive or elective office.

Source: Cibola data, see Schulze, "The Bifurcation of Power . . . ," *op cit.*, pp. 37–38.

visory and honorary positions in local government. Since it is probably fair to assume that the power potential of appointive offices is less than that of elective offices, the shift of economic dominants from the latter may be taken as evidence of continuing loss of formal political power.

Schulze suggests that after 1900 the arena of local involvement of the economic dominants shifted from politics to voluntary associations. The Wheelsburg data confirm his observation. Thus data in Table 3 show that at the beginning the economic dominants were highly represented among the members and officers of the Chamber of Commerce, and that their representation declined at a later era. Apparently the Wheelsburg economic dominants were even more powerful in the Chamber than their Cibola counterparts, for one of their number was president during nineteen of the first twenty years of the organization's existence. During the past two decades their representation in the Chamber has declined, but not so sharply as in Cibola. An historical analysis of the proportion of officerships held by Wheelsburg economic dominants in other civic organizations (major service clubs, community chest, and the board of trustees of the leading local hospital) reveals patterns of withdrawal similar to that evident in Table 3. While it is difficult to estimate the power potential of these officerships, current public leaders or top influentials regard the Chamber of Commerce as the single most influential organization in the city. Yet, as indicated above, direct control of this organization by economic dominants has probably declined over the years.

In both Wheelsburg and Cibola economic dominants reduced their incumbency in public offices at the turn of the century. A similar withdrawal from civic leadership positions began about 1940.[18] A comparative analysis of the economic development of the two communities corroborates some of Schulze's explanations and contradicts others. The evidence fails to support Schulze's position that the growth of absentee ownership and the dissolution local business ties (interlocking direct rates) among the economic dominants a count for their withdrawal from pub office. In both communities these phenom na occurred *after* the withdrawal; Cibola, the first absentee-controlled pla was established in 1932, and in Whee burg as late as 1940 two-thirds of t major economic units were locally own Moreover, 80 per cent of the Wheelsbu

TABLE 3

ECONOMIC DOMINANTS AS BOARD MEMBER OF CHAMBER OF COMMERCE

Period	Median No. of Memberships per Year on Board of Directors*		No. Serving as Presiden	
	Wheelsburg	Cibola	Wheelsburg	Cib
1901–6	8	......	3	...
1906–13	9	......	4	...
1913–20	9	......	4	...
1920–27	10	6	3	3
1927–34	9	3	2	2
1934–41	9	3	2	0
1941–48	4	2	0	1
1948–55	3	1	3	0
1955–59	5	......	0	...

* The number of directors varied from 15 to 18 in Cil and from 15 to 21 in Wheelsburg.

Source: Cibola data, Schulze, "The Bifurcation of P er . . . ," *op. cit.*, p. 49. Since the Cibola Chamber was foun in 1920, there are no data for earlier periods.

economic dominants maintained local bu ness ties with other economic domina as late as 1940. A third factor whi Schulze associated with withdrawal, nan ly, the growth of direct supplier relatic ships to non-local industries by loca owned plants, must also be discarded, i in Wheelsburg no such growth took pla

[18] "Withdrawal" is probably an apt phr because no evidence is available to suggest t there was community pressure on the econo dominants to reduce their community involveme However, individual economic dominants were co stantly changing. Their withdrawal consisted 1 so much in dropping civic leadership positi as in the failure of new economic dominants to s such positions.

and yet the pattern of withdrawal was similar to that of Cibola. Moreover, in Wheelsburg this withdrawal does not seem to have been forced by the growing political power of ethnic groups as was the case in many American cities.[14] There has never beeen a large ethnic proletariat in Wheelsburg, nor have local politics ever been heavily based on ethnic lines or class conflict.

What factors, then, are associated with the sharp decline in political participation by economic dominants (i.e., the bifurcation of political and economic power structure) since the turn of the century? At the broadest level of explanation, the increased involvement of the community and its economic units in state and nationwide social economic systems was, no doubt, an important factor. More specifically, in Wheelsburg, the end of the period in which political and economic power tended to coincide was marked by the rise of a new breed of economic elite, namely, managers and owners of the new automobile and supply plants. Younger, wealthier, operating larger businesses, more directly involved in the day-to-day operation of their businesses, introducing a wide variety of new products, these men did not participate in local politics probably largely because they lacked the time and because they probably found that business was much more exciting. A growing separation of wealth and social honor may have been a second factor, but the new economic elite was partly based on old local wealth and the majority were entrepreneurs rather than simply managers of companies financed by non-local capital. However, in the absence of ethnic and class cleavage in the community, it is doubtful that the new economic dominants, many of whom were classed Horatio Alger success models, lacked the popularity needed for election. They probably did not choose to run.

On the other hand, later withdrawal from civic leadership positions seems to be associated with the introduction of absentee-owned plants and the related decrease in common local business ties (interlocking directorates) among the economic dominants. The importance of the latter factor is underscored in Wheelsburg where economic dominants not only have more local economic linkages but also comprise a larger proportion of the local civic leaders.[15]

The so-called pattern of withdrawal needs to be interpreted within a broad context of the local participation. In Wheelsburg, the historical pattern has been for the economic dominants to become officers of new organizations as they emerged in the community, then to retain membership, and later to withdraw from active participation. Thus when the Chamber of Commerce was created, dominants were its earliest officers; when the service clubs arose they again became officers; when the Community Chest arrived they became its sponsors and officers; and they sponsored the largest hospital and dominated its board. This pattern of domination and later "withdrawal" is subject to various interpretations. We are inclined to believe that it demonstrates two related phenomena: (*a*) assumption of officerships in new organizations validated not only their importance to the community but the power and status of the original officers, namely, the economic dominants, and (*b*) the policies, direction, and administration of the new organizations were set and institutionalized by the original officers. After this initial period the organizations needed

[14] E.g., in New Haven, from the late nineteenth century until recently, local politics were controlled primarily by "ex-plebes," individuals on the rise from the ethnic proletariat, who gained office through "the skills of ethnic politics." From 1842 to 1898, New Haven politics were dominated by the leading entrepreneurs. It may be significant that the period of dominance by economic dominants is almost identical in New Haven, Wheelsburg, and Cibola (see Dahl, *Who Governs?* chap. iii and iv).

[15] Sixty-five per cent of the economic dominants in the 1940–59 period were associated as officers, partners, or directors in at least one other business with other economic dominants.

only informal and non-official guidance from the dominants and not their active officeholding. In other words, a change in officers did not necessarily mean a change in policy or loss of power and control by dominants.[16]

COMMUNITY INFLUENCE OF ECONOMIC DOMINANTS

In order to assess the community influence of current economic dominants in Wheelsburg, two procedures were used. First, their reputational influence was investigated by assessing their representation in the list of public leaders (community influentials as determined by the method outlined in n. 9). Second, their "actual" influence was probed by examining their role in a number of community issues or projects.

In 1958–59, thirty-nine individuals were found to be economic dominants, and coincidentally, thirty-nine people were designated as public leaders. The names of twelve persons (31 per cent) appeared on both lists. This overlap is considerably higher than that found in Cibola where only two of seventeen economic dominants were among the community's eighteen public leaders. Moreover, eight of the top fifteen public leaders in Wheelsburg, including the top four, as rated by the public leaders themselves, were economic dominants. Although major absentee-owned corporations were "underrepresented" among the economic dominants who were also public leaders, "U.S. Motors" (the absentee-owned industrial giant in the community) was represented by three executives (two of whom were not defined as economic dominants). From these observations we cannot conclude that two discrete power sets are found in Wheelsburg.

In order to substantiate the basic dissimilarities between the economic dominants and public leaders in Cibola, Schulze examined their patterns of political and civic participation. He found that the economic dominants had held only about half as many governmental offices as the public leaders. The same was true in Wheelsburg, although both groups were less active. Somewhat surprisingly, economic dominants were as well represented as the public leaders in the five most influential associations. Table 4 reveals a similar situation of high participation by both economic dominants and public leaders in Wheelsburg's most influential associations. However, the Cibola situation of wide differences between public leaders and economic dominants in the number of officerships held in these associations was not in evidence. Table 5 reveals that a higher proportion of economic dominants in Wheelsburg (from both locally and absentee-owned companies) have in the past held office in the five most influential organizations. Differences are small between the two communities in the proportions currently holding such offices. In short, both Tables 4 and 5 document no deep bifurcation in associational participation between Wheelsburg's economic dominants and public leaders. The relatively high rate of participation by absentee-owned corporation executives is especially notable.[17]

One of the reasons for the failure of economic dominants to participate in the civic life of Cibola was that they regarded the city mainly as the locus of their work life and not their community life.[18] More-

[16] Lest the concentration on "withdrawal" be overwhelming, it should be noted that almost half the economic dominants in the 1940–59 period held civic leadership positions in Wheelsburg and that their participation in the Chamber of Commerce was increasing.

[17] Although managers of the largest absentee-owned corporation did not dominate the local scene as extensively as in the case of Bigtown, they did have representatives on most of the local bodies to co-ordinate knowledge of what was going on in the city. For data on Bigtown see Roland J. Pellegrin and Charles H. Coates, "Absentee-owned Corporations and Community Power Structure," *American Journal of Sociology*, LXI (March, 1956), 413–19.

[18] A large proportion lived in other communities in the metropolitan area and may have participated in the associational life of these other communities.

TABLE 4

MEMBERSHIP OF CURRENT PUBLIC LEADERS AND ECONOMIC DOMINANTS IN THE MOST INFLUENTIAL ASSOCIATIONS*

Association	Per Cent Belonging to Association			
	Public Leaders	Economic Dominants		
		Local	Absentee	Total
Chamber of Commerce:				
Wheelsburg	87	96	100	97
Cibola	78	100	87	94
Rotary:				
Wheelsburg	49	38	40	38
Cibola	50	70	14	47
Kiwanis:				
Wheelsburg	18	13	7	10
Cibola	44	30	0	18
Lions:				
Wheelsburg	5	8	0	5
Cibola	11	0	0	0

* In Cibola the five most influential associations were determined by polling the voluntary association heads, public leaders, and economic dominants. The four associations listed above and the Junior Chamber of Commerce were named by all of the groupings questioned. These organizations were also designated by Wheelsburg public leaders as highly influential. Since few public leaders or economic dominants were young enough to be eligible for membership in the Junior Chamber of Commerce in either city, and none were members, *this association was omitted from the table.*

Source for Cibola data: Schulze, "The Bifurcation . . . ," *op. cit.*, p. 47.

TABLE 5

OFFICERSHIPS OF PUBLIC LEADERS AND ECONOMIC DOMINANTS IN FIVE MOST INFLUENTIAL COMMUNITY ASSOCIATIONS

	Public Leaders		Economic Dominants					
			Local		Absentee		Total	
	Wheelsburg	Cibola	Wheelsburg	Cibola	Wheelsburg	Cibola	Wheelsburg	Cibola
Per cent having served as president of at least one of the five associations	31	61	25	20	20	0	23	12
No. of presidencies occupied in the five associations	17	14	8	2	3	0	11	2
Per cent *currently* serving as officer or board member in at least one of the five associations*	18	44	4	10	27	30	13	18
No. of officerships or board memberships *currently* held in the five associations	7	12	1	1	4	2	5	3

* "Currently" refers to the year of research: 1954 for Cibola, 1958–59 for Wheelsburg.

Source: Cibola data, Schulze, "The Bifurcation of Power . . . ," *op. cit.*, p. 48.

over, their private economic interests were primarily non-local. This may not be surprising since the city's largest economic units were absentee-owned and oriented toward a national market. However, Table 6 indicates that a much more extensive network of economic ties exists in Wheelsburg than in Cibola.[19] Despite a high degree of absentee ownership in Wheelsburg, a fairly extensive network of economic ties unites the interests of the economic dominants and the public leaders. These ties may explain the higher rate of civic participation by its economic dominants and their closer social integration to public leaders.

As a final demonstration of the bifurcation of Cibola economic dominants and public leaders, Schulze analyzed the decision-making process on two important community issues. The economic dominants refused to become involved in resolving either of them, leaving the public leaders autonomous but perhaps without a solid power basis for community action.

In Wheelsburg, an analysis of eleven community issues[20] revealed that eight of the economic dominants who were also public leaders were among those mentioned as influential in initiating and resolving these issues. Economic dominants, including some representing absentee-owned corporations, either initiated or co-initiated programs of action for six of the eight issues in which they were involved. Although this evidence suggests that economic dominants have not withdrawn from community decision-making and that they are not just ceremonial leaders, apparently they do not form a monolithic power elite. Different individuals became involved in different issues, doing so in the process of playing their own "games."[21]

Not all of the broad community issues in which economic dominants were involved were controversial. Some of them may more properly be called "projects." The major issues in Cibola seemed to involve a higher degree of conflict in the

TABLE 6

NUMBER OF KNOWN LOCAL ECONOMIC TIES AMONG PUBLIC LEADERS AND ECONOMIC DOMINANTS

	Public Leaders	Economic Dominants	
		Local Firm	Absentee Firm
Public leaders:			
Wheelsburg......	23	31	8
Cibola............	4	3	2
Local-firm dominants:			
Wheelsburg.......	....	47	11
Cibola............	....	15	0
Absentee-firm dominants:*			
Wheelsburg.......	....	....	2
Cibola..........	....	....	2

* In neither Wheelsburg nor Cibola were there any economic ties between absentee-firm dominants from *different* corporations. In the case of two absentee firms in Wheelsburg, a second person in addition to the general manager was defined as an economic dominant because he held a directorship in a local bank as well as an officership in the absentee-owned firm.

Source: Cibola data supplied by Robert O. Schulze in an unpublished manuscript.

[19] "Economic ties" are instances in which a pair of individuals serves as officers or directors of the same firm. Each pair is counted as one economic tie. For example, if four public leaders serve on the board of directors of a bank, there are six economic ties (pairs).

[20] These issues were selected and recapitulated by the public leaders in interviews. They included hospital expansion drive, downtown development, establishment of a metropolitan planning agency, improvement of airport terminal facilities, establishment of a tricounty planning agency, annexation of a school district to the city, widening of a city street, ban on Sunday shopping, proposed shift of location of city hall, proposed sale of bonds by the city to finance construction of parking facilities, and proposed annexation of a suburban shopping center. Our inspection of newspapers and other documents reveals that these indeed represent nearly the full range of community issues during the last five or six years. One or two others might be added by other local interests such as organized labor (see William H. Form and Warren L. Sauer, "Community and Labor Influentials: A Comparative Study of Participation and Imagery," *Industrial and Labor Relation Review*, XVII [October, 1963], 3–19).

[21] Norton E. Long, "The Urban Community as an Ecology of Games," *American Journal of Sociology*, LXIV (November, 1958), 251–61.

political arena. Perhaps this conflict reflected the inertia of partisan party politics which existed in the community as late as 1947. In addition, both of the major issues in Cibola—adoption of a new city charter and annexation—were the direct results of rapid urbanization and industrialization, processes which had occurred at a more gradual rate in Wheelsburg. There, political life seemed less marked by conflict, for local government not only was non-partisan but it traditionally and customarily responded to the needs of business.[22] It is highly probable that the lack of political conflict and the tendency for community decision-making to be channeled to the private rather than public sphere are interdependent. In Wheelsburg there was little evidence of basic differences in values among the economic dominants, the public leaders, and the elected officials. If representation of conflicting interests or values is chosen as the indicator of pluralism in the power structure, Wheelsburg (and most American communities) will be judged less pluralistic than if a weaker test of pluralism, such as the participation of separate individuals in different issues, is used.[23]

Thus, the social climate of the decision-making roles of the economic dominants in the two cities is not identical. Whether Wheelsburg dominants would become involved in highly conflictful issues should they arise is not known. Certainly they hesitated to publicize their involvement in controversial issues.[24] One large firm, for example, refused to become overtly involved in an annexation issue despite the fact that its economic interests were involved. However, it made its position known. What covert influence this might have had cannot be accurately appraised. Yet, since executives of the absentee-owned corporations were less likely to become involved in community decision-making than economic dominants from locally owned enterprises, possibly Wheelsburg's pattern of influence is evolving toward the type found in Cibola. On the other hand, both economic dominants and public leaders work hard to solve issues without conflict, and controversial issues probably arise less often in gradually expanding cities such as Wheelsburg than in cities which have grown very rapidly and have experienced extreme economic fluctuations, such as Cibola. Further research is required to determine the power roles of economic dominants in cities differing in size, social composition, economic composition, and economic history.

CONCLUSIONS

Comparative analysis of the roles of economic dominants in power structures of a satellite and an independent city reveals that in both communities the formal political and economic power structures which were once melded have tended to become bifurcated over time. This process seems to have paralleled the integration of local economic units into national markets and the process of governmental centralization. The economic dominants, once highly active leaders in civic associations, have tended to reduce their participation in this area, especially in the satellite community. This withdrawal coincided roughly with the rapid extension of absentee ownership in both cities. Currently, the nearly complete bifurcation of economic dominants and public leaders (top influentials) found in the satellite

[22] Form and Sauer, *op. cit.*

[23] For a fuller discussion of this problem see Marshall N. Goldstein, "Absentee Ownership and Monolithic Power Structures: Two Questions for Community Studies," in *Current Trends in Comparative Community Studies*, ed. Bert E. Swanson (Kansas City, Mo.: Community Studies, Inc., 1962), pp. 49–59.

[24] The same attitudes were revealed in interviews conducted by Rossi in Mediana. This does not mean that economic dominants had withdrawn from local influence systems because, as Rossi points out, "this is the age of community projects" (Peter H. Rossi, "The Organizational Structure of an American Community," in *Complex Organizations*, ed. Amitai Etzioni [New York: Holt, Rinehart & Winston, 1961], p. 301).

city was not as evident in the independent city, where an extensive network of economic ties bound the two groups together. Moreover, unlike the economic dominants in the satellite city, those in the independent city have not abandoned their decision-making role in community issues.

While the evidence cited in this research is not conclusive, it points to variable patterns of relations between economic dominants and public leaders in different types of communities. Apparently the absence of local party politics, a history of local industries becoming absentee-owned rather than the introduction of branch plants from outside the community, the institutionalization of local political controls, and the absence of ethnic, class, or other cleavages which contribute to partisan politics reduce the withdrawal rate of economic dominants from participation in community associations and local power arrangements. The time is ripe for many rapid comparative studies of a wide range of communities to determine more precisely the factors responsible for the bifurcation of persistence of ties between economic dominants, civic leaders, and community influentials.

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GENERATIONAL AND ETHNIC DIFFERENCES AMONG CAREER POLICE OFFICERS[1]

JAMES Q. WILSON

ABSTRACT

Despite their small proportion of the city's population, second-generation Irish continue to be found in large numbers at all age levels among the sergeants of one large city's police department. There is no direct evidence that Irish over-representation among sergeants is the result of favoritism; there is some evidence that the Irish are unlike "American" and "European" officers in the attitudes they have toward police work and its rewards and in the extent of their friendship and familial ties with other police officers.

The Irish cop, like the Irish politician, has long been a legendary figure. And like many legends, this one has been in great part the popular expression of a sound sociological insight—that the big-city police department, like the big-city political machine, has been an important avenue of upward mobility for a sizable American ethnic group. The difficulty with the insight is that it has not kept up with the legend: The police forces of many large cities have continued to be heavily Irish Catholic long after the great wave of Irish immigration subsided and long after the spread of mass education, the collapse of anti-Irish discriminatory practices, and the growth of the urban middle class should have made police work a career of diminishing value to a group so long in this country.

A survey of all the sergeants of one large American police department provides some data which illustrate the continuing importance of police careers to second-generation Irish and a few clues to the reasons for this phenomenon.[2]

IRISH PREVALENCE

As Table 1 indicates, the proportion of sergeants with fathers born in Ireland has been remarkably constant over all age groups; about the same percentage of sergeants now serving who entered the force before 1936 are second-generation Irish as of sergeants now serving who entered the force in 1951–61. During the same period, the proportion of persons of Irish stock (one or both parents born in Ireland) in the city as a whole fell steadily, and even as long ago as 1930 was not large. By contrast, the proportion of sergeants whose fathers were born in the United States has steadily increased, from less than half of those who entered before 1936 to nearly two-thirds of those who entered after 1951. (Needless to say, a large but unknown percentage of these officers with native American fathers are in fact third- and fourth-generation Irish.) The great decline has occurred among sergeants whose parents were born abroad but not in Ireland—among officers with, for example, German, Italian, Polish, Russian, Swedish, or French parents. Sergeants of non-Irish foreign stock account for almost 30 per cent of those who joined before 1936, but less than 10 per cent of those who joined in 1946–50. In 1951–61, there was a significant in-

[1] Paper delivered at the Fifty-eighth Annual Meeting of the American Sociological Association, Los Angeles, California, August, 1963. I would like to thank Arthur Stinchcombe, Edward C. Banfield, Norton Long, and Albert Reiss for their valuable comments on an earlier draft of this paper.

[2] A total of 818 sergeants completed a self-administered questionnaire in 1960–61. This was virtually a 100 per cent sample of all officers in that rank. Over two-thirds of the sergeants had, at the time of the survey, been in that rank less than five years; hence, a large number of men was included who had only recently been patrolmen. No data are available, however, on the patrolmen themselves.

crease in the proportion of such officers; this may signal the entry into this rank of increasing numbers of second-generation Europeans. (The city as a whole has very large Polish, Italian, and Russian settlements. It also has a very large Negro population, but at the time of this survey there were practically no non-whites above the rank of patrolman on the force.)

Equally as striking as the continued presence of second-generation Irish among police sergeants is the absence of first-generation Irish. Nearly 99 per cent of all sergeants were born in the United States; nearly all of these were also born and raised in or very near the city in which they are now serving. (The law requires local residence.) The more than eight hundred sergeants on this city's police force (and, for that matter, the lieutenants and captains also, as other data indicate) are, to an overwhelming degree, likely to be "locals" rather than "cosmopolitans"—men with long standing ties to their community and neighborhood who have known no other large city and whose friends are of the same type. (This, of course, is not true of all police forces. Data gathered in a large city on the West Coast show that 75.6 per cent of all officers were born and raised outside the city and 50.0 per cent outside the state.)

TABLE 1

PERCENTAGE OF POLICE SERGEANTS NOW SERVING WHO HAVE FATHERS BORN IN VARIOUS COUNTRIES, BY YEAR SERGEANTS JOINED FORCE

Date of Joining	Percentage of Sergeants with Fathers Born in:					Irish Foreign Stock as Per Cent of Total City Population*
	United States	Ireland	Europe	Total	N	
Before 1936	47.2	23.1	29.6	99.9	108	5.0
1936–40	49.0	27.4	23.5	99.9	102	4.2
1941–45	57.0	31.2	11.7	99.9	128	3 8
1946–50	65.0	25.9	9.2	100.1	262	3.4
1951–61	63.2	19.7	17.0	99.9	147	2.6
Total sample	58.4	25.4	16.1	99 9	747†	...

* Irish foreign stock in city consists of persons who were born in Ireland or who had one or both parents born in Ireland (i.e., first- and second-generation Irish combined). Percentages interpolated from decennial census.

† Seventy-one sergeants out of 818 did not answer.

Concomitant with the decrease in the proportion of sergeants who are of non-Irish foreign stock (to be called "Europeans" for the sake of brevity) has been a decline in the proportion who are Protestant. Not only is the leadership cadre of the force as Irish today as it was twenty years ago; it has also become even more Catholic than formerly, as shown in Table 2. Over 99 per cent of all second-generation Irish sergeants (for the sake of brevity, simply "Irish") have always been Catholic; an unchanging proportion (about three-fourths) of all sergeants with American-born parents ("Americans") have been Catholic; the real increase in Catholic sergeants has occurred almost entirely among the Europeans.

SOME EXPLANATIONS

Two different, though by no means mutually exclusive, explanations may be offered to account for the continuingly high proportion of Irish officers on this force. The first is the possibility that the Irish patrolman has been the beneficiary of a promotional system which is (or has been) biased in his favor. He is more likely than a non-Irishman to become a sergeant, so the theory goes, because the distribution of power within the force has operated to in-

sure that at the very least Catholics, and ideally Irish Catholics, would get the top jobs. This explanation is commonly offered by patrolmen who have *not* been promoted to sergeant and by civilian critics of the department and of the city government generally.

There is some circumstantial evidence to lend credence to this view. The most recently promoted group of sergeants (i.e., those with a date of rank between 1956 and 1961) contains a significantly higher proportion of Europeans and of Protestants. These men attained their rank after a new, "reform" police commissioner had taken office. One of the first acts of the new commissioner was to hold new competitive promotional examinations and to change certain administrative policies (such as the method used to assign "efficiency ratings" to officers) which might have worked in the past to make it difficult for non-Irish officers to be promoted. If these efficiency ratings had been used to favor Irish officers, a change in these procedures might explain why non-Irish officers made up a larger proportion of the newly promoted sergeants.

Certainly this seems to be the belief of the non-Irish officer himself. All respondents were asked to indicate whether they agreed or disagreed with the statement that "promotions depend more on who you know than on what you know." Nearly two-thirds agreed (this was before the new commissioner's policies took effect), but agreement was higher among European than Irish sergeants. Whereas 56.5 per cent of the Irish officers agreed, 64.0 per cent of the American and 74.5 per cent of the European officers were in agreement. The promotional system could, of course, be regarded as "unfair" in many ways having nothing to do with ethnicity or religion (the department was for many years notorious for the degree of political influence in promotions and assignments).[8] What is striking is the correlation between perceived unfairness and ethnicity. This creates some presumption that ethnic and/or religious discrimination may have contributed to the perception of unfairness.

In fact, however, such evidence as could

TABLE 2

PERCENTAGE OF ALL SERGEANTS AND OF EUROPEAN SERGEANTS NOW SERVING WHO DESCRIBE THEMSELVES AS PROTESTANT, BY DATE OF JOINING THE FORCE

Date of Joining	All Sergeants		European Sergeants	
	N	Per Cent Protestant	N	Per Cent Protestant
Before 1936	107	25.2	31	45.2
1936–40	101	16.8	24	29.2
1941–45	128	15.6	15	20.0
1946–50	260	13.5	24	12.5
1951–61	145	19.3	25	20.0
Total	741*	17.1	119	26.9

* Seventy-seven sergeants did not answer.

[8] Approximately 83 per cent of all officers felt that, before the arrival of the new commissioner, politicians had some or much influence in duty assignments within the department; nearly 80 per cent felt they were influential in promotions; and over 78 per cent felt they were influential in determining who could join the department. Among the politicians in the dominant party there were representatives of virtually every ethnic group in the city, but clearly the Irish were ascendant. Furthermore, many officers believed that the Catholic church was influential in promotions and assignments.

be gathered indicates no systematic promotional bias in favor of Irish Catholics. Table 3 measures roughy the time it took men now sergeants to attain that rank, classified by ethnicity and religion. It shows what proportion of all sergeants who joined the force before 1945 were promoted by 1956, *before* the new commissioner had taken office and his new policies had gone into effect. Presumably, if under the old system promotions were biased in favor of Irish Catholics, a larger proportion of this group than of the non-Irish and non-Catholic would have been promoted by 1956; those penalized during this period would have had to wait until after 1956 to obtain their promotions. As Table 3 indicates, there is no evidence that Irish or Catholic patrolmen made sergeant more rapidly than others; in fact, the reverse tends to be true. It is possible, of course, that had different promotional policies been in effect before 1956 an even larger proportion of non-Irish patrolmen would have made sergeant by then (this would require one to assume that they were, as a group, better qualified for promotion); there is, of course, no evidence either way on that score.

TABLE 3

DATE OF PROMOTION OF SERGEANTS NOW SERVING WHO JOINED FORCE BEFORE 1945, BY ETHNICITY AND RELIGION

	No. Who Joined before 1945	Per Cent Promoted		
		Before 1956	After 1956	Total
Ethnicity:				
Irish.........	93	43.0	57.0	100.0
American	175	48.6	51.4	100.0
European.....	70	55.7	44.3	100.0
Total......	338*	....	....	....
Religion:				
Catholic......	268	46.3	53.7	100.0
Protestant....	66	56.1	43.9	100.0
Other........	11	54.5	45.5	100.0
Total......	345*	....	....	....

* The difference between these totals is accounted for by seven sergeants who answered one question but not the other.

Read another way, of course, Table 3 implies that, although the Irish may be promoted no faster than the non-Irish, they eventually will be promoted. Without knowing the ethnic background of the patrolmen who sought promotion to sergeant, one cannot dismiss this possibility.

A second explanation pertains to the relationship between ethnicity and police work itself. The data suggest that the second-generation Irish sergeant is more likely to come from a "police family," to associate mainly with other police officers, and to have a police officer as his closest friend than either American or European sergeants at any age level. Furthermore, the Irish sergeant is likely to have a somewhat different conception of the rewards of police work. In short, the son of the Irish immigrant is more likely than the son of the European immigrant or native American to have a family background and a set of attitudes that predispose him toward a police career.

As Table 4 shows, Irish sergeants are significantly more likely to have one or more relatives who are also police officers; of those with police relatives, more likely

to have that relative in the immediate family (father, brother, or both); and, of those with policemen in the immediate family, more likely to have their father be an officer. Nearly two-thirds of the Irish sergeants have police relatives and three-fourths of these have them in their immediate family.

Date of entry into the force seems in most cases to make little difference in these relationships. Irish sergeants who joined after 1951 have as many police relatives as those who joined before 1936 (over one-fifth of all Irish sergeants had *three* or more police relatives). The proportion of American sergeants with police relatives also remains roughly constant for all age groups. Among European sergeants, the proportion was fairly constant (and small) until 1951–61, when it decreased substantially.

Not only are the Irish sergeants surrounded with police relatives, but their friends are more likely to be police officers. Nearly one-third (32.6 per cent) of them listed, as their closest friend, another police officer, as compared with only 22.6 per cent of the Americans and 21.8 per cent of the Europeans. When the respondents were asked how frequently they spent much time off duty in the company of other police officers, 30.0 per cent of the Irish sergeants said either daily or once or twice a week; only 16.8 per cent of the Europeans indicated a similar frequency. Most Europeans (53.0 per cent) said that they never spent much time with other officers off duty.

There is, in sum, evidence to support the popular impression that police work occupies a prominent place in Irish-American culture and that, as a consequence, there are likely to be powerful familial and social reinforcements for choosing a police career. If this is true, it ought to affect the kinds of rewards the Irish see in police work—particularly among the youngest officers who presumably have the widest range of alternatives. For many years a principal incentive for police recruits was the security of the job—steady pay and the prospect of a generous pension at a relatively early age. Particularly since the beginning of the postwar period of prosperity, however, other jobs have equaled or excelled police work as a source of both income and security. Steady employment is the rule rather than the exception in most occupations, and pension and retirement plans have spread rapidly to cover a wide variety of jobs.

Police officers still claim that it is security which is the chief reward of police work for most officers—over three-fourths of all sergeants felt that it was the principal incentive *for other officers*. But when asked what was the chief incentive for *themselves*, the proportion mentioning se-

TABLE 4

SERGEANTS HAVING ONE OR MORE RELATIVES WHO ARE POLICE OFFICERS, BY RELATIONSHIP AND FATHER'S NATIVITY

Father's Nativity	All Sergeants	Per Cent with One or More Police Relatives	Sergeants with Police Relatives		
			Total	Per Cent in Immediate Family	Per Cent with Police Father
Irish	190	67.9	130	76.2	29.2
American	439	55.0	241	62.5	24.9
European	120	34.2	39	56.4	15.4
Total	749*	....	410	....	....

* Sixty-nine sergeants did not answer this question.

curity in some form was much lower. And as one might expect, security was mentioned most frequently by sergeants who had joined before 1940. Over three-fourths (76.2 per cent) of the sergeants who joined during the Depression listed security first as an incentive; less than half (48.5 per cent) of those who joined after 1951, during the postwar boom, mentioned it. These generational differences hold true for all ethnic groups. But among the most recently recruited sergeants, the proportion mentioning security is *least* among the Irish (39.3 per cent) and greatest among the Europeans (64.0 per cent). The sons of Irish immigrants, to a much greater extent than the sons of other immigrants, find other satisfactions in a police career—satisfactions having more to do with the nature of the work than with the guaranty it offers. Among the sergeants who joined during the Depression, there were few differences in this regard—almost all, regardless of ethnicity, were preoccupied with security. The Irish claim that they have lost this preoccupation to a greater extent than Europeans, suggesting that other satisfactions have increased (relatively) in importance. What these other satisfactions are must remain a matter of conjecture.[4]

SOME CONSEQUENCES

If the continued presence of second-generation Irish officers in the senior ranks of this big-city police department made no difference in the way the department functioned or in the behavior of its members, there would be little point in investigating why the Irish continue to find police work attractive. In fact, it does seem to make a difference, but not one that can easily be got at through the use of questionnaires and survey methods. What follows, then, is from one of a series of lengthy interviews with persons who occupy top staff and command positions in the department and who are in the best position to observe the most general implications of ethnic differences for the organization as a whole.

When one of the principal departmental executives, a man whose job required him to be concerned with almost all aspects of the force's work and to meet with almost all senior officers, was asked whether he saw any differences in the behavior of Irish and non-Irish officers, he answered emphatically:

> Of course; no question about it. I have often thought, half-seriously, that if you want to change a police department rapidly and effectively by putting new men at the top who will be loyal to the commissioner and do everything by the book and according to standard operating procedures, you could just about throw away the elaborate personnel tests and screening procedures we have devised and simply promote the northern European officers —the Germans, Scandinavians, English, and the like. You would get the same desired result with less money and time.
>
> (Q: Why is that?)
>
> It's hard to explain. It's not that they are necessarily any more honest than the Irish Catholic officers or that they are any smarter. It's more that they have a much greater and more obvious commitment to some set of rules, standards, or general principles as a way of doing and seeing things. The most striking fact about the Irish Catholic command officers in this department is the extent to which they rely on personal loyalties and the exchange of personal favors as a way of doing things. If there is a perfectly legal, routine way of doing something, you can almost be certain that many of your Irish Catholic officers will prefer to do it through some informal means instead. They deliberately step outside the formal system to do things informally. There is often, in fact usually, nothing at all wrong with what they are doing, it is just that they seem to feel more comfortable working through "contacts," intermediaries, and friends. This makes things hard for us at the top, of course. We set up routines and they work around them. You set up a black-or-white situation and the German or Scandinavian officer will

[4] Some observations bearing on this point may be found in James Q. Wilson, "The Police and Their Problems: A Theory," *Public Policy*, Vol. XII (1963), and in a chapter prepared by the author for a forthcoming book on the control of juvenile delinquency, edited by Stanton Wheeler.

see it just that way: an either-or, black-or-white proposition. And he will do it strictly by the book. The Irish Catholic officer seems to find it hard to live with a black-or-white situation; he will muddy it up with qualifications, most of which will be based on considerations of personalities, personal circumstances, and personal loyalties.

(Q: What might be the reason for this?)

Maybe it goes back to Ireland itself. In Ireland, the Irish for seven hundred years had to live with English rule. To live with it, it was necessary to create an informal system of authority to subvert the formal system of authority and even to make it work. Maybe you find the same thing in politics and in police departments.

(Q: Has this led Irish officers to favor other Irish officers in promotions?)

I used to think so. I am not so sure any more. I think that in the past promotions in this department were based on who you knew, not on what you knew. But the "who" referred to individual men, not to "the Irish" as a group. Naturally, because of their associations outside the department, Irishmen were more likely to know other Irishmen. There may have been favoritism, but I don't think it was done strictly along ethnic or religious lines. It was on a personal basis—"I've got a friend who is in line for that job and I owe him a favor and I know I can trust him"—that sort of thing. Of course, politics often entered into the picture, but even that was on the basis of personal contacts, deals, and friendships.

Naturally, the foregoing does not constitute a systematic and verified account of the consequences of Irish prevalence in a police department. But it cannot be ignored for that reason, particularly when these views are widely shared among senior officials of the department. The ultimate question, of course, is whether there is any systematic relationship between Irish culture and the police career, particularly with respect to those attributes just discussed. It is unlikely that such a relationship, even if it existed, could be convincingly documented; nonetheless, there is some evidence from other studies[5] that the Irish, and perhaps also the Italians and Poles, have entered into American institutions in ways quite different from, say the Jews or northern Europeans. Perhaps what is crucial here is the religious affiliation or the pre-immigration rural-urban cleavage.

An alternative explanation, of course, is simply that the Irish in this particular city were the chief participants in (and beneficiaries of) an old-style system of political and police patronage that placed a high value on the maintenance of an informal system of authority based on personal loyalties; once the system is broken and "new Irish" are recruited and socialized into a new set of "professional" norms, the observed connection between "Irishness" and "copness" will disappear, and the Irish will act in a manner indistinguishable from that of the present "minority group" of northern European Protestants and Jews.

In other large American cities, particularly in the West and South, there are few Irish but many police forces which display (or have displayed) some of the same problems as the city here discussed. This may be evidence in support of an organizational as opposed to a cultural explanation of the phenomenon analyzed by the informant quoted earlier. Or it may only suggest that more than one factor may cause the phenomenon.

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[5] See Edward C. Banfield and James Q. Wilson, *City Politics* (Cambridge, Mass.: Harvard University Press, 1963), esp. chaps. v and xvi; Daniel Patrick Moynihan, "The Irish," in Nathan Glazer and Daniel Patrick Moynihan, *Beyond the Melting Pot* (Cambridge, Mass.: M.I.T. Press, 1963); and the description of "rate-busters" on assembly lines in William Foote Whyte, *Money and Motivation* (New York: Harper & Bros., 1955).

THE THREE-GENERATIONS HYPOTHESIS[1]

BERNARD LAZERWITZ AND LOUIS ROWITZ

ABSTRACT

Herberg maintains that the children of immigrants weakened their religious ties while the grandchildren of immigrants exhibit greater religious participation. Lenski, in Detroit, however, found increasing church attendance associated with increasing Americanization for Protestants and Catholics. National sample survey data on Protestants and Catholics support Lenski's Detroit findings. However, when Protestants and Catholics are subdivided by sex and the children-of-immigrants category is confined to people both of whose parents were foreign-born, it is found that (1) Protestants of both sexes show an increasing frequency of church attendance with more generations in the United States; (2) among Catholic men who are children of immigrants church attendance drops; (3) Catholic women show no meaningful changes in church attendance for the various generations. It is thought that these Protestant-Catholic differences stem from the secular orientation of Protestant immigrants.

The grandchildren of our immigrant forebears are returning to their ancestral religious faiths in increasing percentages, declares Will Herberg in his book, *Protestant-Catholic-Jew*.[2] Herberg's thesis starts with the fact that white immigrants formed religio-ethnic communities in the United States as their basic means of adjustment to a new social environment. But members of ethnic groups who retained "old-world ways" or did not speak American English were looked down upon by native Americans and suffered a considerable social handicap. Therefore, the children of these immigrants rapidly abandoned many of the cultural traits of their parents, stopped using their ancestral tongue outside the home (and in some cases even in the home), weakened their affiliations with their fathers' faith, or left it, and in all possible ways sought to be "proper Americans." Herberg goes on to state that the grandchildren of white immigrants, removed from the social handicaps of ethnicity, obviously fully Americanized and frequently middle-class, are free to sort through their cultural inheritance and select this or that trait to be used or developed as one displays ancestral linens, pots, or glassware. In addition, this third generation needs a more vital form of group self-identification, for they now find themselves part of an atomized, emotionally cold, urban-industrial complex. Hence, the ideal thing to do is to reassert their inherited religious faith, to be more active in it, to spell out their salvation through its institutions. There must result a religious revival as the third generation returns to the church to which their parents paid less attention—a church that has been transformed, in brick, stone, and occupant of pulpit, into a middle-class institution.

In 1961 Gerhard Lenski published *The Religious Factor,* based on research in Detroit in which he was able to test Herberg's thesis.[3] Lenski does not find a decline from the first to the second generation in attendance at religious services. Indeed, he reports: (1) there is no dif-

[1] The authors wish to thank the Survey Research Center of the University of Michigan for permission to use its data. Financial support for this project was provided by the University of Illinois Research Board. The research has profited considerably from critical evaluations and comments by Professors Gerhard Lenski, Bernard Farber, and Louis Schneider. Martin Lubin and Peter Melvoin were the statistical assistants. This article is an expanded version of a paper presented at the 1962 convention of the American Sociological Association.

[2] Garden City, N.Y.: Doubleday & Co., 1960.

[3] Garden City, N.Y.: Doubleday & Co., 1961. The testing of Herberg's thesis was but one among several major objectives of this work.

ference between Protestant first- and second-generation groups and an increase in church attendance for the third-generation-or-more group; (2) among Catholics the second generation increases its church attendance over that of the first, and the third-generation-or-more group outstrips the second generation. These differences obtain within both the middle and working classes in Detroit. Hence, Lenski concludes, "our data suggest a pattern of increasing religious activity linked with increasing Americanization."[4]

The generational concepts utilized by Herberg and Lenski were first introduced into the professional literature by the historian Marcus Hansen. In 1938 Hansen, in an address before the Augustana Historical Society, presented the principle of third-generation interest, namely, "what the son wishes to forget, the grandson wishes to remember."[5]

Hansen's concepts were based on personal observations and on his knowledge of the history of immigration to the United States. Neither he nor his recent popularizer, Herberg, has presented any substantial body of data on the actual behavior of the third generation.

Other writers have treated the behavior of the different generational groups. For example, Margaret Mead in discussing the three-generations idea concludes that the third generation comes to identify with the American past so that other men's ancestors become their symbol.[6] Samuel Koenig has written that the second generation is antagonistic toward their foreign-born parents and finds it difficult to be accepted by the dominant society. This second generation and their children try to escape from ethnic ties but never are able completely to do so.[7] John L. Thomas in a discussion of Catholic immigrant groups notes that ties to the Catholic church are continued while ethnic loyalties become less pronounced.[8] Herbert Gans points out that the third-generation American Jew wears his ethnicity rather lightly but does not give up his religious acceptance of Judaism while changing his religion a great deal.[9] These authors think that the third generation does not return to its ancestral culture, but instead continues in the path set by the rebellious second generation and, at best, retains weakened religious ties.

In a recent paper Seymour Lipset maintained that there has been no long-term trend toward increased religious affiliation on the part of Americans nor any post-World War II religious revival.[10] While he does not specifically discuss generational change, he does suggest the view that the various generations might not differ in church attendance.

Again this debate over contemporary third-generation religious conduct has been carried on without sufficient research upon the actual behavior of the various generations. Except for the use of a few national trend statistics or several community studies, writers on generational behavior have relied upon evidence gathered by historians concerned with immigration to the United States.

Two recent national surveys conducted by the Survey Research Center of the University of Michigan ascertained religious preference, frequency of attendance at religious services, and generations in the

[4] *Ibid.*, p. 41.

[5] Marcus Lee Hansen, "The Third Generation in America," *Commentary*, XIV (November, 1952), 492–500.

[6] *And Keep Your Powder Dry* (New York: William Morrow & Co., 1943), chap. iii.

[7] "Second and Third Generation Americans," in Francis J. Brown and Joseph S. Roucek (eds.), *One America* (New York: Prentice-Hall, Inc., 1952), pp. 505–22.

[8] "The New Immigration and Cultural Pluralism," *American Catholic Sociological Review*, XV (December, 1954), 310–22.

[9] "The Future of American Jewry. II," *Commentary*, XXI (June, 1956), 555–63.

[10] Seymour M. Lipset, "Religion in America: What Religious Revival?" *Columbia University Forum*, II (Winter, 1959), 17–21.

United States as part of minor study objectives. Secondary analysis of these survey data permits some light to be cast upon the debate over whether or not the third generation is "returning to the fold." By analyzing the frequency of church attendance among the foreign-born, among the children of foreign-born parents, and among people who are third-generation-or-more Americans, it is possible to see which among the following previously proposed schemes hold:

1. The second generation drops in church attendance while later generations show increases in church attendance. This is the Herberg and Hansen three-generations hypothesis.[11]
2. The generations show a gradual increase in church attendance which is directly associated with length of time in the United States. This is Lenski's finding in Detroit.
3. As suggested by the writings of Mead, Koenig, Thomas, Gans, and Lipset, there is either a continual decline in church attendance from the foreign-born generation throughout the third generation or no meaningful generational difference.

FINDINGS

One of the two surveys was conducted in the spring of 1957[12] and the other in the fall of 1958.[13] Religion was obtained by asking a respondent's religious preference. Generation in the United States was obtained by a series of questions about the respondent's country of birth and the countries of birth of his parents and paternal and maternal grandparents. The classification used for church attendance was "regularly," attends church once a week or more; "often," attends several times a month but less than once a week; "seldom," attends church a few times a year; and "never attends." All data were obtained from white adult respondents twenty-one years of age or over.

Table 1 gives frequency of church attendance within white Protestant and white Catholic groups for the foreign-born, for their children combined with children of one foreign-born and one native-born parent (hereinafter this latter group will be called "mixed parentage"), and for third-

[11] This is a good point to examine the relationship between age and church attendance. Contrary to long-held expectation, recent studies have failed to find socially meaningful changes in church attendance throughout adulthood. Both Orbach and Lenski report no increase in church attendance with aging for Detroit's Protestants and Catholics, and Lazerwitz reports similar findings with national survey data. See Harold L. Orbach, "Aging and Religion," *Geriatrics*, XVI (October, 1961), 530–40; Lenski, *op. cit.*, p. 41; Bernard Lazerwitz, "Some Factors Associated with Variations in Church Attendance," *Social Forces*, XXXIX (May, 1961), 301–9. Some sociologists of religion do not regard these survey data as sufficiently substantiating this lack of association. Indeed, the use of cohort analysis, instead of the standard cross-section approach employed on survey data, might reveal new and interesting patterns of association between aging and religious behavior. Also, designing surveys specifically to obtain religious behavior data would alleviate the contemporary major limitation of working with surveys designed for other purposes. Nevertheless it should be borne in mind that the best evidence now available fails to reveal an association between age and church attendance.

In this study the small size of the foreign-born and second-generation samples prevents adequate controls for age. Nevertheless, as far as the data permit, the generations were separated into under 35 years, 35–55 years, and over 55 years of age. Comparisons of church attendance for the different age groups within the various generations revealed no meaningful differences.

Finally, even if it does exist, a positive association between church attendance and age would imply less frequent church attendance among the third generation than among either the middle-aged second or the elderly foreign-born generation, thus lessening the strength of the relationships reported here but failing to reverse them. It can well be argued that the strong negative emotions of the second generation which lead to such a rapid abandonment of immigrant ways and faiths cannot be overcome later in life.

[12] See Gerald Gurin, Joseph Veroff, and Sheila Feld, *Americans View Their Mental Health* (New York: Basic Books, 1960).

[13] See Warren E. Miller, "The Party and the Representative Process: A Progress Report on Research" (Ann Arbor, Michigan: Institute for Social Research, 1959) (mimeographed).

generation-or-more Americans.[14] The data agree with Lenski's Detroit findings in that there are small increases in regularity of church attendance with each succeeding generation among both Protestants and Catholics. But none of the percentage differences in Table 1 is statistically significant.[15]

TABLE 1

ATTENDANCE AT RELIGIOUS SERVICES FOR WHITE PROTESTANTS AND WHITE CATHOLICS, BY GENERATION IN THE UNITED STATES

GENERATIONS IN UNITED STATES	ATTENDANCE AT SERVICES (PER CENT)					N
	Regularly	Often	Seldom	Never	Total	
Protestants:						
Foreign-born	30	22	39	9	100	87
Native-born of foreign-born or mixed parentage	35	20	39	6	100	321
Three or more generations in U.S.	39	22	31	8	100	2058
Catholics:						
Foreign-born	69	13	13	5	100	131
Native-born of foreign-born or mixed parentage	70	15	11	4	100	321
Three or more generations in U.S.	77	11	9	3	100	359

Since this over-all picture of the Protestant and Catholic generations can hide important variations within these groups, generational differences were explored after additional controls were applied for urbanization, education, occupation, income, and sex.[16] Within the control classifications the Protestant generations show a tendency to reproduce the Lenski pattern and the Catholic generations to follow Herberg's patterning.[17]

Table 2 summarizes the results of all these controls on Protestant and Catholic

[14] Unfortunately, interviews with Jews were too few for detailed analysis. The pattern of the Jewish data is one of decline in synagogue attendance from generation to generation, as Lenski found in Detroit.

[15] Since the samples in this study are multistage, clustered ones, it is necessary to estimate statistical significance by using the formula for the variance of the difference between two-ratio estimators (see Leslie Kish and Irene Hess, "On the Variances of Ratios and of Their Differences in Multi-Stage Samples," *Journal of the American Statistical Association*, LIV [June, 1959], 416–46). Low- and high-level estimates of significance have been obtained. The low-level estimates are based on the simple random-sample formula for the standard error of the difference between two percentages; the high-level estimates include a generalized clustering factor derived from the variances of 96 specific ratio estimators. The high level is 1.4 times the low level. A difference between two percentages of less than twice the low-level estimate is not considered significant. A difference of more than twice the high-level estimate is considered significant at the 95 per cent confidence level. When the difference between two percentages falls between these points, the question of significance is considered unresolved.

[16] The urbanization control consists of classifying respondents as (*a*) residing in cities of 50,000 or more as of 1950 and (*b*) residing in the suburban areas of such cities. Church attendance percentages of the Protestant and Catholic three-generation groups were computed for each residential category. Education is controlled by splitting the Protestant and Catholic generations into subgroups consisting of respondents with less than four years of high school or with at least four years of high school. The occupational control consists of classifying the respondents' family heads as white-collar or blue-collar workers. Income is controlled by dividing the generations into respondents whose family's yearly income was under $5,000 or was $5,000 or more.

[17] Lack of space prevents presentation of these generational percentages. They can be obtained in Louis Rowitz, "The Relationship between the Three Generations Hypothesis and Church Attendance" (unpublished Master's thesis, Department of Sociology, University of Illinois, 1963).

generations. The table gives frequency of generation rank patterns that occurred when the percentages of those regularly attending church for the three generations were ranked within control groups. A 1-2-3 rank pattern, for example, would arise if that among Protestants five out of ten times the third generation has the largest percentage regularly attending services, the second generation the second largest percentage, and the foreign-born group the smallest percentage. While the Protestant third generation ranks first on regular attendance nine times out of ten, there is only a slight tendency, at best, for the sec-

TABLE 2

SUMMARIZED CONTROL-GROUP GENERATION RANK PATTERNS BASED ON REGULARLY ATTENDING SERVICES FOR WHITE PROTESTANTS AND WHITE CATHOLICS

GENERATIONS IN UNITED STATES	SPECIFIC GENERATION RANK PATTERNS							
	Protestants			Catholics				
	(a)	(b)	(c)	(a)	(b)	(c)	(d)	(e)
a) Foreign-born..................	3	2	3	3	2	3	1	1
b) Native-born of foreign-born or mixed parentage................	2	3	1	2	3	1	3	2
c) Three or more generations in U.S.	1	1	2	1	1	2	2	3
No. of times rank pattern occurs....	5	4	1	2	2	1	4	1

regularity of church attendance were highest for the foreign-born generation, next highest for the combined native-born of foreign-born or mixed-parentage groups, and lowest for the native-born of native-born generation. The ten patterns summarized in Table 2 come from forming two subgroups for each of the five controls.

Assuming complete independence among these ten subgroups, any one out of the six possible patterns of rank relations among the three generations would have only a 2.5 per cent chance of being repeated five or more times on a random basis throughout the ten subgroups.[18] Since, the actual relationship among the ten rank patterns lies somewhere between full independence and dependence, a larger number of replications is required to establish the existence of a consistent intergenerational pattern.[19]

In Table 2, rank pattern (*a*) indicates

[18] In the analysis the rank-order patterns were formed for the percentages attending church regularly and attending regularly plus often. Only the rankings for regular attendance are presented here. However, all conclusions derived from "regularly attending" percentages apply to the "regularly" plus "often attending" percentages.

[19] This lack of complete statistical independence can be tackled by use of significance tests described in n. 15. When a sufficiently consistent pattern replication was found, the actual percentages composing the rank relations were checked for significant differences to give added support for the existence of the rank patterning.

Hence, the proof for the existence of an intergenerational rank pattern consists of an adequate number of replications throughout the ten Protestant or Catholic subgroups plus the existence of significant differences among the various pattern percentages. Where the number of interviews is too small to establish significant differences, the number of replications must be high—preferably seven or more. Fortunately, the data presented do fulfil these rules, and it is thought that the existence of consistent differences between generations has been correctly inferred in the subsequent tables.

For a discussion of various means of handling the problem of evidence in secondary analyses of survey data, including those used here, see Richard Curtis and Elton Jackson, "Multiple Indicators in Survey Research," *American Journal of Sociology,* LXVIII (September, 1962), 195–204. See also Ellis B. Page, "Ordered Hypotheses for Multiple Treatments: A Significance Test for Linear Ranks," *Journal of the American Statistical Association,* LVIII (March, 1963), 216–30.

ond-generation grouping to outrank the first. The Catholics repeat patterns (*a*), (*b*), and (*c*) and add two more. In the Catholic section the second generation ranks below the other two six times out of ten, but the first and third generations do not present differentiated rank patterns. Now the summarized Catholic rank patterns seem to show Herberg's hypothesized second-generation dip and do not repeat the Lenski-like pattern of Table 1.

The reasons for this shift in Catholic rank patterns from Tables 1 to 2 can be best seen in Tables 3 and 4, which present generational rank patterns for Protestant and Catholic men and women. While no consistently different generation rank patterns emerge with the application of urbanization or social status controls, clear-cut pattern changes appear when sex is controlled.[20] Table 3 indicates that Protestant men show a very small drop from the for-

[20] Two other controls, age and ethnicity, were attempted, but small sample sizes prevented full analysis. The control for age did not reveal any consistently different rank patternings (see n. 11). The generations within Catholic and Protestant ethnic groups presented equivalent patterns. If respondents with mixed ethnic backgrounds or who are native-born for at least three generations are included, no one generation is disproportionately composed of a single ethnic group. Indeed, among Catholics the important second generation is composed about equally of Irish, Poles, and Italians

TABLE 3

ATTENDANCE AT RELIGIOUS SERVICES FOR WHITE PROTESTANTS, BY GENERATION IN THE UNITED STATES AND SEX

Generations in United States	Attendance at Services (Per Cent)					N
	Regularly	Often	Seldom	Never	Total	
Protestant men:						
Foreign-born	29	18	35	18	100	45
Native-born of foreign-born or mixed parentage	26	20	46	8	100	168
Three or more generations in U.S.	31	23	37	9	100	872
Protestant women:						
Foreign-born	38	24	38	0	100	42
Native-born of foreign-born or mixed parentage	49	20	27	4	100	153
Three or more generations in U.S.	46	21	26	7	100	1181

TABLE 4

ATTENDANCE AT RELIGIOUS SERVICES FOR WHITE CATHOLICS, BY GENERATION IN THE UNITED STATES AND SEX

Generations in United States	Attendance at Services (Per Cent)					N
	Regularly	Often	Seldom	Never	Total	
Catholic men:						
Foreign-born	64	12	15	9	100	65
Native-born of foreign-born or mixed parentage	63	16	16	5	100	145
Three or more generations in U.S.	77	11	11	1	100	158
Catholic women:						
Foreign-born	77	14	9	0	100	65
Native-born of foreign-born or mixed parentage	76	15	6	3	100	176
Three or more generations in U.S.	75	12	8	5	100	201

eign-born to second-generation group and a small rise in the third generation. This is now more akin to Herberg's decline-and-return pattern than to Lenski's Detroit pattern. Protestant women display a sharp second-generation rise and a quite small drop in the third generation. The Protestant women's pattern conforms more to Lenski's Detroit pattern except for its high second-generation level. In Table 4, Catholic men show only a third-generation increase, while Catholic women present no differential pattern among their generations.

In light of the substantial shift in what appeared to be fairly consistent rank patterns, it was thought wise to re-employ education, occupation, and income controls within the religio-sex groups and to compute new rank patterns. These six patterns are presented in Tables 5 and 6.

Again, Table 5 shows a second-generation dip for Protestant men and a second-generation peak for Protestant women. Table 6 repeats the second-generation dip for Catholic men and shows random patterning for Catholic women.

Finally, all respondents with mixed parentage were removed from the second-generation grouping. Table 7 gives the regular attendance percentages for native-born of foreign-born parents in contrast to native-born of mixed parentage. For second-generation Protestant men, regular attendance changes from 26 per cent in Table 3 to 30 per cent for native-born of foreign-born parents. Second-generation Protestant wom-

TABLE 5

SUMMARIZED CONTROL-GROUP GENERATION RANK PATTERNS BASED ON REGULARLY ATTENDING SERVICES FOR WHITE PROTESTANT MEN AND WOMEN

Generations in United States	Specific Generation Rank Patterns					
	Men			Women		
	(a)	(b)	(c)	(a)	(b)	(c)
a) Foreign-born	1	2	1	3	2	1
b) Native-born of foreign-born or mixed parentage	3	3	2	1	1	2
c) Three or more generations in U.S.	2	1	3	2	3	3
No. of times rank pattern occurs	3	2	1	4	1	1

TABLE 6

SUMMARIZED CONTROL-GROUP GENERATION RANK PATTERNS BASED ON REGULARLY ATTENDING SERVICES FOR WHITE CATHOLIC MEN AND WOMEN

Generations in United States	Specific Generation Rank Patterns						
	Men			Women			
	(a)	(b)	(c)	(a)	(b)	(c)	(d)
a) Foreign-born	1	3	2	1	3	1	3
b) Native-born of foreign-born or mixed parentage	3	2	3	3	2	2	1
c) Three or more generations in U.S.	2	1	1	2	1	3	2
No. of times rank pattern occurs	2	1	3	1	2	2	1

en go from 49 per cent to 52 per cent. Similarly, second-generation Catholic men drop from 63 per cent in Table 4 to 58 per cent for native-born of foreign-born parents. Hence, new rank patterns for Protestant men do not show a second-generation dip, but instead have a tendency to rise in the third generation. Protestant women sharpen their second-generation peak. Catholic men deepen their second-generation dip; Catholic women still retain a random patterning.[21]

CONCLUSIONS

The over-all data of Table 1 support Lenski's assertion that increasing Americanization is associated with increasing frequency of attendance at religious services for both Protestants and Catholics. Nevertheless, these religious groups display different generational rank patterns when sex is controlled and respondents of mixed parentage eliminated. Such patterns can best be seen in Tables 3 and 4. First of all, Protestant men have a second-generation dip only because of the low attendance figures for respondents with mixed parentage. If the often-attending percentages are included, the Protestant men show a distinct tendency toward a third-generation rise which falls into the Lenski pattern. Religious attendance for Protestant women is lowest for the foreign-born generation and rises rapidly for subsequent generations. However, the ladies' second-generation peaking does not conform to any previously predicted pattern.

Catholic men display the Herberg pattern. Their second-generation dip deepens when mixed parentage is controlled. On the other hand, Catholic women have no meaningfully consistent intergenerational differences. Hence, Catholic women provide the only supporting data for the third possible church attendance scheme mentioned in the introduction.

Unfortunately, the small sample sizes for most of the various categories and subgroups together with the usually small intergenerational percentage differences almost always prevent the achievement of statistical significance despite adequate numbers of rank pattern replications. The exception to this is the difference between "fully" second-generation Catholic men and third-generation-or-more Catholic men, which is significant at the 95 per cent confidence level for the regularly attending percentages.

While the data clearly show how important it is to control for sex, all previous

TABLE 7

PER CENT OF SECOND-GENERATION WHITE PROTESTANTS AND CATHOLICS ATTENDING CHURCH REGULARLY, BY SEX AND PARENTAGE

Parentage	Protestants		Catholics	
	Men	Women	Men	Women
Native-born of foreign-born parents	30 (82)*	52 (84)	58 (95)	77 (138)
Native-born of mixed parentage	22 (86)	45 (69)	72 (50)	73 (38)

* Figures in parentheses are N bases.

[21] One would expect the percentages of the mixed-parentage groups to fall between those of the third-or-more-generation and the "fully" second-generation groups, since the greater "Americanization" of one parent should move the respondent's church pattern toward that of the third generation. However, the mixed-parentage percentages fail to fall consistently between the second and third generations, even when these groups are further subdivided by whether mother or father was foreign-born, nor do they show any other consistent tendencies.

discussions about generational behavior have failed to consider the possibility of one pattern for men and another for women. Furthermore, neither the theotretical discussions nor Lenski's data have clarified the issue of what to do about respondents one of whose parents is native-born and the other foreign-born. The inclusion of this mixed parentage group with "fully" second-generation people introduces a different pattern for Protestant men and somewhat obscures the patterns for Protestant women and Catholic men.

Youthful immigrants might more properly be considered sociologically second generation, and their children third. Also more precise classification into sociological generations might stabilize the mixed-parentage group and deepen generational differences. That generational definitions are not small problems can be seen by the sizable ratio in Table 7 of the number of fully second-generation interviews to mixed-parentage interviews.[22] While Herberg and Hansen write in terms of pure generational types (adult immigrants, their children, and grandchildren), the presence of large numbers of mixtures of these pure types demands a more sophisticated discussion of generational differences than has yet been presented.

The different religio-sex generational patterns revealed by the summarizing tables of this study may be accounted for by the different sources of Protestant and Catholic immigrants since, say, 1900. Protestant immigrants have come from increasingly highly industrialized countries such as Canada, England, Germany, and Sweden. These countries also have lower levels of church attendance than has the United States (together with their having the customary Christian norm requiring women to attend church more frequently than men). These Protestant immigrants upon arrival are fairly secularized and urbanized. The Americanization process for these people and their descendants involves, among other things, raising their church attendance frequency, generation by generation, until they reach the norm for the rest of the United States. By the second generation, Protestant women appear to have reached this norm; by the third generation the men arrive at it. If rebellion appears, the children of these urbanized immigrants should react by rejecting their parental norm of low church attendance in favor of the higher church-attendance levels characterizing United States Protestants.

On the other hand, Catholic immigrants have been overwhelmingly rural folk from, until quite recently, poorly industrialized areas such as Sicily, Italy, Poland, and Ireland. In addition, their rural norm of church attendance tends to be a fairly high one. Upon arrival in the United States the non-Irish Catholics adjust to the American Catholic norm of even higher church attendance derived from Irish Catholicism. (This the women easily do, apparently, during the first generation.) Also the sheltered girls do not have the requisite freedom for rebellion against their parents or, if they do rebel, their church attendance does not seem to be affected. Catholic males take three generations to adjust to the national Catholic church-attendance norm.

Hence, Lenski's concept of increasing church attendance with increasing Americanization works for Protestants; Herberg's three-generations hypothesis applies to Catholic males. Historically, when Protestant immigrants were rural peasants, perhaps Herberg's hypothesis fitted Prot-

[22] From 1890 to 1950, the ratio of the number of children of foreign-born parents to the number of children of mixed parentage was about 2 to 1 (see William Petersen, *Population* [New York: Macmillan Co., 1961], Table 6-1, p. 116; and U.S. Bureau of the Census, *Nativity and Parentage, Special Reports, United States Census of Population: 1950*, Vol. IV [Washington, D.C.: Government Printing Office, 1954], Part 3, chap. A, Table 1).

A classification scheme for immigrants that tried to meet some of these problems is found in W. Lloyd Warner and Leo Srole, *The Social Systems of American Ethnic Groups* (New Haven, Conn.: Yale University Press, 1947), chap. ii.

estant immigrant males. The question still remains as to whether the Hansen and Herberg thesis ever did apply to immigrant Christian women.[23]

Generations in the United States do not make a good index of the complex interactions arising from growth of the middle class, migration, urbanization, industrialization, lessened ethnic identification, and the impact of science upon religion. The attempts by Herberg and others to picture all the complexities of religious change as a result of or illustrated by generations is an endeavor to use a crude index on too many social forces and does not seem to lead to much in the way of results.

Finally, one might wonder if Herberg's statement that city-dwellers seek to solve their problems arising out of the great degree of anonymity of modern urban life by identifying with religious institutions is not really a view on the place of religion in the United States rather than in urban society in general. Is not the weight of modern life felt equally in western Europe? Then why do we not hear of a return to their churches by European city-dwellers? There are various ways to resolve "urban anomie," and religious affiliation is just one technique among several that can be tried by city-dwellers throughout the industrialized countries of the world.

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[23] The applicability of the Hansen-Herberg hypothesis to Jews has been strongly challenged. See John J. Appel, "Hansen's Third Generation 'Law' and the Origins of the American Jewish Historical Society," *Jewish Social Studies*, XXIII (January, 1961), 3–20.

BOOK REVIEWS

The Conduct of the Corporation. By WILBERT E. MOORE. New York: Random House, 1962. Pp. vii+291.

What criteria are to be used for judging a book that combines penetrating sociological analysis with half-baked moralism, a book that eschews scholarship because it is written primarily for a lay audience but makes many statements about the real world which only a little scholarly activity would show to be in doubt or in need of qualification? How are we to judge a book that is at the same time the best single book by a sociologist about the operation of business corporations in modern society and yet stops short of the author's capabilities because he has decided to join the ranks of the minor satirists of American life?

The book is divided into six parts and twenty-one chapters of uneven quality and length. Part I, "Challenge," deals with the moral dilemmas of modern management and the possibility that there is not sufficient accountability of management. Part II, "Conjugation," is an extended treatment of the typical topics of bureaucratic analysis—the pyramid of power, communication and coordination, the effects and uses of rules. Part III, "Counterpoint," deals to a greater extent with the informal and *sub rosa* aspects of organization, such as the existence of cliques and claques and intra-organizational competition and conflict. Part IV, "Characters," develops the topic of the types of commitment, the orientation toward mobility and careers, and the interest-group orientation of personnel who handle the external relations of the corporation—lawyers, public relations experts, and purchasing agents. Part V, "Change," deals with the fads and fashions in corporate change, the forces inhibiting innovation, and some effects of technological innovation. Finally, Part VI, "Circumscription," analyzes the constraining forces of suppliers of capital and raw materials, labor, consultants, competitors, customers, and the public.

The book is studded with insights and observations that have the ring of truth. Moore's discussion of communication channels focuses, on the one hand, on the *necessity* of bypassing official channels of communication and utilizing lateral communication and, on the other hand, the necessity that an effective system be based on patterned and legitimate expectations of who says what to whom. Again, his discussion of career mobility and the different lengths and starting points of career ladders breaks away from the assumption that all roads can lead to the top.

But in a fundamental sense, Moore *has* deserted us; not because he has written a book without scholarly apparatus, for certainly sociologists should write for lay audiences. But on three counts I find him guilty of leaving behind the duties of the sociological scholar: he has become sloppy about evidence, he has forgotten that one of the principal tasks of the sociologist is to account for variation, not just search for universals, and he has let his moral vision interfere with his analytical skill.

Two examples which appear early in the book (pp. 14–15) are illustrative. Sloppy use of evidence is indicated in the discussion of tension and doubt, of emotional disorder among executives, which Moore attributes to their lack of social accountability. My perusal of research literature indicates that upper executives have a lower rate of coronary disorder, no higher rate of ulcers, and no higher rate of neurosis than lower executives. And there is little evidence to indicate that businessmen differ from the population at large.

Excessive moralism is shown in Moore's discussion of managerial salaries. Because corporate executives get higher salaries than key figures in other institutions (Supreme Court justices, generals), Moore concludes that high salaries represent a kind of legal plunder voted by the board of directors and that they are bad. The whole problem of differential motivation for types of profes-

sions, the extent of competitive pressures for executives, and the problem of staffing the other institutions are shoved aside. (Few corporation presidents retire at age fifty-five while many generals do.)

This same discussion shows his departure from the search for variation. The major variable Moore uses to account for size of salary is size of firm. Yet I am sure Moore is aware that there are eight to ten executives of General Motors who make as much as the president of AT&T, and AT&T makes more money and has a larger capital investment than General Motors. The executive in the oil industry makes more money and has less education than those in the railroad industry. Although both of these examples are confounded by tax laws in different industries and the status of public utilities, an important variable is the amount of profit that can be considered to be dependent on the executives' skills. Throughout this discussion, Moore ignores differences between corporations in different industries.

In his preface, Moore says that he may have been superficial at points in order to get broad coverage. This superficiality mars an otherwise outstanding discussion.

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The Anatomy of Work. By GEORGES FRIEDMANN. New York: Free Press of Glencoe, 1961. Pp. 203. $5.00.

Friedmann's small book begins with a quote from Albert Camus: "Without work all life goes rotten. But when work is soulless life stifles and dies." Thereupon, Friedmann examines developments which have reduced work to a state of soullessness, notes some recent efforts to restore earlier satisfactions, and offers suggestions for achieving solutions to the problem of alienation in modern industrial society.

The logic of Taylorism has been drawn to a point of diminishing returns, argues the author. The continual subdivision of labor has so reduced a large category of jobs to such meaningless, endless repetitive behavior that even engineers and managers are beginning to see that efficiency has become inefficiency, that a happy or satisfied worker is more productive than the most carefully conceived production organization based upon a minute division of labor.

Friedmann decries the extremes of Taylorism which have excessively simplified work so that not only is the worker denied some way to get satisfaction from his job but also the entire society suffers from a kind of anomie which is the reverse of Durkheim's predictions.

Friedmann offers a number of remedies which he sees as promising partial solutions to work-induced alienation. The revaluation of work is one. Job enlargement, work rotation, regular transfer, and what he calls a "polyvalent education" are among them. Such programs would permit the worker to understand what he is doing and thus offer some hope for him to control his work. Presumably there would result a reuniting of planning with production, functions which scientific management seeks to delineate clearly and separately.

The author sees some hope in automation which reduces the number of semiskilled workers and requires more maintenance and repair people. But he cautions his readers to be wary of the utopias which are often predicted by the automation partisans. Vast numbers of jobs, says Friedmann, are likely never to be amenable to automated technologies. In any case, a great deal of research needs to be done before we can know something of the implications automation is having and will have upon workers and work.

But even more than in a revaluation of work, Friedmann sees hope in opportunities for "self-realization and self-development" in non-work or leisure activities. But he asks if the consumption of leisure, at least as it has developed recently in the United States, can yield the satisfactions lost to the division of labor. He is pessimistic unless there are fundamental changes in the entire framework of traditional capitalist production. State socialism à la U.S.S.R. fails to provide a solution because of its centralization and bureaucratization. Friedmann does see promising alternatives in some of the French experiments with "work communities." This "communitarian socialism" is based on flexible and decentralized institutions which harmonize the individual's psychological requirements with modern techniques of group organization and

production. In such work communities "a new value can be given specialized work within a small productive group of which the worker can feel himself a member with full rights, since he is constantly informed of how things are going and is associated with the running of the whole" (p. 158). But Friedmann also notes that this kind of approach can succeed only if supported by an educational system which at all levels stresses a humanistic culture as the base upon which technical education is provided. He is not very optimistic that such a solution will be achieved on any widespread level in the near future.

This is a provocative book, the product of a lifetime spent in investigating the meaning of work in modern society. It should be read by all sociologists, particularly because it assigns to them the task of standing next to, if not replacing altogether, the power-holders who will ultimately reorganize the production systems.

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The Retail Clerks. By MICHAEL HARRINGTON. New York: John Wiley & Sons, 1962. Pp. x+99.

This paperback, "one of a series of studies of comparative union governments edited by Walter Galenson for the Center for the Study of Democratic Institutions," is a case study of a multi-industrial union which combines blue-collar organization and technique and white-collar ideology; it describes the proletarianization of the white-collar workers who account for the spectacular rise of the Retail Clerks International Association, now one of the ten largest unions in the United States. The proletarianization can be seen in the distribution of RCIA membership (heavy concentration in food and drug chains), trends in membership base (more clerks in large establishments, more men, fewer expecting quick promotion), and the erosion of the personal relationship (from sales talk to check-out counter and charge plate, from customer contact to materials handling). Although the lag of clerical income behind blue-collar income has now been reversed and the ideology of service persists, a substantial minority of retail workers are impoverished and a "more rationalized, semi-skilled, and depersonalized work process" is producing a union-prone white-collar man. Note these topical headings in a workbook for organizers: "the loss of relation to the product; the growing division of labor; alienation for white-collar workers; work as a means to an end" (p. 84).

Harrington argues that industry structure, not community environment, is the crucial determinant of union structure which, together with the type of leadership, decides success. In southern Illinois, "under good union conditions (cohesion, loyalty, acceptance by the community . . .), the RCIA local [is] economically backward" (p. 58). Elsewhere, as in Los Angeles, the new distribution system—huge self-service supermarkets and discount houses—gives the union its opportunity in otherwise hostile environments. Industry structure also explains variations in success vis-à-vis rival unions. Because of the development of powerful locals in food chains, a tendency toward more inclusive units, and the related effectiveness of such techniques as the boycott, RCIA has been able to fight off the teamsters; the "strategic alliance" and the dependent local, so prominent in the early history of this union, have declined.

The story about union government is uncomfortably familiar: a union faced by internal dissension, hostile employers, and rival unions centralizes; a modern leader, President Suffridge, upon accession to power centralizes further, maintaining unchallenged hegemony since 1944 through performance (a vigorous organizing drive, expansion of staff services) and constitutional provision (powers to fire staff, fix salaries of vice-presidents, appoint convention committees, and put locals into trusteeship).

Many writers have been struck with the popularity of the combination of paternalism, oligarchy, and efficient service in ostensibly democratic organizations. Harrington sees this as the dilemma of welfare unionism versus democracy, points to the need for structural reform, but adds little to the literature on this problem. He is impressed by the Los Angeles local—tightly run, effective in its grievance procedures, tough in bargaining (the 1958–59 strike added psychiatric care to an already large package of welfare benefits). There is no question, the author says, that the members back the De Silva adminis-

tration. They are "well cared for and provided with union assistance in almost every contingency." The one thing they lack is "a means of really active participation" in the decisions affecting them (p. 52). Nationally, too, in Harrington's judgment, the membership is now satisfied; unity is so great that it has never been necessary to crush internal opposition (p. 82). He concludes, "A democratic union has no responsibility to manufacture opposition; it does have the obligation to provide structural safeguards for opposition" (p. 21), and he recommends that vice-presidential compensation be fixed in the constitution and that a public review board be established to give substance to rights of appeal (p. 88).

Although marred by some obviously hasty prose (in unfortunate contrast to *The Other America*) and by superficial analysis of the problem of union democracy, this book describes a case strategic for a grasp of trends in stratification and unionism. Students of organizations, work, and politics will find it useful.

HAROLD L. WILENSKY

University of California, Berkeley

The American Worker in the Twentieth Century: A History through Autobiographies. By ELI GINZBERG and HYMAN BERMAN. New York: Free Press of Glencoe, 1963. Pp. 368. $7.50.

This is an unusual book because it attempts to depict the American working class as a whole and in historical perspective. Studies of American workers are almost always limited to one occupation or community, and we have lacked the equivalent of Hoggart's *Uses of Literacy* or Zweig's *The British Worker* because the common features of an American working-class subculture have been overridden by profound ethnic, regional, and industrial diversity. While remaining sensitive to the problems of generalizing about our heterogeneous working class, Ginzberg, an economist, and Berman, a historian, have attempted this difficult task and made an important contribution to the social history of the American worker.

The authors examine the "quality of lives" led by American workers in three periods, the years from 1890 to 1910 which were marked by the growth of heavy industry and the influx of an immigrant labor force, the years from 1918 to 1939 which saw the extremes of prosperity and depression, and the recent postwar era. They let the workers speak for themselves: more than two-thirds of the volume consists of ninety-three personal documents, composed by workingmen in a wide variety of occupations. These fragments from life histories, published autobiographies, magazine articles, government commission hearings, and research interviews are the most successful feature of the book and are extremely well chosen to convey the mood of each period. They express in language often as readable as literature the daily preoccupations of ordinary men and women, the nature of their tasks, their problems with exploitative employers, their living conditions in boarding houses, company towns, and suburbs, and their hopes and aspirations for the future. From the responses of workers to their daily problems and anxieties, we get a sense of major historical trends: the reduction of hours of work, the virtual elimination of unbridled exploitation and petty favoritism, the improvement of standards of living—in short, the long-range process by which the American working class has moved from alienation and separation toward integration into the larger social order.

Excellent as these personal documents are, their place in the over-all analysis raises a number of problems. A kind of undisciplined impressionistic empiricism results from the sheer quantity of the case materials and from the way they follow one another. These documents demand more critical interpretation than the authors give, and the significance of the subject matter requires more historical generalization. The chapter that synthesizes the entire seventy-year period is only fifteen pages long and could have been written by many knowledgeable students of American industry without access to the rich human materials, the raw data of this book.

The major question is the representativeness of the case materials. It is curious that the authors do not even include a paragraph on the scientific problems involved in generalizing from historical documents, although they do present a long (and I feel unneces-

sary) justification for their study in terms of the gaps in previous industrial research. Yet why expect that workers who wrote books, contributed articles to the liberal press, or testified before government investigating commissions were representative of the working population of their day? Certainly these articulate workers described conditions that were experienced also by others. But as a group the articulate ones were more socially conscious, more favorable to unions, and more personally ambitious than their more taciturn comrades. From this kind of sample Ginzberg and Berman have drawn generalizations (e.g., that *many* workers at the turn of the century were ambitious enough to go to night school) the accuracy of which should be checked through the use of other data, such as actual night-school enrolment records.

Personal documents also need careful and precise analysis in order to provide historical generalizations. Through techniques of content analysis we could test the hypothesis that industrial workers see less hope for advancement within the firm today than in the past and determine how the workers' characteristic grievances on the job have changed over time. My impressionistic reading of *The American Worker* suggests that earlier dissatisfactions over long hours, low wages, and corrupt and exploiting employers have been replaced by complaints about the pace of production, the standards of output, and the lack of autonomy and responsibility in much mass-production work.

A few minor criticisms: Ginzberg and Berman may be too optimistic in their emphasis on the affluence of today's working class. Only two paragraphs are devoted to the lower 20 per cent of the labor force still living in poverty. The lack of footnoting in the brief interpretive chapters is disconcerting, especially when one wants to question statements that are not self-evident, for example the ellegation that favoritism is still widespread in the filling of supervisory positions. And the authors' inevitably cursory discussion of previous research should at least have referred to the studies of Hughes, Walker, Gouldner, and Chinoy—particularly the latter, a number of whose findings and interpretations appear in the text. But despite these weaknesses this is a most important and successful book. The human documents often capture beautifully the impact of the social organization of the economy and of the work place on the personal experiences of the manual worker.

ROBERT BLAUNER

University of California, Berkeley

Industrial Participation, Theory and Practice: A Case Study. By J. A. BANKS. Liverpool: University Press, 1963. Pp. 150.

This book is a research monograph attempting to explain workers' interest in promotion to the rank of supervisor or in serving as joint consultation representative or shop steward. The research was carried out in a medium-sized firm in the seed-crushing and animal-foodstuffs industry. The firm was a subsidiary of the Unilever Organization with workers represented by the Union of Shop, Distributive, and Allied Workers. Data were secured through interviews with the workers at the work place and from records provided by the firm and the union.

Statistical analysis followed three lines of division: (1) workers indifferent to promotion to the rank of supervisor or to becoming shop steward or joint consultation representative, or some combination of these, as compared with workers interested in these offices; (2) workers interested in promotion to the rank of supervisor, irrespective of their other interests, as compared with workers not interested in promotion; and (3) workers interested in *voluntary* office, that is, in being elected shop steward and/or joint consultation representative, as compared with those not interested in such offices.

The extent of participation was as follows: Sixty per cent of the workers were interested neither in promotion nor in voluntary office, 32 per cent were interested in promotion, 15 per cent were interested in the office of joint consultation representative, and 15 per cent were interested in the office of shop steward. These figures add up to more than 100 per cent because they refer to different sets of men, and some men were interested in more than one kind of participation.

Four factors of the in-plant situation were selected as likely to explain the data: (1) the relationship between the men and their supervisor, (2) the relationship between the

men and those responsible for the control and administration of departments; (3) the technical demands of the process in which the men are engaged; (4) the customary practice in some departments of filling joint consultation posts with shop stewards, or by independent election of the joint consultation representative.

The author concludes that the influence of technical organization is small compared with interpersonal factors created by formal and informal relations. He believes that the influence of the departmental administration can be crucial and that the practical conclusion to be drawn is that measures to encourage attitudes of democratic leadership on the part of management will eventually result in greater participation among the workers.

This study is another careful product of the Liverpool Social Science Research Group which now has a decade of experience and a record of research investigations including dock workers, steel workers, clerks, managers, and many community studies. This report makes it clear that a desire for industrial participation is no great mass yearning. At least in the plant studied, where a long tradition of joint consultation exists, only a minority are interested—even taking a broad definition of participation. This is not to say that it is not important, but that a zeal for democratic participation is as limited within a work place as in a democratic society generally. Even the same principle seems to apply—the more skilled the job, the greater the likelihood of participation.

In spite of some concrete findings in this pioneering study, the research team was unable to find any significant effects due to technical factors. It is not demonstrated whether this lack of significance is a real one or is merely the result of faulty research design. As only one factory was studied, there is little variety. Also there is no indication that the research team was familiar with Leonard Sayles' work on *The Behavior of Industrial Work Groups,* which might have had a profound effect on the design of the study.

The isolation of other factors is also shown to be most difficult. On page 125 the author states that the influence of the departmental administration can be crucial in developing interest in participation. On page 144 one analysis shows no significance between departments on this factor when day workers are compared. Other calculations on pp. 144–46 leave the reader with doubts as to the direction of the findings.

DELBERT C. MILLER

Indiana University

The Corporation and Its Publics: Essays on the Corporate Image. Edited by JOHN W. RILEY, JR. New York: John Wiley & Sons, 1963. Pp. xiii+195. $5.50.

If the patent medicine men of Madison Avenue have tarnished the concept of "corporate image," this book does much to restore its luster. Treating the topic as analogous to the individual's problems of self-image and social reputation, this symposium volume underscores the importance of corporate identity in any society which relies heavily on corporate action.

The corporation's image among its publics is in part a function of the nature of those publics. Kingsley Davis insightfully traces some implications of changing population composition and structure for public attitudes toward corporations and also for corporate behavior. Helen Dinerman, examining the American corporation abroad, digs beneath differences in tradition to explore ongoing developments and reactions to the American corporation as a participant in those developments. Observing that typical measurements in America reveal a bland image of the corporation, Herbert Hyman indicates that this may be an artifact of populational rather than ecological sampling. He and Reuben Cohen offer suggestions for improved methods.

That the corporation is influenced by what people believe it does as well as says is observed by several authors, and Robert S. Lee finds that public evaluation of corporations as introducers of automation is indeed complex. Gerhart D. Wiebe raises questions about corporate nurturing of the society from which it draws its sustenance, and Robert O. Carlson warns that the firm which wants to be better known must be ready to answer some searching questions.

Riley and Marguerite F. Levy contribute

a pregnant typology of approaches to the development of a corporate image, and Riley notes that all of the chapters call attention to the importance of the theory of corporate enterprise. If that theory had been recognized as a special case of general organization theory, the symposium might have profited from notions such as Perrow's distinction between "intrinsic" and "extrinsic" prestige, or Levine and White's concepts of organizational "domain."

But if the symposium missed an opportunity to draw an organization theory, it gave Kenneth Boulding an opportunity to contribute to organization theory. Boulding selects six variables, about which mountains have been written, and lucidly and skilfully spins a host of exciting propositions. Anyone seriously interested in organization theory will find the book profitable and Boulding's chapter mandatory.

JAMES D. THOMPSON
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Man, Work and Society. Edited by SIGMUND NOSOW and WILLIAM H. FORM. New York: Basic Books, Inc., 1962. Pp. viii+612. $8.50.

The editors of this anthology have assembled sixty-eight selections and presented them in fifteen sections with such headings as "The Meaning of Work," "Economic, Industrial and Occupational Systems," "Industrial Change and Occupational Trends," and so forth. The vast majority of the selections are abridgments from longer writings. Each section is preceded by a brief introduction (usually running around three pages) by the editors. The book includes an extensive bibliography (thirty pages) of recent articles and books on the sociology of work.

Nosow and Form express the belief that "the choice of readings reflects a response to a coherent frame of reference which is detailed in the introductions to the chapters." Their editorial strategy, however, has resulted in enormous variety in theoretical perspectives and specific empirical concerns, and it would take both a conceptual frame of great generality and devilish ingenuity to unify that variety. There are excerpts from broadly conceived histories and reports on limited empirical studies; there are observations based on personal experience and systematic quantitative studies; there are articles by economists, anthropologists, psychologists (several schools of thought represented), sociologists, and journalists. The content of the fifteen introductions cannot be said to present a frame of reference that truly orders this great diversity.

If one withholds the claim to conceptual unity, however, variety can be a strength rather than a weakness. The editors have made, in this reviewer's opinion, judicious selection from within an expanding field, and one will find many important ideas in these pages. Most of the selections are worth at least a rapid reading, and although one can always quibble about omissions (especially, in this case, of non-American writers), the general balance seems to be good. The abridgments will irritate in some instances, but the over-all result is that this volume combines in one place, for the first time, an adequate sampling of the range of concepts, ideas, and data that make up the present state of knowledge in the sociology of work. It will prove of value, therefore, to those who have a serious interest in this field.

The instructional uses of the volume, however, are more problematic. The high price leads one to pause before recommending it for purchase by students unless it is to occupy a central place in the course readings. Instructors vary, I suspect, in their strategies of breadth versus intensity, and those who want students exposed to relatively brief accounts of many topics will find this reader useful. For such instructors, this book simplifies assignment of readings which might otherwise be difficult to obtain, but the brevity of the individual readings might well confuse rather than enlighten some students who would benefit from fewer, more fully developed statements.

The high price, however, may be a necessary concomitant of the care which the publisher has shown in the making of the book. The print is large and legible, there are relatively few obvious typographical errors, and a satisfactory name index is included (subject references are limited). Statistical tables, graphs, and charts have also contributed to the cost of the volume and to its general clarity. The bibliography of recent work is

very helpful to those of us working in the area. Within the limits imposed by their editorial strategy, both the editors and publishers have done a careful and useful piece of work.

DAN C. LORTIE

University of Chicago

Conflict and Defense: A General Theory. By KENNETH E. BOULDING. New York: Harper & Bros., 1962. Pp. ix+349. $7.00.

This book is not easy to review. On the one hand it is unmistakably the work of a brilliant man, erudite, original, charming, witty, engaged—a man who has a genuine "feel" for what social science is about and for how it can be used to shed light on the most important problems related to human conflict. As an exercise in synthesis it commands enormous respect. But—here enters the ambivalence—exactly what does the new contribution consist in?

Anybody who ventures into interdisciplinary work is bound to provoke some reaction from workers in the disciplines who feel that basic assumptions or perspectives or variables have been neglected. In general this kind of criticism tells more about the provincialism of the specialist, but Kenneth Boulding the economist at times sounds strange as a sociologist. His societies somehow do not have social structure: there are no status networks, no communication networks; class and mobility play a minor role. Society is presented as a struggle between two parties for one thing or the other, and the parties are presented in a relatively flat typology: individuals, groups, or organizations. That makes for, in principle, three kinds of conflicts between parties of the same kind and three kinds between parties of different kinds, which are presented in the five middle chapters of the book.

The first four chapters are devoted to four theoretical approaches to conflict: static models, Richardson processes, game theory, and a theory of viability which is the theoretical nucleus of the book. In these extremely lucid chapters Boulding presents in a highly original way some of the most important tools of mathematical analysis in social science, of course particularly in modern economics. In his "theory of viability," derived from the theory of competition of firms, a number of interesting conclusions are drawn, such as the conflict strategy of making one's self as similar as possible to the other party (as opposed to dissimilarity inherent in the traditional polarization model), seen in the fact that "five-and-ten's cluster on Main Street, churches on Church Row, and steel mills in Pittsburgh and even that, in a wider field, Methodists get to be indistinguishable from Baptists and all ciders are too much alike" (p. 75).

In the last part of the book the general theory is applied to special aspects of conflict such as economic conflict, industrial conflict, international conflict, and ideological conflict. The reader will enjoy it because Boulding always has something important to say. Nevertheless, at times one may feel that the conclusions do not justify the relatively technical (at least relative to standard "sociologese") apparatus. To take an example: viability is defined as a kind of autonomy; the party "cannot be absorbed or destroyed as an independent source of decisions." For the theory of competitions between firms the price of a product at the place of production and the costs of transportation are major factors in determining the area of viability. In international conflict the analogy is made with "home strength" and "loss of strength gradient"; in modern terms, your autonomy depends (1) on how many megatons you have and (2) on how far you can transport them and with what efficiency. Unconditional viability, then, depends on whether the party is "stronger than the other at home." One agrees, and one also agrees with the basic conclusion arrived at (pp. 241–42): "The recipe for stability is to have high cost of transport of violence, countries a long way apart, and rapidly diminishing efficiency with increase of scale. It is because of a failure of all three of these conditions that we face an acute breakdown of the system of national defense in the world of today." Without belittling the importance of a systematic derivation of the known, in his next book. we want Boulding not only to derive some of our present known nightmares, but also to devote his brilliance to the derivation of

very new insights and the construction of viable worlds, given the reality of these nightmares. In this respect the last chapter, the epilogue, is disappointing however much one may sympathize with the author.

There is no reason to go into more detail, for the basic force and basic weakness of the book are clear enough. Boulding's method is partly deductive, mainly heuristic. There is hardly a social phenomenon he is not able to catch and turn into a diagram that somehow makes the phenomenon look manageable theoretically. On page 30 appears, probably for the first time in human history, an equilibrium analysis of the relations between saints, publicans, and regenerate sinners; and conditions under which the saint becomes "continually more saintly and the devil continually more devilish" are spelled out. Some of it is so close to operationalization that it could be subjected to rigorous testing, which is never done in the book. Boulding proceeds by way of illustrations and insights more than by systematic evidence. In this he is more in the tradition of economic than sociological analysis: his models are true as models, almost apodictic, and their relations to social reality are not spelled out. They are abstractions, ideal cases in a kind of social vacuum. But instead of this becoming an argument against Boulding it becomes an argument for him. In sociology we have thousands of empirical studies with varying soundness in theory and methodology. Boulding's book stands out against this ocean of knowledge about social reality as a reservoir of heuristically good models that may inspire empirical research. It is in this spirit that the book should be read and enjoyed, not as a set of neatly ordered and confirmed hypotheses.

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Applications of Graph Theory to Group Structure. By CLAUDE FLAMENT. Translated by MAURICE PINARD, RAYMOND BRETON, and FERNAND FONTAINE. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 142. $6.95.

We are told by the experts on marriage and the family that successful marriages require common interests; and, as an observer of the rather stormy honeymoon of mathematics and sociology, I strongly suspect that the same principle applies to intellectual unions. Without wanting to appear a gossip, I am forced to admit a suspicion that sociology courted mathematics more in the hope of upward mobility in the intellectual community than because they had much in common.

Part of the problem, of course, stems from the mathematical illiteracy of us sociologists, but another part seems to come from an odd empirical phenomenon, that sociologists just do not think in ways that fit the bulk of the easily available mathematics. Rather than being concerned with variables, increases, decreases, and changes, sociologists tend to think about structures, patterns, and qualities of relationships between individuals or groups. The result has been that, while a large amount of mathematical sociology has been produced, it tends to treat new problems and issues rather than to help sociologists to think more clearly about their old ones.

Claude Flament's book constitutes a conspicuous exception to these generalizations, and as such it is a potentially important book, because despite the misleading name (graph theory has nothing to do with graphs in a spatial sense), the mathematics of graph theory is precisely the formal analysis of relationships and their structures. In point of fact, the application of graph theory to sociology is not totally new. Frank Harary and his collaborators have published applications in the general area of group dynamics for a decade, and graph theoretical theorems on sociometric problems were published by Festinger, Luce, and others more than a decade ago. At the same time, this book makes a unique contribution. Part naturally stems from new theorems, but much comes from the simple fact that by covering a wide range of sociological applications (clique structures, communications experiments of the Bavelas-Leavitt type, the theory of structural balance, and Lévi-Strauss's analysis of kinship) Flament convinces us that graph theory is not just a mathematical model of limited social situations but a language for treating a wide variety of sociological problems.

The book is divided into three parts. Chapter i develops the concepts of graph theory from set theoretical notions. Chapter ii presents theorems and algorithms for analyzing communications structures of the Bavelas-Leavitt genre. Chapter iii treats the Heider-Cartwright-Harary theory of structural balance, with particular attention to changes over time in an unbalanced structure moving toward balance. In the latter chapter a number of quite remarkable theorems are developed showing that a rather complex graph can be analyzed in terms of a single point and its triangles.

Certainly the sociologist concerned with communication nets and structural balance will have this book on his "must" list, but, even more important, any sociologist who wishes to treat the phrase "social structure" as meaning something specific rather than a rhetorical ornament in his prose, should read this book.

It is only fair to warn, however, that this is a difficult book to read. Oddly enough, it is not because of the mathematics. One needs no more math than the set concepts developed in the first few pages to handle it. However, Flament has a terse style which makes R. A. Fisher seem like Thomas Wolfe. I study best by underlining the key points, and there are many pages in my copy in which I have underlined complete paragraphs. In addition the translation is literal rather than smooth, and, while some of the strange wordings merely add piquancy (e.g., "the following properties are equivalent to characterize a graph"), in some places the meaning is lost. For example, it took me half an hour to figure out that "That adjacent graphs do not differ by their degrees of unbalance is attested" really means "That adjacent graphs do not *necessarily* differ *in* their degrees of unbalance is attested." This example on page 111 and a jumbling of the letters in the formulas on pages 96 and 97 are the only really serious problems I found, but I must admit that I managed to complete the 126 pages of text in this book only by daily half-hour sessions over a period of about a month. Let me add that they were my most profitable half-hours that month.

JAMES A. DAVIS

Johns Hopkins University

A Stochastic Theory of Social Contacts: A Laboratory Study and an Application to Sociometry. By KULLERVO RAINIO. ("Transactions of the Westermarck Society," Vol. VIII.) Copenhagen: Munksgaard, 1962. Pp. 103.

This monograph is an important work, both for what it attempts and what it accomplishes. The studies reported are an experimental test and a further empirical application of a formal model developed in the author's previous work, *A Stochastic Model of Social Interaction.* To this reviewer, the monograph represents a unique marriage of theory construction and experimentation with more orthodox sociometric investigation.

Rainio concerns himself with two substantively distinct processes: the process of choosing social contacts and the process of opinion change resulting from social contact. He regards these processes, however, as formally similar; that is, both can be represented by the same formal system, namely, "a simplified version of Bush and Mosteller's two operator learning model." The models for the two processes differ only in the empirical identifications of the elements of the mathematical system; for example, the quantity p is interpreted as "the probability of making a contact" in one case and as "the probability of holding a certain opinion" in the second. The basic rationale for considering these two processes in the same terms arises from the author's view of social interaction as "a process derived from the learning of the individuals participating in it."

The principal assumptions in these two models include the following: (1) a contact occurs between a pair of individuals, (2) each individual in the contact experiences the behavior of the other as A or not A, (3) this experience is always either rewarding or punishing to the individual, and (4) the probability of behaving in the same manner (either choosing the same contact or holding the same opinion) increases with reward and decreases with punishment according to the two equations in the Bush-Mosteller learning model.

The author devised an experiment to capture these elements of the interaction process. Although in general he would regard these processes as hypothetical and not observable,

he has designed his experiment to make them overt and observable rather than inferential. Once Rainio has tested the goodness of fit of the model to his experimental data, he returns to using the model inferentially to describe the behavior of a set of sociometric choices taken at three six-week time intervals. Here he employs a computer to simulate what would happen if the model were an adequate description of the underlying process, that is, he generates theoretical data which he compares with the observations from sociometric questionnaires.

The experiment and the sociometric study are not, however, two independent investigations. The experimental findings, for example the numerical estimates of the model's parameters, enter directly into the simulation of the sociometric observations. Furthermore, the results of applying the model to sociometry suggest modifications of both the model and the original experiment. This promising interplay may turn what is now a baseline model (that is, a model whose assumptions are only minimally substantive) into a theory with greater substantive import.

It is intriguing, however, that this baseline model with its admittedly unrealistic assumptions can provide an adequate description not only of experimental data but also of much less controlled questionnaire results. Although the application to sociometry is quite crude and somewhat *ad hoc*, Rainio's results should give pause to those who rail against simplification and abstraction as destroying the essence of a phenomenon. Indeed, this reviewer believes that one feature of the model designed to make it more realistic causes more trouble than it is worth, at least in the experimental situation. Allowing the constants which change the behavior probabilities to vary from individual to individual is "more realistic" but, from the data, does not seem to be necessary and clearly complicates the problems of estimation. The experiment certainly could be modified to eliminate any necessity for this assumption.

The strong points of this work also contain some of its weaknesses. The author wrestles with many significant problems of model-building but perhaps does not give adequate attention to any one of these problems. At least his report in many places is so elliptical as to be difficult to understand. The difficulty for the reader lies not in any mathematics (which is minimal) but in the overly concise discussion of the issues involved in analyzing the data. It is unfortunate that this lack of clarity together with the inadequate English of the translation will limit the audience for this highly interesting work.

BERNARD P. COHEN
Stanford University

Notes on Technology and the Moral Order. By ALVIN W. GOULDNER and RICHARD A. PETERSON. Indianapolis, Ind.: Bobbs-Merrill Co., 1962. Pp. xvi+96. $1.95.

The purpose of this monograph is to locate the main variables by which the social structure and culture of societies can be described (by factor analysis) and to assess the causal priority of these variables, with special attention to the relative explanatory power of technology, kinship structure, and the organization of morality (again largely by factor analysis). The two parts of the purpose are to make the causal assessment and to see whether factor analysis works for making such assessments. The data are previously published ratings of seventy-one societies on fifty-nine variables from the Human Relations Area Files. The authors conclude that, in explaining variations among social structures, the level of technology is most important, followed closely by a variable they call "Apollonianism," which involves stratification items (such as the existence of chiefs, judges, organized priesthoods, governing councils) and characteristics of social control (ceremony and ritual, codified laws, promise of future life, no marriage by capture).

The data are fascinating and the speculations based on the analysis are provocative and strike me as sensible. Complete publication of the zero-order correlation matrix and the stages of factor analysis will enable other scholars to use the data for their own purposes. Aside from the factors of level of technology and "Apollonianism," which bear the main brunt of the analysis, the authors name a lineality factor (patrilineal-matrilineal) and a sex-dominance factor (males

powerful versus equality). Some of the other factors seem to have conceptual meaning: a "degree of fixity of settlement" factor (V-4, O-4); an "intensity of real-estate use and private property" factor (V-8, O-7); a "commercial" factor (V-9, O-3); and a "post-marital sex separation" factor (V-5, O-2). The numbers refer to the orthogonal (V) and oblique (O) factor matrixes published in the appendixes; the names are my interpretation, not the authors'. Scholars for whom these variables ring a bell will find a good part of their analysis already done for them, but unexploited, in the appendixes. But the book is proposed as a methodological essay on the use of factor analysis in causal inference. The evaluation of this, unfortunately, requires fairly extended discussion.

The basic model of factor analysis assumes that all the items are caused by underlying factors. In particular, it assumes that the items themselves have no causal interrelatons independent of being common effects ("indicators") of the underlying variable. Statistically this is expressed by manipulating the correlation matrix so that when the underlying factors are located, the partial correlations among the items, controlling for the basic factors, will be near zero. This is the common interpretation of factor analytic results, as indicated by the notion that the problem of interpretation is naming the factors. Another rarely used imagery is that factor analysis is not a technique of multivariate analysis at all, but merely a device for reducing a large group of numbers to a small group of numbers which is, in certain respects, mathematically equivalent and which is then analyzed by whatever multivariate techniques are appropriate. The usual causal model makes good sense in psychometric measurement, where we deliberately construct the items so that they are not causally interdependent—so that the correct answer to one question does not cause a man to be better able to answer the next. But when the data are naturally occurring variables, the assumptions are problematic.

To take an egregious example from Gouldner and Peterson's monograph, do we really think that the prevalence of agriculture is causally unconnected to the use of grain for food? Does it make good sense in this case to look for a hypothetical underlying variable which will reduce the partial correlation of these items to zero? Can anyone seriously propose that growing grain is only "spuriously" related to eating grain, and that if we only knew the true causal structure the items would turn out to be unrelated under controls? The same logic would lead us to explain the correlation between temperature on the ground and the angle of the sun at noon by an underlying "summer-winter" factor.

I will try to show that this faulty causal image creates difficulties by giving reasons from the literature for believing that three of the "factors" found are effects of one of their common items, namely the presence or absence of agriculture. Many of the highest zero-order correlations are between the existence of agriculture and other variables. But agriculture turns out to have relatively low loadings on three factors: "degree of fixity of settlement," "intensity of real-estate use and private property," and "technological level." But (1) it has been argued by historians, who have temporal ordering to help assess causal ordering, that the development of agriculture causes fixity of settlement, and practical men who want to settle nomads in the Middle East, central Asia, and Africa start agriculture to cause nomads to settle; (2) an increase in the intensity of use of communally held land by introducing cultivation has, historically, led to its appropriation as private property in England (in the inclosure movement), the United States (the settlement of the frontier), and Latin America; in the United States much of our herding, hunting, and gathering lands are still communally held, but agricultural land has become private property; and (3) it has often been argued that various kinds of elite services, including technological expertise, require settled agriculture as a necessary condition: certainly practical men have found it difficult to bring elite services (government, education, priestly services) to nomads, which is one of the reasons governments try to settle them.

What the literature suggests, then, is that agriculture *causes* items on all three "factors." These items may indeed be assumed to have zero partials among themselves after the

causal effect of agriculture is controlled but the partials between agriculture and each item cannot be asumed to be zero. Examination of the zero-order correlations shows that the above theory is consistent with the facts as given, without computing the partials. If this theoretical tradition is right, it does not make good sense to consider agriculture as merely a rather bad indicator of three basic variables and to divide up its variance between them.

If, from previous theory, knowledge of temporal order, or characteristics of the patterns of partial correlations, we can infer a causal structure among the items different from that assumed in factor analysis, this structure should be extracted first. For instance, it would make good sense to extract the variance attributable to agriculture first, and then to factor-analyze the remainder matrix to see whether that suggests other causal orderings. This is equivalent to specifying in advance one of the basic vectors of the factor space, that is, a "factor" rotated so as to have a loading of 1.00 on agriculture (or with a loading equal to its communality, if the rotation is done after factors have been extracted by standard techniques). In fact, inspection of the factor matrix shows that four other items are almost collinear with agriculture, that is, they would have large loadings on the factor created by the item "agriculture" and low loadings on other factors (items 1, 2, 12, and 54). Thus the proposed rotation makes sense. Causal assessments should be made before factor analysis is carried out in order to choose the appropriate treatment of the correlation matrix. Different causal models will give rise to different "factor analyses."

But my capacity to carry out analyses on different causal assumptions and to obtain sensible results is a reflection of the richness of the data in the monograph and the amount of simplification in multivariate analysis which factor analysis yields as a device for eliminating redundant numbers in a correlation matrix. The combination of rich data and interesting theory makes this book much more important than its size would indicate. Everyone should read it.

ARTHUR L. STINCHCOMBE

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Outlines of Sociology. By LUDWIG GUMPLOWICZ. Edited with an Introduction and Notes by IRVING L. HOROWITZ. New York: Paine-Whitman Publishers, 1963. Pp. 336.

This is a reprint, with some revision of the text and translation by the present editor, of a short work first published in English by the American Academy of Political and Social Science in 1899, after the first German edition of 1885. Gumplowicz issued a revised German edition in 1905, and Professor Horowitz has attempted to bring the original translation by Frederick W. Moore into conformity with the latter German edition.

Ludwig Gumplowicz (1838–1909) was among the important sociologists of the second generation, perhaps, as the present editor contends, a figure of somewhat greater importance than the attention paid to his work in this country would indicate. Albion Small gave considerable publicity to the contribution of Gustav Ratzenhofer through the amount of space he devoted to a résumé of Ratzenhofer's ideas in *General Sociology*, but, except for the use of a short, crucial passage from Gumplowicz's *Der Rassenkampf* (probably his most significant work) in Park and Burgess' *Introduction to the Science of Sociology* (1921, 1924), nothing comparable was done to acquaint American students with the work of Gumplowicz. The first American edition of *Outlines of Sociology* has, of course, been long out of print and obtainable only in libraries. It is doubtless a service, therefore, to have this new edition of the work made available.

The value of Gumplowicz's sociology is limited for many present-day students of the subject by his emphasis on the "polygenetic hypothesis"—the separate origin of the various races of man from prehuman ancestry—and on a deterministic philosophical monism as a presupposition of all sociological inquiry. It is not clear to the present reviewer that either of these assumptions had much relevance for the points that Gumplowicz made as contributions to the substance of a general sociology. He emphasized conflict as a social process of great significance and defined the group as the fundamental social fact.

Unfortunately in the present edition both the main translated text and the editor's long Introduction are marred by numerous

errors in proofreading; and the language of the Introduction is so peculiar that the editor's intention is sometimes difficult to make out. He makes curious use of quotations from other authors (as on pp. 11–12, 16–17, 22–23); this hardly seems a good use of space.

FLOYD N. HOUSE

January Hill
Ivy, Virginia

Einführung in die soziologische Theorie ("Introduction to Sociological Theory"). By PETER HEINTZ. Stuttgart: Ferdinand Enke Verlag, 1962. Pp. 275.

The title of this volume, which grew out of a series of lectures first given in Chile, is seriously misleading. This book is not an introduction to general sociological theory; in fact such theory is not considered at all. It consists rather of a series of loosely connected essays on such topics as power and prestige, fashions, technological progress, primitive law, modern bureaucracy, culture conflict, rural-urban migration, juvenile delinquency, and the marginal man. A number of shrewd observations can be found in most of these essays, yet as a whole these read more like chapters from a high-level introductory text than like efforts to generate new knowledge.

American sociologists have been pleased in recent years by the extent to which their contributions have found their way into the work of our German colleagues; such receptivity has seemed an indication of declining parochialism in German social science. But this volume, so it would seem to me, shows that there can be too much of a good thing. We know about overconformity; it now appears that there is also overreceptivity. Most of these essays are based on American materials; in fact the author shows an impressive command of the current American literature. Unfortunately, however, in the effort to assimilate the American sociological tradition he seems to have forgotten his own. He writes on fashion, mentioning all sorts of American contributions, but does not mention Simmel. He discusses the personality of city dwellers but again fails to mention Simmel. He writes on power and considers Max Weber only incidentally while spending pages reporting American small-group researches on leadership. He discusses "Feudal Remnants in Bourgeois Society" without mentioning Schumpeter. When he refers to Ralf Dahrendorf, he cites the American translation of his work on class conflict rather than the German original.

The author's ethnocentrism in reverse is perhaps best illustrated by his discussion of the prestige of bureaucracy in modern society. He claims that "especially the governmental bureaucracy has relatively little social prestige; its prestige does not correspond to the power it commands directly or indirectly." And he asserts further that high-status groups are "generally disinclined to accept bureaucratic positions, even in the high and highest ranks." Had this been written by an American sociologist, one would put it down as an instance of American provincialism, of a tendency to draw unwarranted general conclusions from limited American data. But coming from a European sociologist this is truly amazing. The high prestige of the upper levels of civil servants in such countries as Germany or England is obvious to anyone who has spent even a short time in these countries. In the German prestige rankings quoted in the well-known Inkeles and Rossi study, high civil servants rank fourth among some forty occupational categories; and any English observer would be surprised by the assertion that members of the upper class in his country are not attracted by upper-echelon positions in the civil service. One can only wonder how German readers will react to assertions that would seem to be so obviously in conflict with empirical reality. Such foreign-culturebound approaches can hardly contribute to the sociological self-understanding of German society.

Certain of these essays, those on the modern family and on the middle classes, for example, are excellent summaries of the current state of research and thinking in these areas. They can be recommended on this score.

LEWIS A. COSER

Brandeis University

Ideology and Society. By DONALD MACRAE. New York: Free Press of Glencoe, 1962. Pp. xii+231. $4.50.

This book contains essays published or read at various meetings during the period 1950–60. Part I contains essays dealing with the development of sociology in Great Britain. Part II deals with class, race, and Darwinism. Part III deals with political ideology and contains sociological vignettes of Lassalle, Lord Acton, the Webbs, Sartre's existentialism, and probably the best essay of the book on "The Bolshevik Ideology," first published in 1951, remarkable for that period, and developing the concept—first encountered in Raymond Aron and Jules Monnerot—of "secular religion."

These essays are easily read. They are genteel, often witty, with an occasional Latin citation and excerpts from Dryden, Tennyson, Balzac, Zola, and Diderot; but one cannot advocate that a professional sociologist should drop what he is doing to read them unless he has reached that mixture of superego satisfaction and quiet desperation that the correcting of sixty blue books may have engendered in his soul. The academic returnees that one sees drifting to our universities for postgraduate degrees might read them with profit as a general introduction to the field of sociology.

This reviewer found these essays often frustrating because they dabble in an amateurish vein with ideas that would seem to deserve more thorough handling, but here English customs may have decided the amount of thoroughness that was good form. It is interesting to speculate as to why sociology has had such a difficult time establishing itself in Europe considering indeed that, as MacRae reminds us, most of the intellectual capital of sociology comes from Europe (although ranking Hobhouse with Durkheim and Max Weber would seem to stretch the point beyond what polite ethnocentrism might require). There were, in 1960, 40 sociologists in British universities as against 350 economists. No doubt, anthropology is a much more serious competitior in Britain. Its cataloguing and collecting habits, its adventurous field work, its benevolent concern with the natives who were (and still remain) the white man's burden, permitted an easier assimilation to Oxbridge patterns. (Archaeology is even nicer.) Sociology asks impolite questions of Englishmen rather than reserving them for the Ashanti. It is (or was) a London, Redbrick adoption of something essentially "American," and full of clever, dark, little men. Although he touches on almost all of these ideas, I wish that MacRae had developed some of them, to sustain or refute them rather than stress, besides the handicap of female do-goodism, the losses of World War I as an explanatory factor. It is pious to mention that the best ones died, but is it good sociology? Is sociology destined to be "twentieth-century radicalism," or is it the God-given soft subject for the hordes of lower-middle-class students invading the state universities in order to share in the good life and secure their passport to the better white-collar jobs?

Professor MacRae reminds us, among other useful things (the British middle-class counterrevolution between 1945 and 1955; Lord Monboddo's anticipation of Darwin), of the debt that functionalists owe to Spencer, and that Morris Ginsberg was apparently the "inventor" of the concept "quasigroup" which has seen recent service in the analysis of juvenile delinquency. All in all, pleasant reading, with a few gems to be noted for future reference.

JESSE PITTS

Wayne State University

Racial Themes in Southern Rhodesia. By CYRIL A. ROGERS and C. FRANTZ. New Haven, Conn.: Yale University Press, 1962. Pp. xviii+427.

This book is a welcome addition to the literature on racial prejudice. It indicates that the patterns of attitudes we have come to think of as standard in situations of discrimination in the United States are to be found in almost boring reptition in Southern Rhodesia.

Rogers and Frantz asked of a random sample of the white population of Southern Rhodesia their attitudes to sixty-six practices of racial discrimination sanctioned in the colony by law or custom. Their purpose was to find out not only the range of attitudes of the European community (highly concentrated on the "conservative" end of the spectrum) but also the correlations with twelve personal and social characteristics.

They find first of all that the European

population is determined to maintain certain patterns more than others, in particular those relating to sexual relations and social and recreational facilities. They are relatively most willing to envisage change (in the direction of diminishing discrimination) in areas of legal justice and educational and training facilities.

In comparing subpopulations they find significant differences along lines that would be expected by those with a knowledge of Southern Rhodesia, according to political party preference, national or ethnic origin, religious affiliation, country of birth, occupation, type of industry, length of residence. They find that the best over-all predictor of race attitudes is length of schooling, the more highly educated group being more willing to diminish discriminatory practices. They find a statistically less significant correlation of attitudes with income, census district, age, and sex, although even here there is a consistency of higher scoring along the expected lines (lower income, older persons, and women showing somewhat more prejudice). They point out further that the statistically more significant correlations "point to real rather than artificial groups."

The data then are useful, if not very surprising, and they are compiled with sufficient care that one can credit them with high reliability. The presentation of the data is less happy than their collection. The authors are so marked by their effort to overcome the resistance to scientific inquiry of a population that identifies it with radicalism—some interesting press comments on the survey are reproduced in an appendix—that they lean over backwards to explain their objectivity to the reader. This leads them into such exercises as giving brief courses on statistical methods, explaining such difficult terms as "hierarchy," and otherwise boring the scientific audience for what I suspect are fruitless and perhaps incomprehensible excursuses for the non-specialist. Furthermore, the style tends to make even the more interesting findings dull. Jargon such as: "the immigrants had come from societies that were late recipients of the impulses of civilization emanating principally from the eastern Mediterranean region"—I presume this means that they are Westerners—detracts from scientific clarity. Last, the occasional remarks on the larger political context are sometimes naïve and could be misleading.

Nevertheless, this book is to be recommended for two reasons: it provides the most systematic documentation of racial attitudes of whites in southern Africa presently available; it confirms and specifies a number of lower-level generalizations previously developed about racial prejudice.

Immanuel Wallerstein

Columbia University

Negro and White Youth: A Psychological Study in a Border-State Community. By Albert J. Lott and Bernice E. Lott. New York: Holt, Rinehart & Winston, 1963. Pp. iv+236. $5.00.

Although the Negro American has hardly been slighted by social science, competent, broadly gauged research on Negro personality is surprisingly scarce. This modest monograph is an initial attempt to fill this need.

The authors report data on 116 Negro and 185 white high-school seniors. The sample was drawn from four virtually uniracial schools in Kentucky, two rural and two urban. Sampling biases as to school selection are not discussed. Three standard psychometric instruments—a Goal Preference Inventory (to measure needs for academic recognition, social recognition, and love and affection), a simplified form of the Allport-Vernon-Lindzey Study of Values (to measure Spranger's six basic values), and projective measures of both needs for achievement and affiliation—plus a background questionnaire provided the central data; interviews were held with a special group of fifty-eight leaders.

In spite of a distinctly lower socioeconomic standing, the Negro sample revealed a value- and need-structure remarkably similar to that of the white sample: religious and social concerns rated highest, aesthetic lowest. Some differences do appear, however. Negroes on the average scored lower on need achievement and proved more oriented toward security than self-expression—both trends consistent with their predominantly working-class status. Of special interest, given the sit-in movement which soon followed the completion of this study, is the Negro youth's

outlook on the future. In general, he was more optimistic than the white youth; yet he shrewdly appraised his position in society, his better chances for white-collar jobs in the North, and his need to end discriminatory barriers.

Several caveats are in order. The selection of the sample, the small number of cases, and the narrow range of measures and methods severely limit the value of the research. As the authors themselves note, their approach was preliminary and descriptive, rather than a testing of hypotheses; and they make no effective effort to untangle the joint effects of class and caste. The unimaginative analysis is especially frustrating. For instance, there was far less sex differentiation in interests among Negroes than among whites—a finding the authors interpret as a function of the greater absence of fathers in the Negro homes. Yet no comparisons within the Negro group are presented that test whether in fact the father-absent students reveal less sex differentiation (as they do in other research).

Nevertheless, this little book heralds two encouraging trends. First, it marks a renewed interest in a neglected realm of great national significance. Second, it adds to the growing number of brief research monographs marketed by major publishers. It is to be hoped that future volumes of this type will appear in less expensive paperback editions, enabling undergraduate teachers to assign such works as required supplementary reading.

THOMAS F. PETTIGREW

Harvard University

Ethnic Patterns in American Cities. By STANLEY LIEBERSON. New York: Free Press of Glencoe, 1963. Pp. 230. $4.95.

This book is a competent and useful study of ethnic segregation in ten American cities, for ten ethnic groups in each city, as revealed by data from the censuses of 1910, 1920, 1930, and 1950. The main content of the study is the analysis of segregation indexes comparing the various immigrant and second-generation ethnic groups and contrasting them with the native white population and with the native white of native white parentage, respectively. The main finding is that all of these ethnic groups (defined by European nationalities) have experienced substantial decreases in segregation over the time period—evidence of assimilation—while the rank order of segregation among ethnic groups has remained stable.

The remainder of the study is an effort to throw further light on the assimilation process. Major factors considered are length of residence, education, housing costs, mother tongue, citizenship, and occupation.

Length of residence can be measured in the 1930 census directly, both as median year of arrival and as per cent arriving after 1900. It has a strong correlation with segregation from native whites, but this relation is greatly reduced when education is taken into account. The "old-new" immigrant distinction also bears on length of residence. The "new" immigrants are far more segregated than the "old" immigrants (from northwestern Europe and Germany). However, censuses of Boston in 1850, 1855, and 1880 reveal that the old immigrants had similar experiences in their initial period of settlement.

A cute demonstration that housing costs do not account for the ethnic distribution is given for Cleveland (Table 23); however, the less segregated the ethnic group the more similar the housing cost pattern to that of the native white, save for the peculiarity that immigrants are in excess as home owners (of low-value homes).

Decentralization of an ethnic group is associated with declining segregation save for the existence of industrial, or working-class, suburbs.

Some ambiguous comparisons of ethnic groups with Negroes are presented.

The persistence of foreign languages is associated with segregation independently of length of residence. However, citizenship is taken up more rapidly by "new" immigrants when time of arrival is taken into account (p. 146).

Analysis of occupational segregation reveals a pattern similar to residential segregation.

The greatest methodological question mark in this study is the heavy reliance on indexes proposed by Duncan. The statistical properties of these indexes have never been studied in detail. In consequence we have no way of

deciding whether a given difference is small or large, especially in Tables 30 and 31 on centralization. Also, the discussion of Table 25 is unclear, while Table 33 is incomprehensible.

The explicit theoretical content of the study consists of the usual loose metaphors from Adam Smith and Charles Darwin that please human ecologists; for example, differences in occupational distributions are explained by "competition."

The implicit theory, however, is a very reasonable effort to use notions of diffusion theory. The contact mechanism in diffusion is often brought into the detailed discussion. Rather clumsy yet pertinent efforts to use decision theory are also evident ("propensities" are used in place of "utilities").

The main value of the study is the assembling of the data from a process point of view; a systematic diffusion theory can be constructed from these empirical guide lines. Mathematical sociologists with an interest in the contact mechanism will be rewarded by detailed study of this book.

JAMES M. BESHERS

Massachusetts Institute of Technology

Urban Growth Dynamics in a Regional Cluster of Cities. Edited by F. STUART CHAPIN, JR., and SHIRLEY F. WEISS. New York: John Wiley & Sons, 1962. Pp. x+484. $8.95.

If one had any naïve expectations about proportionality between the input of research time and effort and the output of significant findings, this book could only dampen his optimism. As the fruit of several years' work on the part of a score or more of social scientists, the book succeeds in reaffirming some old knowledge and in indicating that that knowledge applies descriptively to the five cities of the Piedmont crescent in North Carolina. Whether their work has brought the authors any closer to the formulation of important questions about the nature of urban development than they were at the outset is not revealed.

The twelve research papers which constitute the substance of the book are, according to the Introduction, cast in a common framework. That framework, if I may paraphrase it while retaining the editors' italics, is as follows: *Urban development* is a product of urban *value systems* motivating *behavior patterns* which, in turn, are guided by *control processes* toward predetermined goals. This "schema" (a word defined in my dictionary to mean "diagram, plan, or scheme") is presumably addressed to the study of change. But "urban growth dynamics" is a rather misleading phrase, for the research papers have little to say on the subject. Oddly enough, the historical dimension appears in only three papers, and there it spans no more than a decade. The remaining papers report cross-sectional analyses of economic interactions among cities, of correlations of agricultural with urban variables, of the roles of planners, business leaders and Negroes, of the attitudes of recent migrants and other residents to city livability, and of land-use patterns. These, say the editors, deal not with dynamics but with micro-dynamics, causal factors that have been arrested for observation through the microscope of research techniques. How they operate, what forces they generate and the interactions among them appear to be topics for later investigation.

While most of the papers are tedious recitations of survey findings, two stand out as provocative and useful contributions to the literature. The chapter by Ralph W. Phouts on "Patterns of Economic Interaction in the Crescent" develops some unique data and subjects them to a gravity-model analysis in a competent manner. A second, "Political Decision Making in Arcadia," by Benjamin Walter, is a fascinating documentation of the events culminating in three political decisions in a particular city and a searching analysis of the data with reference to their bearing on the role of the expert and on current thinking regarding community power.

AMOS H. HAWLEY

University of Michigan

The Sociology of Urban Regions. By ALVIN BOSKOFF. New York: Appleton-Century-Crofts, 1962. Pp. xiv+370. $6.00.

This textbook presents the standard, elementary information that is normally contained in basic courses in urban sociology. Demographic and ecological facts are pre-

sented first, then various facts about urban social organization such as those pertaining to voluntary associations and social stratification.

The author sprinkles among his recitations of factual information various classifications, some borrowed, some presumably more or less original. The teacher using this book as a text might be somewhat embarrassed about asking his students to learn the classifications because, once presented, they are usually ignored even by the author. The alert reader will at once perceive the elegance of the author's position in this respect.

The basic facts are in this book. They are simply presented. They are also to be found in other textbooks.

THEODORE R. ANDERSON

University of Iowa

Married Women Working. By PEARL JEPHCOTT with NANCY SEARS and JOHN H. SMITH. Foreword by RICHARD TITMUSS. London: George Allen & Unwin, 1962. Pp. 208. $7.00.

This is a study of working wives in the London working-class borough of Bermondsey. It began in 1954 with a survey of married women workers in a biscuit factory located in the area, a firm employing 5,000 workers with women on part-time shifts constituting 46 per cent of all its operatives. Following this came a survey of a sample of 465 Bermondsey households aimed "to provide a balanced picture of married women's employment in one area." It was estimated that 52 per cent of married women in the borough were working, about one-half of them part time. Additional information about children of working and non-working mothers was obtained from the records of various social agencies such as schools, Probation Service, Public Health Department, and others.

Although not by design this turned out to be a "success story," a case study of wives working under exceptionally favorable conditions. Severe labor shortage put pressure upon the biscuit factory and other firms in the area to recruit women with domestic responsibilities by providing a variety of part-time shifts. The stable population and the presence of relatives in the neighborhood eased practical problems created by employment. The authors describe the local provision for the leisure-time activities of children as unusually generous. The long tradition of working mothers in the area has provided both a favorable climate of opinion and techniques of coping with domestic responsibilities. As long as the children were not neglected, the working mother in Bermondsey enjoyed respect as one "who'll put herself out for the kids' sake."

Finally, the working mother was not typically the mother of very young children. Only 24 per cent of women in their twenties, 50 per cent of women in their thirties, and 73 per cent of those aged forty to forty-nine were employed. All of this naturally poses the question: Which of the alleged problems caused by the employment of mothers do or do not persist under such favorable conditions? The rich data of the book go a long way to supply the answer.

As far as the factory was concerned the employment of a large number of part-time married women obviously proved economically feasible, but at a cost. Absenteeism was high and the system of flexible shifts created special problems of administration. Labor turnover was high in the early months of employment. Once trained for the job, the part-time worker proved neither less nor more efficient that the full-time worker.

For the crucial question of the effect of the mother's employment upon children, the authors turned to the records of social agencies and the opinions of teachers, health visitors, and the play-center staff. In health, school attendance, and school progress the children of working mothers appeared to be under no disadvantage. The authors concluded that standards of homemaking also did not suffer from the women's employment. The major domestic help came from the husband, and the extra earnings were used to improve the family's standard of living and especially to provide advantages for the children. The authors made no attempt to study the subtler psychological implications of job-holding for parental or marriage relationships. They do cite their impressions of a closer partnership and an increase in joint decision-making in these marriages with two earners. On the debit side is the loss of leisure and, for some women, the physical strain of the double job.

The book as a whole illustrates what a

general social survey, not guided by theory, can and cannot teach us. It tells us much that is interesting and important. It can guide social action to remedy some existing problems. But the limitations of this type of study may be conveyed by the impression that, apart from some census-type facts, the researchers discovered little that is not known to the residents of Bermondsey. To cite but one example, there is nothing in the book to match the sophisticated current studies of the effect of the wife's employment upon her power in marriage under a variety of conditions. A theoretically focused study can reveal relationships which escape the perception of the layman.

MIRRA KOMAROVSKY

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Columbia University

Aging in Minnesota. Edited by ARNOLD M. ROSE. Minneapolis: University of Minnesota Press, 1963. Pp. vii+320. $6.75.

This book declares itself to be "a report of the Minnesota planning committee for the White House Conference on Aging," but its heartwood is a remarkable 116-page essay by an eighty-year-old woman, Aldena Carlson Thomason, entitled "We Who Are Elderly." Mrs. Thomason sets herself the task of interpreting old age for us—its needs, its pleasures, and its pains—and in this she is remarkably successful. On first reading her essay, one feels that to understand fully all its content and flavor would be to know almost everything that is worth knowing about being old. But doubts strike. The strange combination of intricate, concrete detail and bold generalization—is this an old mind at work? Or was she always like that? And the evocative descriptions of the mixture of freedom and frustration that age brings? Is this the "seductive combination of increased wisdom and decaying powers" that E. M. Forster describes? Or is this only Mrs. Thomason's old age? It is impossible to tell.

Nevertheless, in spite of being only one case, this particular essay suggests a hundred ideas, and for this reason it is a major contribution to an area not noted for fresh new themes. Furthermore, it offers some wry rebukes to those who patronize the old: "Younger people are generally greatly impressed by what they feel must be the dreadful dullness of old age. Most of us can remember when we shared this feeling." That Rose can say, some dozen pages later, that "elderly people have a surfeit of time and little knowledge of how to use it" suggests that gerontology does not always listen when old age speaks.

None of the remainder of the book reaches Mrs. Thomason's standard. It is divided among several authors. Bernard E. Nash and Ethel McClure describe the history of Minnesota's legislation for the welfare of the aged. Nash describes the organization of a five-county demonstration project in service to the aged. His chapter probably belongs in that body of literature known to social workers as "community organization." Isaac L. Hoffman contributes an inventory of Minnesota's older population, describing their health and welfare status, and Marvin J. Taves and Gary D. Hansen report the marginals from a questionnaire survey of a sample of older people. Arnold Rose concludes the book with his opinion of what should be done for old people in the future.

A theme that dominates this book is one that dominated the White House Conference itself. Old people need medical care and adequate incomes. As Mrs. Thomason says, "We have now perhaps said enough to convince ourselves and others that for us who are elderly, *security and health* [author's italics] are our greatest needs, and that if we are not further robbed or deprived of our well-earned measure of security, and if science can help us in maintaining health, we ought to be, and probably shall be, largely able to satisfy our other needs ourselves."

M. ELAINE CUMMING

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Gerontology: A Book of Readings. By CLAUDE B. VEDDER. Springfield, Ill.: Charles C Thomas, 1963. Pp. xv+430. $9.50.

In the last two decades gerontology has become part of the professional establishment in this country. Three professional journals are now devoted to reporting research on aging. Local, regional, national, and inter-

national institutes and symposiums focusing on human aging have become common. Three substantial handbooks surveying developments within the field have appeared. And, if further evidence that gerontology is here to stay is needed, one may note the current concerns among gerontologists to recruit and train other gerontologists and to divide the field into subspecialities. This book opens with a brief review of these developments.

Professor Vedder's book of readings presumably is meant to introduce the uninitiated to human aging as a scientific and social problem. The readings are collected in seven chapters entitled (1) "Development of Genrontology," (2) "The Sociology of Aging," (3) "Demographic Characteristics of the Aged," (4) "Economic Aspects of the Aged," (5) "Health Problems of the Aged," (6) "Recreation, Politics and Crime," and (7) "Community Responsibility and Research."

As an introduction to gerontology, the book has several limitations. The editor has neither offered an overview of this complex field nor attempted to interpret and relate material in the various chapters. Unfortunately, the title of the book invites the inference that these readings outline the field of gerontology. More accurately these readings focus on *social* gerontology as indicated by the attention given to this subspecialty in the opening chapter and by the readings selected.

An unfortunate consequence follows from the failure of the editor to provide background and transitional material: important issues raised by specific articles are either masked or oversimplified. This is illustrated, for example, by Havighurst's article, "Successful Living," which discusses briefly the theoretical and methodological issues surrounding disengagement theory. There is much more to be said about these issues than any brief article could possibly say, although the uninitiated reader would get only a hint of this. Moreover, the relative merits of longitudinal and cross-section research in aging receive only passing reference. This omission, while not in itself crucial, points up the lack of attention given in this book to methodological problems, even though part of one chapter is devoted to research.

For the person seriously interested in gerontology, the existing handbooks (*Handbook of Aging in the Individual: Psychological and Biological Aspects; Handbook of Social Gerontology: Societal Aspects of Aging; Aging in Western Societies*) would provide a more accurate and informative introduction to the field. Vedder's book would probably be most useful as a source of supplementary readings for undergraduates. Its greatest usefulness would be in the context of a course that provides the background and interpretative information necessary to put these readings in perspective.

GEORGE MADDOX

Duke University

The Funnies: An American Idiom. Edited by DAVID M. WHITE and ROBERT H. ABEL. New York: Free Press of Glencoe, 1963. Pp. 304. $7.50.

The comics reach one hundred million readers daily. They appear in almost every newspaper in the country. People follow the actors' lives and careers and "miss" their favorites. The comic strip is well institutionalized, has a vital existence and a secure future. Since the comics are a part of American life and such an intimate feature, one would think that the analysis of the world of the comic strip would uncover great gobs of understanding about American society. In this respect the professional reader will find *The Funnies* a disappointment. For the educated layman, the book is worth his time. Both professional and layman can enjoy the appropriate illustrations. An extensive bibliography is appended.

More than two-thirds of the articles in *The Funnies* are reprinted from literary and academic journals and are readily available in their original form. The authors represent collectively the social sciences, arts, literature, and the creators of comic strips. All in all, little of what is said advances our understanding of the mass arts. What the authors say of the comics has been said of movies, magazine fiction, radio serials, and television. The comics, as the other mass arts, reflect American tastes and values, involve the reader in vicarious experiences, simplify and stereotype the "real" world, reflect changing values and concerns, occasionally teach the reader, propagandize and moralize, and are not necessarily funny. The average comic-

strip reader is fairly well educated yet considers the comics to be low-brow and escapist. He is vaguely ashamed of his daily perusal of comics but finds fun and pleasure in reading them.

Two articles are of special interest to the professional. Frank E. Barcus' study, "The World of Sunday Comics," investigates the goals of and means employed by comic-strip characters, the effectiveness of the means, and the marital, occupational and ethnic status of the characters; it contains interesting cross-tabulations. Gerhart Saenger's "Male-Female Relations in the American Comic Strip" reveals a pattern of male-female role reversal. The male is emotional, excitable, less intelligent, and shorter than the female.

For this reviewer, the best statement made on the subject of comics and humor is in a witty and wise essay by Al Capp, creator of "Li'l Abner." An understanding of the funnies must at least coincide with an appreciation of the elements of humor. Capp provides that appreciation.

KENNETH KESSIN

Illinois Institute of Technology

Old Societies and New States: The Quest for Modernity in Asia and Africa. Edited by CLIFFORD GEERTZ. New York: Free Press of Glencoe, 1963. Pp. viii+310. $6.95.

This book is a sign of the times in several ways. It results from the efforts of a committee (the University of Chicago Committee for the Comparative Study of New Nations) composed of scholars from several disciplines whose interests merge in the topic of the comparative analysis of social and political development in the states achieving national sovereignty since 1945.

The first chapter, written by Edward Shils, contains an urbane, sophisticated, yet deceptively simple discussion of the new states' condition, hope, and place in the world, a statement of the proper comparative study of new states as defined by the aspirations of this group of social scientists, and a persuasive summary of the importance of such a study. Shils's orientation remains macrosociological, antirelativistic, universalistic, humane, cultivated, and comparative—with respect to historical as well as contemporary human societies. There is much that is laudatory in this, and one wishes that such a view were reflected in more sociological work. But the references Shils makes to "social research" and the "best traditions of contemporary social science," even though they may accurately describe other work of the committee, are misleading as an introduction to these efforts.

Only one of the eight chapters contains a systematic presentation and analysis of data. In this chapter, Mary Jean Bowman and C. Arnold Anderson reach several interesting conclusions and challenge some widely accepted assumptions about the role of education in development. This will be of interest to many readers. However, the data are admittedly crude and there is at least one figure which contains an error (Fig. 10, p. 270).

The remaining chapters are essays. Although I am in complete sympathy with the aims and efforts of the authors, I must conclude that the essays seldom succeed in achieving the promise contained in Shils's introductory chapter. The essays are often too sweeping in their generalizations and too wide in their range of application. They are occasionally repetitious, frequently contain a rehash of the synthesizing ideas of other writers (e.g., Tocqueville and Weber), sometimes are vague and use jargon unnecessarily; and, when dealing with big ideas and whole nations in a few words, they often become platitudinous.

Nevertheless, the exercise is far from a total loss. Most of the authors are deeply knowledgeable about some particular area or areas outside of North America and western Europe, and each has had his share of insights and new formulations as he expanded his map of the social world to include other areas that he knew less about before the committee was formed. For example, David E. Apter, well known for his original research in Ghana and Uganda, considers a large number of societies at one point or another in his essay including Guinea, Mali, China, Indonesia, the Soviet Union, Japan, South Africa, and France. He discusses the factors underlying the nature of the changing political systems in some of the new states, his thesis being that latent instabilities in the development process in several of the new states cannot be resolved either by democracy or totalitarianism, but that new theocracies are

emerging based on what he calls "political religion." Refreshingly, Apter does not compromise the meaning of democracy in his discussion and he calls a spade a spade, but at several points in his essay he does permit his functional analysis of undemocratic trends to come dangerously close to a justification of the actions of undemocratic leaders.

McKim Marriott has contributed a piece on the cultural policies of some of the new states which is of particular interest because of its emphasis on the problematic and volitional features of cultural development. Viewing culture as something which can be managed as a result of conscious planning alongside the other "arts of the possible" such as economic policy, military strategy, and politics, he explores the various cultural policies actually chosen by a selected number of newly independent states.

Some of the problems of social integration in Indonesia, Malaya, Burma, India, Lebanon, Morocco, and Nigeria are insightfully discussed by Clifford Geertz. The interrelationships between equality, modernity, and democracy in the new states are the subject of Lloyd Fallers' essay, and it deserves more comment than is possible in this brief review. Sociologists may disagree with Fallers' use of some terms and his necessarily brief account of our knowledge of social trends and of major differences between societies, but they will nonetheless profit from his thinking on this popular subject.

Max Rheinstein seems to be more confined to the armchair than the other essayists. While reading his discussion of the problem of law in the new nations of Africa, one must remind oneself that it is important to raise questions as well as to answer them.

The volume ends with an essay by Robert Levine on political socialization and culture change. In some respects this is the best essay in the book. He formulates a set of propositions of limited scope, explicitly and clearly stated, which are correctly labeled "hypotheses," which derive from a consideration of specific empirical studies, and which are stated in testable terms. It invites empirical research.

When I finished reading this work, my first reaction was that another non-book had been published. But that is too harsh a judgment, both because there are saving graces and because hard-working field researchers taking an intellectual flyer should be tolerated. Nonetheless, there is a limit. And in view of the increasing number of non-books in the social sciences, I wonder how many readers would join me in a plea for less writing and more research.

WENDELL BELL

Yale University

Leadership in Communist China. By JOHN WILSON LEWIS. Ithaca, N.Y.: Cornell University Press, 1963. Pp. xiii+305. $5.00.

This study provides a great deal of material on Chinese Communist theory, policy statements, and organizational directives, much of which has previously been available in English translation. The author appears to have a sovereign command of his sources, and he has clearly worked his way most diligently through a variety of textual material. Yet the fulness of documentation unfortunately is not matched by a similarly sustained effort at interpretation. In fact, this study lacks an interpretative framework; practically no attempt is made to place the material adduced within a conceptual or historical context.

The author does not distinguish between what has been taken over from the international Communist model and the distinctive new elements in Chinese Communist doctrine. Similarly, he does not distinguish between ritualistic affirmations and genuine policy directives. We are served with long and often tedious quotations from Mao and other top leaders but without critical or sympathetic evaluation as to their historical context. Furthermore, there is no attempt made to check these ideological pronouncements against social realities. As a result we learn a good deal about how the Communist apparatus is supposed to operate, but almost nothing about how it in fact operates.

It is no doubt useful to try to find out how a Communist elite sees itself and its tasks, but if this is not supplemented by some independent assessment of its success or failure the danger is high that one's perspective becomes distorted, through an involuntary tendency to take the elite's pronouncements at face value. This danger the author has not resisted nearly enough. Despite his basic re-

jection of Chinese communism, he bows in admiration before the "flexibility," "devotion," and "amazing success" of leadership. Had he paid more attention to other than the official sources and had he tried to assess the raw materials here presented within a wider framework, he might have arrived at more modulated judgments.

Political sociologists will find much here to whet their appetitites; the statistics on the numerical growth of the Communist Party, for example, indicating that 70 per cent have joined since 1953, and that annual attrition rates are around 5 per cent, are fascinating; so are the calculations that by 1961 there was about one party member for every eight Chinese families. Similarly, the facts that the members of the Central Committee stem in the main from the upper and middle classes, have fairly high educational levels, joined the party many years ago, and are now middle-aged and older (80 per cent being between the ages of 49 and 63, while only around 8 per cent of the membership is between these ages), seem of major sociological as well as political relevance.

At least some of the raw materials for an analytical study of the Chinese Communist elite and its policy are now available; one can only hope that subsequent students will utilize them to advantage and bring our knowledge of the Chinese Communist movement up to the high level which informs the study of Communist movements in Russia, western Europe, and America.

LEWIS A. COSER

Brandeis University

Africa in World Politics. By VERNON MCKAY. New York: Harper & Row, 1963. Pp. xii+468. $6.75.

A book with this title written fifty years ago would presumably have been about the scramble for Africa in the late nineteenth century. Now that we are in the midst of what Tanganyika's President Julius Nyerere has called "the second scramble for Africa," it is extremely useful to take an over-all look at the many facets of Africa's contemporary involvement in world affairs. This is what this book does in a comprehensive, if summary, fashion. Its author is most fitted to the task by his academic and organizational background and, one might add, by temperament.

The book explores Africa's role in the United Nations and the U.N.'s role in Africa. It surveys pan-African, Afro-Asian and Eurafrican movements. It then treats separately the relations of Africa with India, with Russia and the Communist world, and—at greatest length—with the United States. Strangely enough, the reproach that might be made to this book is that it neglects Europe, which is surely still part of the world.

While the sociologist might have preferred some more systematic comparative analysis which might help to explain some of the shifts noted in the world's attitude toward Africa and Africa's toward the world, the book nevertheless offers a good descriptive base for further work. As the further, more specialized, studies come along, it will be worthwhile, I suspect, for students and scholars to refer back to this broader perspective.

IMMANUEL WALLERSTEIN

Columbia University

Administration and Economic Development in India. Edited by RALPH BRAIBANTI and JOSEPH J. SPENGLER. ("Duke University Commonwealth-Studies Center Publication." No. 18.) Durham, N.C.: Duke University Press, 1963. Pp. 312. $7.50.

This collection of papers, some read at and some written after the 1960 meetings of a seminar held at Duke University's Commonwealth-Studies Center, shows the advantages and disadvantages of keeping the members of a symposium innocent of the contents of each other's papers. On the one hand, the separate contributions retain their own internal logic and point of view, but on the other, the effect of the separate articles is non-additive. Hence, this book is best considered as a set of articles strung loosely on the threads of economic development and public administration.

Ralph Braibanti's lead essay comprises a long billet-doux to the Indian Civil Service and to a lesser extent to its successor, the Indian Administrative Service. Braibanti admires their intellectuality, their preference for generalists over technical specialists, and

above all, their ability to withstand the contaminating blight of foreign, particularly American, advice on public administration. The latter he sees as faddist, threatening to destroy the values of the old system without offering a successful substitute, moved by cultural imperatives native to other societies, hampered by the governmental bureaucracy which seeks to transmit new ways, and in general an unworthy import which Indians have done well to treat with some disregard.

Hugh Tinker disassociates himself from the idyllic romanticism with which the village is perceived in current Indian political thought and economic development plans; denies the historicity of representative government in the villages; regrets the departure of that benevolent despot, the British district collector; laments the "increasingly Byzantine characteristics of the Community Development empire"; and urges that a real and not merely apparent decentralization of power take place.

S. P. Jagota outlines and briefly defends the training programs for the various civil services. In another article later in the volume he argues that planning has bent a little the constitution, as originally conceived, particularly in state-center relationships; but the fundamental structure is still intact.

Wilfred Malenbaum sees as the greatest need for economic development in India a new leadership which will attend not only to distant goals and Western econometric models but will look with fresh eyes at the practical lessons to be learned from the failure of the Second Plan. An *ad hoc*, short-run point of view with no ideological or theoretical blinders should be taken, action should be substituted for policy statements, and foreign experts should be more fully utilized in reaching important decisions. On the latter point, it is not clear whether Braibanti would demur.

Richard Park describes the effect of economic development activities and the attempts to create local representative government upon the traditional functions of the hard core of local administration, the revenue hierarchy.

Robert Tilman takes as axiomatic the notion that caste is opposed to economic development. I am not exactly clear as to why this is so self-evident a proposition. Caste, he concludes, is changing but still has a great deal of life left in it.

Joseph Spengler treats Kautilya's *Arthashastra* almost as if he were reviewing a contemporary book. The resulting analysis is not quite as rewarding as the classic review of Genesis in the *New Yorker*, but it is nonetheless a highly useful presentation.

N. V. Sovani argues that the evil effects of British rule are both exaggerated and in some instances, such as the hardening of caste lines, overlooked. The fundamental obstacles to India's economic progress he sees in assorted features of the social structure and national character. This article is an early example of an increasingly important stream of literature.

Ashok Mitra argues that the tax burden on Indian agrculture is too low; it must be raised, hitting hardest at the well-to-do cultivators.

In all, the articles are thoughtful and well written, a mixture of the bland and the polemic, with the bland leading.

RICHARD D. LAMBERT

University of Pennsylvania

Land Reform in Relation to Social Development: Egypt. By SAAD M. GADALLA. ("University of Missouri Studies," Vol. XXXIX.) Columbia: University of Missouri Press, 1962. Pp. xiii+139. $3.75.

As the author of this competently written but small volume points out, there is no discipline which comprehends all aspects of land reform. Dr. Gadalla's own discipline is rural sociology and his approach is primarily descriptive. His concern is to describe how well Egyptian land reform of 1952 had achieved its social objectives by 1956.

About half of the book is devoted to the familiar details of agrarian conditions before 1952 and to the provisions of the reform law. The following two chapters on the effects of the land reform law on rural communities and on rural families, respectively, comprise the heart of the book. The information contained in these sections was compiled from 600 interviews carried out among Egyptian peasants in six villages in the Nile Delta. Three of the villages had undergone land reform and three, still under the control of the Ministry of

Pious Foundations, were used as controls because they were still being run as in prerevolutionary times. The author is to be greatly commended for carrying out this field research, which, even for a native-born Egyptian, is no mean achievement. The elaborate research apparatus did not serve importantly, however, for the major conclusions are simple but significant. Obviously, agrarian "structure" has changed with the expropriation of the old landlords. Now the paternalistic officials who run the agricultural co-operatives have succeeded the landlords as the "upper-upper" class. Peasants who have acquired land under the new law and those whose rent payments have been similarly controlled are temporarily better off financially. In due course, the author assures us, population increase and the fragmentation of holdings through inheritance will eat up this gain.

For the rest, there is little difference between the peasants in the reformed villages and those in the control group with regard to social habits, family relationships, political initiative, or even in debt condition. The only difference of statistical significance that could be isolated was with regard to the way in which leisure is spent by each group. The reformed group spent about twice as much of their leisure in coffee houses as did the control group. Gadalla does not think that this is due only to their having the price of a cup of coffee but rather to the "absence of adequate measures for utilization of leisure." In his conclusions and recommendations, the author points out that more effort was being spent on the welfare of the peasant's livestock than on the peasant himself. He also comes out strongly for birth control.

The major merit of this study is the description of the condition and life patterns of the peasants in both the experimental and control situations. The careful sampling technique lends additional support to the information we have from Ayrout and Ammar. Furthermore, the author's lengthy on-the-spot investigation has lead him to make a clearer presentation than that of Wheelock (*Nasser's New Egypt*) and a much more sober evaluation than that of Warriner (*Land Reform and Development in the Middle East*). One would have hoped that Gadalla would devote his great talents to describing more complex aspects of rural social relationships in Egypt. Instead, as we learn from an addendum on the last page, the author "has since conducted research on farm accidents in Missouri under a three-year grant from the National Institute of Mental Health."

LEONARD BINDER

University of Chicago

Arab Rediscovery of Europe. By IBRAHIM ABU-LUGHOD. Princeton, N.J.: Princeton University Press, 1963. Pp. 188. $4.75.

This is a valuable study—an early example of a genre that will become more common in the years ahead as the "new nations" become less capriciously self-congratulatory and more seriously concerned with understanding their current situation in terms of their own recent past. This case study performs this function for the Middle East, focusing upon the Arab "rediscovery" of Europe in the nineteenth century as an aftermath of the Napoleonic invasion and British conquest. It covers the period 1798–1870.

The study is modestly framed. There is no social theory beyond a simple schema of "culture-carriers," that is, the transmitting agents of cross-cultural transfer. There is no social history beyond three early Arab chronicles of the Napoleonic expedition and its impact. The author tells us that "the historian of culture-contact, attempting to assess the significance of such interaction, is faced with a total absence of any literary work describing even meagerly the nature and impact of such contacts."

The amateur observer of this history must boggle at such a dictum, even when announced by a specialized scholar. True, only a tiny fraction of Egyptians was literate 165 years ago; hence, there was no publication of books or a periodical press. But some literate Egyptians penned memoirs, kept diaries, composed letters. So did the officers and men of the French expeditionary force. When the author insists on "the *total* absence of *any* literary work" one must inquire what criteria of "literary work" he is applying.

The query does not detract from the interest and importance of Abu-Lughod's presentation of the later "literary work" that he does deal with. He traces the nineteenth-century impact of the West through two main

sets of records: translations and travels. In two chapters, he traces the development of the Translation Movement and then reviews the translated material. These are followed by three chapters on travelers to Europe. After a review of the available travel accounts, including a useful chronology, he focuses on the two main topics they cover: European political and social organization, and the educational system and the social order.

The book concludes with a summary of themes and a glimpse of what was to come after 1870, a period to be reviewed in a separate work. We shall look there, with particular interest, for the evolution of the early theme of "specialization between cultures—a spiritual East and a materialistic West."

DANIEL LERNER

Massachusetts Institute of Technology

Clan, Caste, and Club. By FRANCIS L. K. HSU. Princeton, N.J.: D. Van Nostrand Co., 1963. Pp. x+335. $7.95.

In the tradition of Ruth Benedict, this book concentrates on "the world as Hindus see it and approach it"; Chinese and American ways of life, analyzed by Hsu in earlier writings, are here examined again for contrast with the Hindu. Hsu argues that the fundamental patterns of these three cultures differ in the following ways: (1) as to cultural orientation, India is supernatural-centered, emphasizing unilateral dependence within hierarchic contexts, China is situation-centered, emphasizing mutual dependence, and America is individual-centered, stressing self-reliance; (2) the dominant relationship within the nuclear family is that of mother-son in India, father-son in China, and husband-wife in the United States; (3) the Chinese father-son relationship is continuous in that most sons will become fathers; this makes for a strongly centripetal tendency in China. The Indian mother-son relationship and the American husband-wife relationship are discontinuous, and this makes for a primacy of centrifugal forces; (4) after the nuclear family the most important human grouping is the caste in India, the clan in China (Hsu means lineage), and the club, or voluntary association, in the United States. On the basis of these hypotheses and other considerations Hsu also presents "a new psychosocial perspective" on the dynamics of the Hindu caste system (chap. viii).

Whatever merit these and other interpretations may have is tremendously compromised by Hsu's inadequate theory and methodology. The reader searches in vain for any significant theoretical advance over Benedict's *Patterns of Culture* (1934). For Hsu, it is "psychocultural orientations" which affect the development of human groupings and which give direction to the Chinese, Hindu, and American worlds. The emphasis is on personality variables, for example, "needs for sociability, security and status" on the one hand and broad cultural patterns on the other hand. In taking Benedict's concept of broad cultural patterns essentially for granted, Hsu fails to take into account recent advances by Anthony F. C. Wallace, Ward Goodenough, and others. Hsu's theory also contains some elements of the idealistic theory of social action, the limitations of which were well charted by Parsons in *The Structure of Social Action* (1937). Hsu's work represents an earlier generation of comparative analysis, in which the objective was to compare the essentially *unique* configurations of different cultures. The most important trends in comparative analysis today, on the other hand, are concerned not with unique configurations, but with common, theoretically derived variables along which all societies may be measured and compared (see F. Kluckhohn and F. Strodtbeck, *Variations in Value Orientations*).

Hsu spends much of the book attempting to show that all facts either (*a*) support his interpretations or (*b*) were wrongly interpreted by the original analysts. (On the latter, see, for example, pp. 216–17, where Hsu turns Riesman inside out in order to prove that Americans are really both self-reliant *and* other-directed!) He even attempts to rationalize his method: "The function of theory . . . is precisely to connect data which may be . . . divergent and incompatible. . . . The role of the scientist is to . . . link divergent phenomena, even complete opposites" (pp. 20-21).

Formal research methods and procedures of validation are eschewed. Hence, though Hsu claims (p. 25) that he is *demonstrating* the above hypotheses about China, India, and America and not merely illustrating them, the

reader has no way to judge this for himself. Hsu's attacks on British structuralist explanations, on Homans, on theories of caste, on the works of earlier specialists on Indian society, etc., are for the most part quite unconvincing. He regards "psychic cohesion" as a non-Durkheimian form of solidarity, apparently innocent of Durkheim's concept of mechanical solidarity.

There is no doubt that Hsu has read widely in the best recent studies on India or that he has a definite "feel" for Chinese and American patterns of action. But he is too wedded to a theory which has been losing ground for twenty years and too given to arrogant castigations of some of the best trends in data analysis. In *Clan, Caste, and Club,* Hsu asks for trouble again and again. He is bound to get it.

ROBERT M. MARSH

Cornell University

ERRATUM

The *Journal* has received corrected figures from Professor Oscar Grusky in connection with his paper, "Managerial Succession and Organizational Effectiveness" which appeared in its July, 1963, issue:

Column (5) of Table 1 should read from the top of the column down (corrected figures are italicized) as follows: *4.6,* 3.4, *4.1, 2.8,* 6.9, *6.6, 2.1,* 4.9, 6.6, 6.8, *1.1,* 2.9, 3.9, 2.6, 6.8, 5.4. Column (6) of the same table should read: 6.5, 2.9, *3.3,* 5.3, 4.2, 4.4, *4.1*. . . . (The remainder of the column is correct.) The rank-order correlations by Kendall's tau are correct except for the correlation between columns (2) and (5), which is —.62 and not —.60.

One error in Table 1 (for the Braves) affects Tables 2 and 3. Reading down in Table 2, the first column should show 2, 4, and 1 case, respectively, and the second column should show 0, 1, and 7 cases. Thus, $p = .009$ by Fisher's Exact Test and not .0014. In the top half of Table 3, the lower left-hand cell should have 1 and no 0 cases, and the lower right-hand cell should show 2 and not 3 cases. The correct p level is .45 and not .11. Hence, unlike Tables 1 and 2, the confidence level is altered considerably. The bottom half of the table remains correct.

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DISPLACEMENT OF SCOPE: A PROBLEM OF THE RELATIONSHIP BETWEEN SMALL-SCALE AND LARGE-SCALE SOCIOLOGICAL THEORIES

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ABSTRACT

This paper deals with the ways in which some exponents of structural-functional orientations and interpretative-interactional approaches handle the problem inherent in the contrast between micro- and macrosociological concerns. Most authors assert that their theories fit both areas: they solve the problem by projecting a small-scale theory on the macrosociological screen, or vice versa. This "fallacy of displaced scope" is avoided by some theorists by assuming a dualistic position, and by others by withholding judgment. The author concludes that the problem of differentiated scope remains; it poses tasks which are much more complex than present-day theorists seem to realize.

THE PROBLEM

Many modern sociologists are convinced that social phenomena cannot be adequately explained in terms of mere physical, biological, and psychological conceptions. The rejection of reductionist interpretations leads to the accentuation of social, that is, ***interpersonal human,*** interaction as one basic concern of sociology, and possibly its most distinguishing characteristic.[1] On this, there is agreement among such widely differing American theorists as Howard Becker, Herbert Blumer, George Homans, Talcott Parsons, and Pitirim Sorokin, to name but a few. Yet it would be a gross simplification to divide American sociological theories into reductionist (e.g., neo-positivist) theory and "social action theory,"[2] hoping thereby to have successfully disposed of the problem of the theoretical differentiation of the discipline. Among sociologists who subscribe to the social-action approach alone, we discern not only considerable substantive but also great methodological differences.[3] Within the confines of the present paper, we shall focus on one aspect of these differences which cuts through substantive as well as methodological dividing lines:

[1] The italicized qualification is essential for two reasons. (1) The concept of "interaction" itself may be used in a purely physical connotation, as the "interaction of mechanical parts." George Lundberg has used the term sociologically in this mechanistic sense. (2) The concept of "social" may be used in order to designate the instinctive mass behavior of animals, as in the term "social insects." "Human ecologists" tend to use the term "social interaction" in this biological sense. Both conceptions are reductionistic and thus basically different from the one used in the present exposition.

[2] This seems to be implied by the presentation of sociological theories by Roscoe C. Hinkle, Jr., and Gisela J. Hinkle in *The Development of Modern Sociology* (Garden City, N.Y.: Doubleday & Co., 1954).

[3] For some of these differentiations, see Helmut R. Wagner, "Types of Sociological Theory," *American Sociological Review*, XXVIII (October, 1963), 735–42.

the difference of scope. Empirically, sociological interests range all the way from the study of interactional encounters between two persons to the analysis of whole societies; theoretically, they deal with processual or structural units from the diad to the societal system. This implies both a tremendous quantitative span and a huge array of qualitative differentiations. Setting all other considerations aside, we may state that most sociologists operate, at least in particular phases of their work, either within small-scale, or intermediate, or large-scale ranges. This "differentiation of scope" is a consequence of the inherent difficulties in treating the whole range of sociological interests successfully from a single baseline.

We may speak of this differentiation of scope as one form of a sociological division of labor. It poses problems for sociology as a whole, problems whose basic significance is not always recognized. As a rule, sociologists who work in intermediate areas—such as community studies, stratification research, or organization theory—are more aware of the limited range of their subject matter than the theorists at the extremes. Yet, it is exactly the juxtaposition of small-scale and large-scale operations which brings the problems of sociological scope into focus. It is for this reason that we shall discuss them in terms of the differences between microsociological and macrosociological considerations and approaches.[4]

Nineteenth-century sociology, as a new type of social philosophy with scientific ambitions, pentrated the ranks of the older "social sciences" on a macrocosmic scale. Microsociology, by contrast, is a product of the early twentieth century. It has its roots in the work of the founders of American social psychology[5] and, independently, in contributions of the two foremost German sociologists, Georg Simmel and Max Weber. With its appearance, that particular dualism of scope with which we are concerned here was introduced into the discipline.

Western sociologists had fifty years to settle the ensuing methodological and theoretical problems. Yet, it is characteristic for the discontinuity of the discipline that even the mere existence of the problem has been recognized only sporadically. Modern sociology, we are inclined to conclude, is hardly closer to solving it than it was in the first decade of our century. Consequently, it would be premature to predict the eventual elimination of the problem with progressing methodologcal sophistication on the one hand, and theoretical refinements or new departures on the other. By the same token, we are not prepared to take the opposite stand and to declare the given dualism of micro- and macrosociological orientations and operations an inherent and irrevocable feature of sociology as a whole. But, whether unavoidable or not, the differentiation of scope poses serious problems of theoretical consistency as well as of general coherence of the discipline. Among these problems, that of the transition from micro- to macrosociological considerations and expositions is crucial.

CLASSICAL APPROACHES

The outstanding sociologists of the period prior to World War I did not necessarily state the problem of the differentiation of scope, but they nevertheless dealt with it indirectly.

For Émile Durkheim, sociology was macrosociology by definition. His theses of the externality of social facts and of

[4] The terms "microsociology" and "macrosociology" are not often used by American sociologists. They play a prominent part in the theoretical system of the French sociologist, Georges Gurvitch, notably in his major work on *La vocation actuelle de la sociologie: Vers une sociologie différentielle* (Paris: Presses Universitaires, 1950). While adopting these terms as convenient shorthand designations of the two polar ranges of the sociological continuum of formal scope, we do not subscribe to the particular meaning Gurvitch ascribed to them.

[5] Notably William James, William McDougall, James Baldwin, Charles Horton Cooley, and later George H. Mead.

the exclusive embodiment of "the social" in the "collective consciousness" as the only realm of social "reality" make small-scale events into incidents within the large-scale social processes in which "society" exists. As he demonstrated poignantly in his treatment of *Suicide*, subjective decisions of individuals warrant not the slightest sociological attention: they average each other out in statistical mass processes, and the latter follow certain objectively ascertained "laws."[6]

While Durkheim established the unity of his theory with the help of his conception of the "collective consciousness," Charles Horton Cooley achieved the internal coherence of his social psychology through his conception of "human nature." "Society" and "individual," for him, are "simply collective and distributive aspects of the same thing." This holds for the "smallest, simplest, or most transient" groups of humans as well as the most "extensive, complex, or enduring" configurations. There is, then, "no society or group that is not a collective view of persons"; but neither is there an "individual who may not be regarded as a particular view of social groups." Observed differences, he added, do not reside "in the facts themselves" but exist only "to the observer on account of the limits of his perception."[7] The human mind, then, comprises the microcosmos of the individual as well as the macrocosmos of society. The sociologist separates them solely because he is unable to perceive and grasp both simultaneously.

Both Durkheim and Cooley extended their theories over the whole scope of sociology with the help of ontological assumptions. Georg Simmel's ontology, by contrast, precluded such an attempt. His starting point, clearly, is microsociological: "There exist . . . an unending number of smaller, in individual cases seemingly unimportant, forms of relationships and kinds of interactions between men; . . . by inserting themselves between the large-scale, so to speak official social formations, they alone produce society as we know it."[8] Initiating the purely sociological (not social-psychological) turn from macro- to microsociology, Simmel occupies a unique place in the history of modern sociology. His formal theory of sociation is anchored in his treatment of the diad and the triad, a treatment which is sociologically as relevant today as it was when first published. But Simmel defies classification. It is often forgotten that he, with ease and elegance, transferred his statements about the relations between two and three persons to the relations between religious sects, political parties, economic market groupings, power constellations of nations, and others.[9] From this, it would follow that the formal aspects of social relations are identical for all ranges of sociology. Yet, Simmel punctures this conclusion in his no less important essays, "On the Significance of Numbers for Social Life" and "The Quantitative Determination of Group Division."[10] Here, he shows conclusively that the mere quantitative growth of social groups, etc., leads to significant qualitative

[6] The principle of Durkheim's position is clearly expressed in the following quotation: "the social suicide-rate can be explained only sociologically. At any given moment the moral constitution of society establishes the contingent of voluntary deaths. There is, therefore, for each people a collective force of a definite amount of energy, impelling men to self-destruction. The victim's acts which at first seem to express only his personal temperament are really the supplement and prolongation of a social condition which they express externally" (Émile Durkheim, *Suicide: A Study in Sociology;* American edition edited by George Simpson [Glencoe, Ill.: Free Press, 1951], p. 299).

[7] Charles Horton Cooley, *Human Nature and the Social Order* (New York: Charles Scribner's Sons, 1902), pp. 1–3.

[8] Georg Simmel, *Soziologie: Untersuchungen ueber die Formen der Vergesellschaftung* (1st ed., 1908; 3d ed.; Munich: Duncker & Humblot, 1923), pp. 14–15.

[9] See the sections on "The Expansion of the Diad" and "The Triad" in *The Sociology of Georg Simmel,* ed. Kurt H. Wolff (Glencoe, Ill.: Free Press, 1950), pp. 135 ff., 145 ff.

[10] *Ibid.*, pp. 87 ff., 105 ff.

changes even when the proportions between various subdivisions remain the same. To accuse him of logical and methodological inconsistency, though, would not do justice to his position. He explains the "completely fragmentary and incomplete character" of his own work, which inevitably produced inconsistencies, in terms of the basic inability of sociologists to master the "unending complication of social life" with their relatively poor and highly limited concepts and methods. To pretend the opposite would be megalomania.[11] Accordingly, a systematic theory of sociology is impossible, and inner consistency, among the fragments of sociological inquiries, is unattainable.

Anyone who accepts this position, then, could consider the dualism between micro- and macrosociological concerns but another manifestation of the sociologist's inability to cope coherently with the "unending complication" of his subject matter. This, however, is not a foregone conclusion. Max Weber, for instance, shared Simmel's opinion that the subject matter of sociology is inexhaustible and defies total systematization; yet he considered it possible to apply a uniform approach to problems of small as well as large scope. In contrast to Simmel, he did most of his work on the macrosociological scale. In fact, he ventured into the area of microsociology rather late in his scholarly career. His theory of social action and subjective understanding is more of an afterthought to, than a basis of, his major inquiries.[12] It came to the fore only in 1913, when he explained that the single individual and his actions form the basic unit or "atom" of the sociology of understanding.[13] He left not the slightest doubt of his intention to carry this approach through the whole realm of his sociology: "In sociology, concepts like 'state,' cooperative,' 'feudalism,' and similar ones, in general designate categories of specific kinds of human interaction; thus, it is its task to reduce them to 'understandable' action, and this means without exception: to the actions of specific single individuals." Therefore, behind such terms as " 'state' . . . stands nothing but a course of human actions of a specific kind."[14]

In the continuation of his considerations, Weber defined a series of basic concepts, such as consociated and associative actions, factual organization, institute, and others. These were later combined with still other concepts and discussed in the first chapter of *Wirtschaft und Gesellschaft*. Combining (non-quantitative) probability expectations with the conceptualization of "typical behavior" in various areas of human relations, Weber indicated a methodological path leading from the "understanding" of individual behavior to the analysis of larger interactional combinations, processes, and structures. Unfortunately, all these fruitful suggestions remained in the elementary state of initial definitions and general remarks. Weber's many students and followers, significantly, failed to take up these suggestions. This alone is an indication that they did not recognize the tremendous problem of scope which the German sociologist had just begun to tackle.

Among later European sociologists, George Gurvitch seems to be the only one who not only clearly recognized the ex-

[11] Simmel, *Soziologie*, pp. 12–13, n. 1.

[12] Its first explicit statement is to be found in his essay "Ueber Einige Kategorien der Verstehenden Soziologie," *Logos*, Vol. IV (1913). However, only a later version is available in English translation, from the first chapter of *Wirtschaft und Gesellschaft*, a work published posthumously.

[13] He excuses himself for making this "in itself questionable comparison" between individuals and atoms.

[14] Max Weber, "Ueber Einige Kategorien der Verstehenden Soziologie," reprinted in Max Weber, *Gesammelte Aufsaetze zur Wissenschaftslehre* (2d ed.; Tübingen; Mohr, 1951), pp. 439, 440 (my translation).

istence of this problem but also offered a clear-cut theoretical and methodological answer to it. His own sociological system is "differential" and comprises the two distinct branches of microsociology and macrosociology. Both differ from each other in subject matter as well as in procedures.[15] Within his frame of reference, then, the dualism of scope is paralleled by a methodological and theoretical dualism: it is treated as a basic and irrevocable fact.[16] In this fashion, then, Gurvitch denied the possibility of successfully completing the task which Weber had outlined before him.

The positions of these five sociologists indicate a whole range of basic answers to the problem of differentiated scope. Three of them assume the possibility of a unitarian theory covering the whole scope of sociology: Durkheim postulates this unity by considering microsocial occurrences as mere epiphenomena of macrosocial processes; Cooley establishes it by telescoping micro- and macrosocial phenomena into one social-psychological complex; and Weber suggests a social continuum ranging from the individual "social actor" to society as a whole. The fourth position, that of Gurvitch, is explicitly dualistic: it postuates the existence of two social realms necessitating two sets of theories and methods. Finally, a fifth position is offered by Simmel. He remains uncommitted and, in a sense, brackets the whole problem.

MODERN APPROACHES

The positions discussed seem to exhaust the logically possible answers to the problem of differentiated scope. Thus, when we turn to some of the more recent theories of American sociologists, we do not expect "new" answers. Rather, we want to see how the problem occurs in the light of certain theoretical approaches. This calls for attention to at least two aspects of the matter: (1) the actual ways in which microsociological in comparison with, or contrast to, macrosociological subject matters are handled, and (2) the theoretical suggestions for dealing with the problem "in principle."[17] For obvious reasons, we will have to confine ourselves to a few examples; they will be taken from structure-functional orientations on the one hand, and from interpretative-interactional theories on the other.

Aside from other relevant differences between these two approaches, they are formally distinguished by their points of departure. Structure functionalism starts with the conception of social system and sees smaller units, down to the individual, as structural subparts whose functions are essentially defined and confined by the whole system. Interpretative-interactional theories, by contrast, start with the individual actor and view larger wholes as results of the interlinkages and interrelations among a multiplicity of individual actors.[18] This suggests that structure functionalism is best equipped for the analysis of macro-

[15] His theory is rather elaborate; it does not follow the simple cutting line which we have suggested above. It seems hardly necessary to explain the details of it in the present context (see Gurvitch, *op. cit.*).

[16] Gurvitch developed a theory of multiple levels of social reality; his considerations imply an ontological position which make the dualism of scope inescapable.

[17] Insofar as such statements of principle constitute an *intention* of solving the problem of differentiated scope, we would have to add a third point here, that of the execution of this intention in the actual work of the theorists concerned. This, however, would entail a detailed investigation which cannot be compressed into a single article. Our summary discussion of the first two aspects, nevertheless, will indicate what, if anything, we may expect from an elaboration of the positions of individual theorists.

[18] It is this contrast of starting points which predisposes structure functionalism to become an "objective" theory operating on the bases of mechanistic or biological natural-science models and interpretative sociology a "subjective" theory of "voluntaristic" interpretations.

social, and interpretative-interactional theory for that of microsocial, phenomena. Yet, "microfunctionalism"[19]—under names such as "small-group theory"—is a well-established variation of American sociology; and numerous "interactionists" operate on the macrosociological scale. Thus, exponents of both approaches do not consider themselves limited to a certain scope of the sociological subject matter. In fact, most of them claim that their theory is applicable to the whole range of the micro-macrosociological continuum.

Yet, regardless of such intentions, an inspection of the actual work of the sociologists involved shows that scope has had a definite influence upon their operations. Microfunctionalism tends to acquire the formal characteristics of the interactional approach, and macrosociological theory of action those of structure functionalism.[20] These transverse alignments, obviously, are connected with the problem of the empirical and theoretical "control" of a sociological subject matter under varying quantitative conditions, such as numbers of people involved, size and structural complexity of collectives under consideration, etc. Observation of a few persons allows, invites, and even "urges" paying attention to aspects of the behavior of individuals in interpersonal exchanges; de facto, it tends to become interactional. Description of large-scale structures and/or mass processes, on the other hand, necessitates broad conceptions and summary statements behind which small-scale matters, such as individuals and their concrete interactions, disappear. For all practical purposes, it seems to be technically compelled to resort to interpretations which, de facto, assume the form of large-scale functional representations.[21]

This, then, is a most poignant expression of the *actual* problem of scope, in contradistinction to its *theoretical* treatment by exponents of the approaches under discussion. The remainder of the present paper will be devoted to an exemplification of this treatment.

MICROFUNCTIONALISM AND MACROFUNCTIONALISM

The proponents of the microfunctional "small-group" theory are convinced that the "social atom" of any interactional constellation of a few persons[22] is a full-fledged social system. Paul Hare, Edgar Borgatta, and Robert Bales explained, in their joint preface to *Small Groups*, that they are "concerned with the microscopic study of small social systems." Their branch of small-group research "is more than the study of one concrete 'compartment' of social phenomena among many others. It is the study of the generic social process on the microscopic level." Thus, it is "a method for the study of social systems" in general, "of culture, and of personality—all three."[23]

In view of the connection between at least two of the editors of *Small Groups* and Harvard, it is not surprising that this

[19] The term has been used by Don Martindale, *The Nature and Types of Sociological Theory* (Boston, Mass.: Houghton Mifflin Co., 1960), p. 501. It is the counterpart of the "macrofunctionalism" of large-scale system theory.

[20] This statement implies merely a noticeable degree of approach between one methodological position and the other, but not a "merger." The latter, in addition, is prevented by the original methodological positions: a functional interpretation of interaction remains positive; a social-action interpretation of societal systems tend to [illegible] elements of interpretative explanations.

[21] This does not imply acceptance of a totally functional system theory; Sorokin's "congeries" are as much included as other configurations.

[22] This conception of the "social atom" has been developed for sociometric purposes by J. L. Moreno (see, e.g., "The Social Atom and Death," *Sociometry*, X [1947], 80–84).

[23] Paul Hare, Edgar Borgatta, and Robert Bales (eds.), *Small Groups: Studies in Social Interaction* (New York: Alfred A. Knopf, Inc., 1955), pp. v and vi. See also Robert F. Bales, *Interaction Process Analysis: A Method for the Study of Small Groups* (Cambridge, Mass.: Addison-Wesley Press, 1950).

microfunctional conception is closely related to the macrofunctional theory of Talcott Parsons. However, it is of interest that, during a period of close collaboration with Bales,[24] Parsons convinced himself of the applicability of two additional methodological principles to his structural-functional system theory. One concerned the possibility of replacing the bio-organistic model for functional system theory by the physicalist model of classical mechanics,[25] and the other the assumption of a far-reaching structural-functional homology of macro- and microsociological systems. With regard to the latter, he stated in 1956 that "there are continuities all the way from the two-person interaction to the United States of America as a social system"; consequently, "we can translate back and forth between large scale social systems and small groups."[26] A few years later, this "translation" had resulted in the formulation of a "basic paradigm of system structure and functioning," which "was originally derived from the study of small groups, then extended to the family as a small group and finally to quite other levels of organizations."[27] The introduction of physicalistic in place of organicistic interpretations[28] doubtlessly simplifies the acceptance of a position according to which the differences between micro- and macrosocial "systems" are reduced to, such as size and overt structural complexity. Thereby, the problem of scope has been solved "theoretically" by postulation: the postulation of the homology of small and large social systems.

It may be of interest to compare the position of Parsons and Bales with that of another small-group theorist, George Homans. Homans based his earlier study of *The Human Group* on the combination of a kind of multifactor analysis within a functional equilibrium theory. Here, he posed "the problem of the relation between small groups and the civilization of which they are a part." He drew a tentative line of "emergent evolution" from small groups to large civilizations, and saw a similarity between certain processes in both. Yet, he remained aware that civilizations represent combinations of groups and develop features beyond those which are necessary to maintain a group; yet, in order to persist, they must maintain "some of the features of the small group itself."[29] There may be doubt as to whether or to what degree Homans tended here toward the notion of a homology between groups and civilizations. However, he dispels this doubt in his later book, *Social Behavior*. De-emphasizing broader functional considerations and stressing the development of sets of analytical-functional propositions, he takes exception to the homology thesis. While admitting that there are "striking resemblances" between small informal groups and society at large, he does not

[24] The literary results of this collaboration may be found in Talcott Parsons, Robert F. Bales, and Edward A. Shils, *Working Papers in the Theory of Action* (Glencoe, Ill.: Free Press, 1953); and Talcott Parsons and Robert F. Bales, *Family, Socialization and Interaction Process* (Glencoe, Ill.: Free Press, 1955).

[25] Parsons and Bales, "The Dimensions of Action Space," in *Working Papers*, p. 102.

[26] Parsons, "The Social System: A General Theory of Action," in Roy R. Grinker (ed.), *Toward a Unified Theory of Human Behavior* (New York: Basic Books, 1956), pp. 190, 194, quoted by Charles and Zona Loomis, in *Modern Social Theories* (Princeton, N.J.: D. Van Nostrand Co., 1961), p. 427.

[27] Parsons, "General Theory in Sociology," in Robert K. Merton, Leonard Broom, and Leonard S. Cottrell, Jr. (eds.), *Sociology Today: Problems and Prospects* (New York: Basic Books, 1959), pp. 16, 17.

[28] Parsons continued to operate in terms of a physicalist "action space." He defined the dimensions of this space in terms of "a law of inertia, on the *analogy* of the use of the term in classical mechanics" ("An Approach to the Psychological Theory in Terms of the Theory of Action," in Sigmund Koch [ed.], *Psychology: A Study of a Science* [New York: McGraw-Hill Book Co., 1959], III, 631).

[29] George C. Homans, *The Human Group* (New York: Harcourt, Brace & Co., 1950), pp. 441, 447, 459, 468.

think "that the one is simply the other *writ small*." His objection is based on the *relevant argument that the "fundamental processes of behavior" in small groups consist* in "direct exchanges with the other members"; in the larger society, persons relate to each other by virtue of their "position in some institutional scheme." In other words, the elementary social processes observed on the small-group level are modified and complicated through the "institutions of society at large."[80] The latter, then, are characteristics of societal systems which do not appear, in miniature form, on the level of small-scale interactional units.

Homans' work differs from that of the structure functionalists proper in that he, from the beginning, operated in terms of a minimum of variables. When he proceeded from descriptive to predictive general propositions, he definitely disavowed the thesis of the homology of all social systems, regardless of size. He is, then, a small-group theorist with functional leanings who recognizes that differences of scope pose a problem which cannot be removed by theoretical fiat.

INTERPRETATIVE SOCIOLOGY AND SYMBOLIC INTERACTION THEORY

The theoretical agreement between Bales, the Parsonian small-group sociologist, and Parsons, the universal large-scale system theorist, is understandably close. It has no parallel among the sociologists who agree on a voluntaristic action theory but work at the opposite ranges of the micro-macrosociological continuum. This could be demonstrated from the writings of numerous theorists; but a few illustrations will have to do.

Among the theorists of modern American sociology who subscribe to interactional orientations but are concerned with the large-scale phenomena of history and society, Pitirim Sorokin occupies a conspicuous place. For him, "the meaningful interaction of two or more human individuals" *is "the most generic model of any sociocultural phenomenon."* He defines interaction as "any event by which one party tangibly influences the overt actions or the state of mind of the other." Such interaction is called meaningful insofar as "the influence exerted by one party over another has a meaning or value superimposed upon the purely physical and biological properties of the respective actions."[81] Yet, being essentially concerned with two objectives of universal proportions—a general theory of society, culture, and personality, and a general theory of the history of culture—Sorokin hardly ever lost himself in microsociological considerations. Thus, almost a decade after the appearance of the main exposition of his general sociological theory, he discussed Parsons' shift of orientation from 1937 to 1951 in the following manner:

> His new framework shows a very tangible departure from the semi-nominalistic and singularistic standpoint of the *Structure of Social Action* with its main axis of the "means-ends schema." Now this standpoint and schema are practically abandoned in favor of "a more generalized level" of analysis . . . and the Weberian semi-nominalistic and singularistic framework of "actions," "actors and roles" is embraced by a more adequate "realistic standpoint" of "social system," "cultural system" and "personality system," or by the larger framework of *"the whole play"* in which "roles, action, and actors" are but components. This shift explains why Parsons' present framework is more similar to my basic system than to that of his *Structure of Social Action*.[82]

It is clear, then, that Sorokin accepted the concept of "meaningful action" as an elementary starting point but decided, on the macrosociological level of his universal-

[80] Homans, *Social Behavior: Its Elementary Forms* (New York: Harcourt, Brace & World, 1961), pp. 379, 380.

[81] Pitirim A. Sorokin, *Society, Culture and Personality: Their Structure and Dynamics* (New York: Harper & Bros., 1947), p. 40.

[82] "Similarities and Dissimilarities between Two Sociological Systems" (distributed by the author, in mimeographed form, in or about 1955).

istic concerns, to avail himself of structure-functional devices. The latter, obviously, greatly simplify dealing with the structure of the social as well as the cultural "universe," with the "dynamics" of large-scale social and historical processes, and similar topics.[33]

Other exponents of interpretative-interactional orientations, insofar as they are essentially concerned with macrosociological problems, do not seek this solution. Instead, they agree with the late Howard Becker who viewed societies as "simply larger networks of sociation within which other groupings as smaller networks find their appropriate place"; they all "take form only out of the continual interweaving of social actions."[34] Yet, none of these sociologists seems to have significantly advanced beyond Weber. While they do not subscribe to Sorokin's implicitly dualistic solution, they do not display much concern with the methodological problems of scope either. In their actual work, they resort usually to functional rather than interactional representations of large-scale social phenomena. Thereby, they introduce a disquieting element of inconsistency into the whole area of "interpretative sociology," an inconsistency of which they seem not overly aware.

By contrast, most proponents of the microsociological versions of this approach insist strongly on monolithic interpretations of the total range of social phenomena. This is especially true of the "symbolic interactionists." As followers of George Herbert Mead, they stress the spontaneous and volitional character of human intercourse, and divide their attention between the two basic problems of socialization and sociation. The latter are related to the forms of meaningful interaction among individuals, and to the emergence of mutual expectations with regard to the ways in which persons relate to each other in specific situations, the roles they assume, the rules according to which they interact, and in general the orientations with the help of which they tend to establish continuity in their life activities, success in their undertakings, and the predictability of the outcome of social activities in future situations. These interactional processes and expectations are symbolic because they are mediated by, and find expression in, language and gestures, both of which convey ("symbolize") meanings.

Theorists of symbolic interaction insist that there is "no empirical observable activity in a human society that does not spring from some acting unit" in which individuals relate themselves to each other in meaningful ways.[35] The analytical concepts of social structure, culture, social system, etc., "may serve the purpose of emphasizing the importance of behavior that is learned in contrast to what is instinctive"; but they remain abstractions which "only describe what men do in generalized terms and do not force anyone to do anything."[36] In other words, social reality can be ascribed only to concrete interactional-situational processes; it can be described only in terms of symbolic-interactionist theory. This ontological commitment, then, governs the definition of soci-

[33] He differs from Parsons in that he keeps his "systems" flexible and deals as much with the lack of integration and equilibrium as their opposites. Likewise, from the start he has paid the same attention to problems of dynamics and change as to those of structure and stability.

[34] Howard Becker, *Through Values to Social Interpretation* (Durham, N.C.: Duke University Press, 1950), p. 43. In dealing with large-scale historical processes, however, Becker resorted to the heuristic device of constructed culture types along his famous sacred-secular continuum. While this procedure goes a long way toward explaining the persistence of societies in terms of the persistence of the concomitant complexes of value orientations, it does not supply a sufficient theoretical answer to the problem of scope.

[35] Herbert Blumer, "Society as Symbolic Interaction," in Arnold M. Rose (ed.), *Human Behavior and Social Processes: An Interactionist Approach* (Boston: Houghton Mifflin Co., 1962), p. 187.

[36] Tamotsu Shibutani, *Society and Personality: An Interactionist Approach to Social Psychology* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1961), p. 175.

ety itself, as given by the proponents of this orientation. In general terms, Herbert Blumer says that human society "is to be seen as consisting of acting people, and the life of the society is to be seen as consisting of their actions."[37] Tamotsu Shibutani explains: "Society consists of the recurrent adjustment and cooperation of associated persons through which action patterns of all kinds are formed, sustained, modified, evaded, or contravened. Sometimes the co-ordinated activities become highly organized, but there are also transient forms of interaction." Therefore, society "might best be regarded as an ongoing process, a *becoming* rather than a *being*."[38] Hugh Dalziel Duncan says that "society arises in, and exists through, the communication of symbols used to express relations between superiors, inferiors, and equals."[39] Arnold M. Rose, finally, sees in society "a network of interacting individuals" who have learned "culture" in "symbolic communication from other individuals back through time." This culture consists of "the related meanings and values by means of which individuals interact" and with the help of which they "can gauge their behavior to each other and to the society as a whole."[40]

These "definitions" of society progress in a certain fashion. Blumer focuses simply on acting people and their actions; Shibutani is concerned with the action patterns resulting from adjustment and co-operation, that is, the lasting forms of interaction which spring from the most general objectives of the actors; Duncan adds symbolic communication as the expressive medium through which men relate themselves to others; and Rose points to the given cultural, that is trans-situational, source of not only the semantic but also the eval ative meanings of the symbols used in co munication which is always concretely s uational. He, by the way, is the only o of the authors mentioned who, in this co text, expressly states that the interaction network of society with its cultural mea ings and values "precedes any existi individual."[41]

Statements of this kind are indicative the conception of society that exponen of symbolic interaction theory hold. In o sense, they are declarations of an inte tion: the intention to look upon "societ as if it were an interactional network ti together by symbolic communicatio Methodologically, the decision to acce and apply this approach is similar to th of a structure functionalist who looks u on society as if it were an organism other "whole" in unstable equilibrium. O tologically, it is superior to all approach which define social reality not in term of individuals but in terms of the possib emergent properties or characteristics human collectives. It would be wrong say, for instance, that Durkheim's "co lective representations" or Parsons' "fun tional imperatives" ought to be reduced the individual-psychological level; but the allude to phenomena which are derivativ of the actual social and ideational activ ties of individuals.[42] Yet, the terms of an sociological theory, "symbolic interactio ism" included, are constructs and as suc not identical with their "referends." Ther fore, the mere decaration of intention o fered by the theorists of symbolic intera

[37] *Op. cit.*, p. 186.

[38] *Op. cit.*, p. 174.

[39] *Communication and Social Order* (New York: Bedminster Press, 1962), p. 325, n. 1.

[40] Arnold M. Rose, "A Systematic Summary of Symbolic Interaction Theory," in Rose (ed.), *op. cit.*, pp. 10, 13.

[41] *Ibid.*, p. 13.

[42] Even Durkheim was eventually forced to sta that "collective representations" are nowhere lo cated if not in the minds of individuals. In 191 he wrote, not without a strong metaphysical ur dertone, "that the mind (*âme*) is the collecti consciousness incarnated in the individual" ("L Problème religieux et la dualité de la nature hu maine," *Bulletin de la Société Française de Philo sophie*, March, 1913, p. 74, quoted by Charle Elmer Gehlke, in *Émile Durkheim's Contribution to Sociological Theory* (New York: Columbia Uni versity Press, 1915), p. 44.

tion does not excuse them from the obligation to demonstrate the workability of their approach. This they can do only by carrying out their intention in a combination of empirical investigations with the further specification and amplification of their theoretical generalizations.

Some of the theorists of symbolic interaction stress the validity of their approach for the total range of social phenomena more than others, but all seem to refuse the notion that they are nothing but small-group sociologists. Restraint is recommended by Erving Goffman, who warns that "we must be very cautious in any effort to characterize our own society as a whole with respect to dramaturgical practices," that is, repeatedly performed symbolic-interactional practices within the quasi-permanent setting of "social establishments."[43] Most of the literature of this "school," however, shows little awareness of the actual difficulties which await the execution of their theoretical intentions. One may fully agree with Herbert Blumer's statements that people "do not act toward culture, social structure, or the like; they act toward situations," and that any other factor enters the sociological picture only "to the extent to which it shapes situations in which people act."[44] This, however, does not imply that such factors are exempt from proper sociological attention. Even if we grant that a situational interpretation of social behavior forms the core of all sociological concerns, we cannot avoid treating factors which, according to Blumer, *shape* situations.

The previously quoted "definitions" of "society" themselves contain elements which point to the existence of the problem of the differentiation of scope. The mere mention of "society" and "life of society" indicates areas of sociological concern which are bigger than situations. The term "action patterns" suggests a quasi-permanence of formal elements recurring in series of situations. The "communication of symbols" presupposes the existence of language, that is, a cultural medium of a considerable history and social spread. The distinctions between "superiors, inferiors, and equals" may occasionally arise in specific situations; more often than not, however, persons bring their status into specific encounters with others: it is anchored in institutional arrangements. References to "culture" and "networks of interacting individuals," finally, are an acknowledgment of the existence of trans-situational social factors of macrosociological magnitude and historical depth.

We may add that the interlocking of large networks of recurrent interactive situations of specific, quasi-permanent patterns leads to the emergence of social phenomena which cannot be predicted from any single interactional situation involved. To take an example from a faraway country: a man, identified as the president of a large corporation, speaks with another man, identified as an independent lawyer. Later, the lawyer meets a small number of men, identified as members of the country's senate. Still later, these men meet other senators at a committee meeting. There, they successfully oppose a governmental proposition which the president of the corporation, in the first encounter, had mentioned as being detrimental to the "industry." There is nothing in the three concrete situations which would explain the transformation of the president's opinion into senate-committee action. It manifests "power" which is not a product of any of the concrete situations but which is anchored elsewhere. Power, like status, is a trans-situational factor of great importance. Arnold Rose found that, in interactionist theory

[43] *The Presentation of Self in Everyday Life* (rev. ed.; Garden City, N.Y.: Doubleday & Co., 1959), p. 245. In his "Conclusions" to the book, the author promises a "simple addition" to the attempts at bringing "Personality-Interaction-Society" into a single framework of concepts (p. 242), but simply proceeds to discuss problems of "performance disruption" in microsocial settings. With regard to broadening the whole approach, he prudently suggests beginning with "smaller units" and adhering to the "case-history method" (p. 245).

[44] *Op. cit.*, p. 190.

and research, there is "a neglect of power relations between persons or groups"; they "are generally assumed to exist" but "are seldom given due weight."[45] It seems that "symbolic interactionists" find dealing with the problem of social power no less difficult than structure functionalists.

CONCLUSION

A basic assumption of this paper is that the problem of differentiated scope is inherent in the tremendous range of sociological subject matter itself. In other words, it imposes not only theoretical but also methodological tasks upon sociologists who are concerned with the micro-macro-sociological continuum as a whole, regardless of their particular theoretical frame of reference. Concentrating on selected proponents of structural-functional orientations on the one hand, and interpretative-interactional theories on the other, we concerned ourselves with two distinctly different aspects of the matter: suggested "theoretical" solutions and actual treatment of the problem. Most authors, apparently, have not devoted much attention to exploring the conditions for its solution. Instead, they have offered statements of principle or declarations of intention. These, however, were found unacceptable as actual solutions.

It remains to compare the positions of modern American sociologists with the basic answers offered by earlier theorists, as discussed in the first part of the paper.

1. Structure functionalism comes close to the Durkheimian position, even though it is not identical with it. Of the two approaches discussed, it seems to have a better chance for applying its analytical schemes over the whole range of the sociological subject matter. Obviously, a stable diad can be treated as a "social system" as well as, or better than, a stable social body of large dimensions. In spite of this advantage, the Parsons-Bales homology thesis lacks conviction. It is based on the assumption that all essential structural and functional components of large-scale systems are already contained in small-scale constellations. The theory, then, implies a Spencerian position: empirically, certain social systems of different size can be viewed as the stages of an organicistic growth process which unfolds itself from its first embryonic stages to a final mature form; analytically, the general scheme of the Parsonian system provides the necessary equipment for a generalized treatment of the ensuing problems of size and differentiation.[46]

The Parsonian system theory has been challenged repeatedly. From his macrosociological position, Sorokin made a strong case for the coexistence of integrated and unintegrated groups, of "systems" and "congeries," within large societies which, thereby, can no longer be treated as coherent systems in the Parsonian sense.[47] From his microsociological perspective, Homans indicates that the effects of institutional factors upon large-scale processes and involvements cannot be predicted from small-scale situations. Etzioni, in a recent article, develops a theory of "epigenesis" in which he demonstrates that organizational expansion follows not necessarily the organicistic assumption of structural-functional theorists; it may proceed by accumulation, that is, by addition of new structural elements and acceptance of new functions.[48] Fur-

[45] Rose (ed.), *op. cit.*, p. x.

[46] We do not assert that Parsons overtly subscribes to Spencerian views. His procedure is strictly "analytical." The implications of the homology thesis, however, are organicistic if not evolutionary.

[47] Sorokin, *op. cit.*, p. 313, *passim*. In agreement with this solution, Robin M. Williams distinguishes between the "factual cohesion" of social aggregates, which merely means absence of overt conflict, and "societal integration" by value-consensus, which means Parsonian system-coherence (*American Society: A Sociological Interpretation* [2d ed.; New York: Alfred A. Knopf, Inc., 1961], p. 544).

[48] Amitai Etzioni, "The Epigenesis of Political Communities at the International Level," *American Journal of Sociology*, LXVIII (January, 1963), 407–21.

thermore, there are reasons to suspect that the difficulties of the functional-system theorists in accounting consistently and adequately for the phenomena of power spring in part from the homology thesis.[49] These are some of the pertinent reasons which lead to the conclusion that this thesis does not remove the basic explanatory and methodological problems arising from the differentiation of scope. Therefore, we consider it fallacious.

The fallacy involved may be called the fallacy of displaced scope. It is committed whenever a theorist assumes, without further ado, that theoretical schemes or models worked out on the basis of macrosociological considerations fit microsociological interpretations, or vice versa. In the present case, this type of displacement has been simultaneously worked from both ends of the continuum: Bales insisted that his small-scale interactional model fits the large-scale social system, and Parsons proclaimed that his social-system theory fits the small group. The basic agreement between the two authors, who subscribe to the same principles of a positive functional approach, has made this telescoping of the microsociological into the macrosociological framework possible. Thus, at least in a formal sense, they placed themselves halfway between the classical position of Durkheim and that of Cooley.[50]

2. The majority of the proponents of symbolic-interaction theory suggest a solution which is both formally and substantively in agreement with that of Cooley. They pay great attention to the microsociological processes of situational encounters and develop sociologically promising propositions from their observations and reflections. Simultaneously, they consider their social-psychological and microsociological framework, seemingly as it stands, as both a necessary and a sufficient basis for a general sociological theory. Essentially, they assume a sweeping theoretical gesture: here are the basic tenets and propositions of our approach; in their terms, the interpretation of the rest of the total sociological subject matter poses no problem. Their writings are conspicuous for the absence of such a systematic and comprehensive explanation. Instead we note, so to speak, a big "et cetera" behind their microsociological expositions.

This is a repetition of the fallacy of displaced scope in a different form. If symbolic-interaction theory is to serve as an adequate basis for the interpretation of macrosocial phenomena, its exponents will have to demonstrate more than that their theory works well on the microsocial level. They will have to perform the transition from small-scale situational interpretations to interpretations of all those cultural and institutional factors which "shape situations," without sacrificing the subjective-interactional approach. But even more important, they will have to submit adequate interpretations of large-scale societal structures and processes, without resorting to non-voluntaristic explanations. This, we submit, is a formidable task in the face of the apparent machine-like character of large social systems which seem to follow their own mechanical laws, as well as in the face of the unanticipated consequences produced often enough by social processes of various magnitudes.

3. The position taken by Howard Becker, among others, resembles that of Max Weber. Like the latter, it constitutes a statement of intention rather than a solution of the actual problem. The task of finding an effective transition from micro-

[49] The main reason for these difficulties lies in the organic conception of the functional system itself: its ideal-typical model reflects the idea of a harmonious balance of all components which eliminates coercive tendencies from initial consideration. Like "change," they enter the picture afterward in the form of "disturbances" rather than intrinsic aspects of the system.

[50] It should be added that, in approaching Cooley's position, they reversed a subjective-volitional scheme into an essentially objective-positivistic one. The semblance is thus purely formal.

sociological interactional conceptions to the analysis of macrosocial phenomena, as formulated fifty years ago by the German sociologist, remains still to be done.

4. Sorokin, in contrast to three kinds of theorists discussed previously, selects a dualistic solution. His position, then, approaches that of Gurvitch. Recently, he even spoke of "microsociology."[51] In his general sociology, he decided that a large-scale theory of society should not, or could not, be burdened with interactional-situational considerations. He seems to imply that the latter should be left to small-group theorists and other specialists interested in microsociological phenomena. Societal structures and processes have to be treated in terms of macrocosmic concepts, which reduce mass processes to a single generalized denominator. This dualism, it seems to us, is realistic insofar as it takes cognizance of the apparent inability of "interpretative" sociologists to transfer their interactional conceptions effectively from micro- to macrosociological areas. To admit this difficulty, however, is not identical with subscribing to the principle that the problem of scope can only be solved dualistically.

[51] Sorokin, "Sociology of My Mental Life," in Philip J. Allen (ed.), *Pitirim A. Sorokin in Review* (Durham, N.C.: Duke University Press, 1963), pp. 3, 7, 11; and Sorokin, "Reply to My Critics," *ibid.*, p. 492. The term, however, is reserved for a representation of the case history of individuals with regard to, e.g., their intellectual development.

5. Homans and Goffman, finally, hav accepted a position of non-commitmen Following Simmel, they caution their fe low sociologists not to anticipate thec retically the solution of a task whos magnitude they apparently have not ye grasped and for which they seem as ye not sufficiently equipped. As it happen both have a stake in microsociology, eve though they adhere to quite different ap proaches. Adherence to either one, ther does not preclude the rejection of prema ture answers to a problem which othe of its proponents have solved with th help of mere theoretical declarations.

We may conclude with the followin statement: The realm of modern sociolog displays a number of incisions, th sharpest of which have been brought abou by the differentiated basic approaches t the sociological subject matter as such Whether the present obstacles to bridgin the gap between microsociological an macrosociological considerations are in dicative of another basic dividing line cutting through all types of approache and theories, ought not to be decided by fiat. If it is desirable to develop socio logical theories which are equally fit fo the analysis of small- and large-scal phenomena, they will have to be worke out gradually in the combination of sus tained empirical efforts with intellectua ingenuity.

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STRUCTURAL VERSUS INDIVIDUAL EFFECTS[1]

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ABSTRACT

Methods for separating the effects of group structure or composition from individual effects have been proposed by Blau and by Davis, Spaeth, and Huson. Both methods are useful, but they do not always hold individual or group characteristics strictly constant as intended, thereby making it possible to obtain spurious group or individual effects. Several modifications of these techniques are proposed, making use of more precise matching and correlational techniques. The advantages and limitations of each are discussed. Several additional problems are considered, including the effects of deviants, overlap of distributions among groups, selection processes, and the conceptual definition of structural or compositional effects.

One may define structural constructs as opposed to purely individual variables for purposes of group or organization theory. However, the frequent reliance, in empirical studies, on measures based on individual member responses often creates some operational ambiguity. Do the relationships observed when employing measures based on individual responses truly represent the effects of structural variables, or are they simply reflections of individual-level relationships?

Blau has suggested one approach to this problem. He proposes an analytic technique which provides, in effect, an operational definition of structure.[2] Davis, Spaeth, and Huson also provide an approach through the measurement of what they refer to as "compositional" effects.[3] These approaches overlap in several essential respects, and both represent significant contributions toward the solution of a difficult problem of sociological analysis. It is our intention to explore further the meaning of these methods, to consider some of their assumptions which appear to impose limitations on their applicability as presently formulated, and to suggest several means which may be helpful in reducing (if not overcoming) the effects of these limitations. Since Blau's approach is simpler in format, it will be easier to introduce the issues of the present paper primarily through reference to that approach. We shall then indicate their relevance to the method of Davis, Spaeth, and Huson.

Blau's strategy for determining structural effects may be summarized in three steps:[4]

1. An empirical measure, Z, is obtained that pertains to some characteristic of individual group members that has direct or indirect bearing upon the members' relations to each other (e.g., group identification, sociometric choices, initiation of interaction, rate of communication, or promotions).

2. The scores for measure Z, which

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[2] Peter M. Blau, "Formal Organization: Dimensions of Analysis," *American Journal of Sociology*, LXIII (1957), 58-69, and his "Structural Effects," *American Sociological Review*, XXV (1960), 178-93.

[3] James A. Davis, Joe L. Spaeth, and Carolyn Huson, "A Technique for Analyzing the Effects of Group Composition," *American Sociological Review*, XXVI (1961), 215-25.

[4] The following section closely paraphrases Blau, "Formal Organization . . . ," *op. cit.*, p. 63.

describes individuals, are combined into one index for each group, and this index no longer refers to any characteristic of individuals but to a characteristic of the group. The value of this index is presumed to vary across groups; we will define this variable as Z_{gp}. Thus any individual may now be characterized in terms of his *own* score along variable Z and his *group's* score along variable Z_{gp}.[5]

3. To isolate a structural effect, the relationship between the group attribute (Z_{gp}) and some dependent variable, W, is determined while the corresponding characteristic of individuals (Z) is held constant. The structural effect thus refers to the effect of Z_{gp} on W.

TABLE 1*

PERFORMANCE SCORES BY RATE AND FREQUENCY OF DISCUSSION

(Hypothetical Example)

Individuals Who Discuss Their Problems	Groups Most of Whose Members Discuss Their Problems	
	Rarely	Often
Often	0.65 (1)	0.85 (2)
Rarely	0.40 (4)	0.70 (3)

* Adapted from Blau, "Formal Organization . . . ," *op. cit.*, p. 64.

This method is illustrated by Blau through the hypothetical data of Table 1 in which five hundred persons are assumed to be arranged in fifty groups of about ten members each. We have numbered the cells for convenience from 1 to 4. Blau suggests that a structural effect is demonstrated by the differences in average performance scores between the two columns in Table 1. "This finding would show that, even when the effect of the individual's discussion rate of his problems on his performance is eliminated, just to be in a group where communication flows freely improves performance—other things being equal."[6] This statement, however, is based on an assumption which we must question.

The assumption of constancy within rows is asserted frequently by social researchers in relation to the type of analysis represented in Table 1. It can, however, lead to serious misinterpretations of data. It is important to recognize first of all that continuums underlie each of the axes in Table 1, even though dichotomous categories are employed. Individuals (and groups) are not simply "often" or "rarely" communicators, but are likely to differ along a broad continuum of frequency of discussion. With this in mind, let us assume that all distributions within groups are normal (although almost any type of continuous distribution would lead to the same conclusion). The effects of this assumption can be seen in Figure 1. The points Z_1–Z_4 represent the average individual discussion scores of individuals in cells 1–4 of Table 1. Several facts of importance are apparent from Figure 1:

1. In comparing individuals in cell 1 of Table 1 with those in cell 2, we are comparing individuals who have relatively low (Z_1) discussion scores with those having higher (Z^2) scores. We are not, in other words, holding the individual independent variable (Z) constant, and cannot say that the difference between the two cells on the dependent variable represents the effects of social structure. The same problem applies to the comparison of the remaining two cells.

2. The failure to hold Z strictly con-

[5] Lazarsfeld and Menzel would define the Z_{gp} variable in this usage as a "contextual property" of individuals, i.e., a property which stems from the individual's membership in a group (Paul F. Lazarsfeld and Herbert Menzel, "On the Relation between Individual and Collective Properties" in Amitai Etzioni [ed.], *Complex Organizations* [New York: Holt, Rinehart & Winston, 1961], pp. 422–40; see also Hannan C. Selvin and Warren O. Hagstrom, "The Empirical Classification of Formal Groups," *American Sociological Review*, XXVIII [1963], 399–411).

[6] Blau, "Formal Organization . . . ," *op. cit.*, p. 64

stant within rows has its counterpart in the failure to hold Z_{gp} constant within columns when more than two groups are being analyzed. The reader can see for himself how this unfortunate state of affairs develops by adding two normal frequency distributions, representing two additional groups, to the curves drawn in Figure 1. The pair of curves on the right would be labeled "High Discussion Groups." However, the one furthest to the right would contribute more members toward the computation of the mean in cell 2 than would the second group in that pair, while it would contribute fewer to the computation of the mean in cell 3 than would the second group. We would therefore be contaminating the individual-level (i.e., within-column) comparison with group effects.

Figure 1 implies a positive correlation between the Z scores of individuals and the Z_{gp} scores assigned these individuals according to the groups in which they are located. A more detailed and concrete illustration of this relationship and of the problems it creates can be seen from data which we have obtained employing Monte Carlo (random) techniques as follows: (*a*) A random sample of 150 individuals was drawn from a population which is normally distributed on individual variable Z. (*b*) This sample was randomly divided into fifty groups of three members each, and a Z_{gp} score (equal to the mean Z for the three members) was derived for each group. Figure 2 presents the data obtained in this way. Each of the 150 "statistical individuals" is located in the matrix according to his own Z score and the Z_{gp} score assigned to his group.

Z_1 = Average discussion score for individuals high on frequency of discussion in low-discussion group

Z_2 = Average for high-discussion individuals in high group

Z_3 = Average for low individuals in high group

Z_4 = Average for low individuals in low group
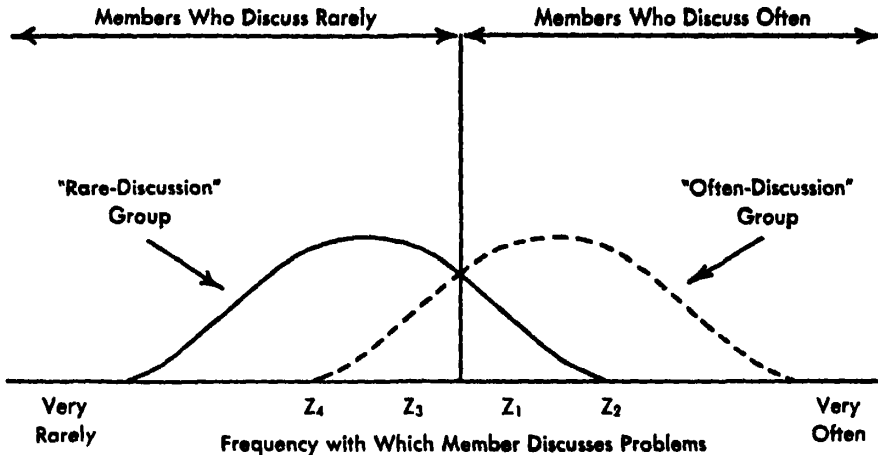
FIG. 1.—Hypothetical frequency distributions of members within two groups on a scale of frequency with which member discusses problems.

Let us define for these data a perfect linear relationship between the individual variable Z and the dependent variable W.[7] Table 2 analyzes these data by the

[7] For the sake of clarity we have assumed a linear correlation of 1.00 between Z and W. It is important to note, however, that the general observa-

Blau method. According to this method the results would be interpreted as showing a strong individual-level effect coupled with a moderate (but quite definite) direct structural effect. However, we have defined dependent variable W as being perfectly related to individual variable Z and have assigned members randomly to groups, thus effectively ruling out any possibility of a genuine structural effect. The spurious structural effect indicated in Table 2 reflects the failure to hold the individual characteristic strictly constant within rows.

The processes underlying the problems noted above can be seen more clearly by returning to Figure 2. The intersecting lines in the diagram correspond to the dichotomies employed in deriving Table 2, and the four quadrants match the four cells in that table. The solid black circle in each cell indicates the mean Z and Z_{gp} for those cases falling within the cell. It is apparent that the level of Z for individuals in cell 1 is, on the average, lower than that for individuals in cell 2. In other words, individual effects are not held strictly constant across the "high Z" in-

FIG. 2.—Scatter diagram showing Z and Z_{gp} scores based on Monte Carlo data

tions which we will illustrate with these data apply equally well when there is *any* direct positive relationship between Z and W. The use of a perfect correlation in our illustration simply serves to rule out random variation or "noise." Individual-level, curvilinear relationships between Z and W might lead spuriously to "contingency" or "inverse" type structural relationships described by Blau, depending upon the shape of the individual-level relationships. We are illustrating here a spurious "direct" type structural effect (see *ibid.*).

ividuals. And, of course, the same prob-
m appears for the "low Z" individuals
ı cells 3 and 4.

The failure to hold group effects con-
ant within columns can also be seen
adily from this figure. "Low Z_{gp}" indi-
iduals in cell 4 come, on the average,
om groups with *lower* Z_{gp} scores than
ɔ individuals in cell 1; and Z_{gp} scores
e lower for "high Z_{gp}" individuals in
ll 3 than for those in cell 2.

The strategy used by Davis, Spaeth,
ıd Huson is similar in several respects
ı that proposed by Blau. However, the
ırmer dichotomizes only on the Z variable
ıd not the Z_{gp}. The groups are spread
ıt along the horizontal axis according
ı their Z_{gp} scores. This eliminates the
oblem of contaminating within-column
fferences with group effects. However,
ıe problem of eliminating individual ef-
cts in the intergroup comparisons re-
ains. A limited solution to this problem,
ıplicit in the Davis, Spaeth, and Huson
ethod, is its restriction to individual
ıaracteristics that are dichotomous:
Within each population, individuals may
characterized by the presence or ab-
nce of a given *independent* attribute
A or $\bar{A}$)."[8] To the extent that the indi-
dual variables involved are truly
ichotomous, neither the Blau method nor
ıat of Davis, Spaeth, and Huson need
concerned about the problem of con-
olling for individual effects. However,
ost variables of interest to social scien-
sts (including some of those discussed
y Davis *et al.*) are continuous, and the
oblem remains for these. In Figure 3
e apply the method of Davis, Spaeth,
ıd Huson to our Monte Carlo data and
e demonstrated (spuriously) a "Type
IA" compositional effect: "a constant
ıdividual difference, along with a linear
fect of group composition."[9]

[8] Davis *et al.*, *op. cit.*, p. 216 (italics as in origi-
al).

[9] *Ibid.*, p. 220.

STRATEGIES FOR HOLDING CONSTANT INDIVIDUAL AND GROUP CHARACTERISTICS

The problems we have discussed stem from the assumptions that individual variables are held constant within rows and that group variables are held constant within columns. It is possible to reduce, if not to overcome, these problems through several modifications of the Blau or the Davis *et al.* methods. However, it is worth noting that the two problems may not be equally important in all situations. For example, a researcher who is interested

TABLE 2*

DEPENDENT VARIABLE W AS RELATED TO INDIVIDUAL VARIABLE Z AND GROUP VARIABLE Z_{gp}

(Hypothetical Example—Blau Technique)

Individuals	Groups	
	Low Z_{gp}	High Z_{gp}
High Z	93.3 (1)	100.4 (2)
Low Z	49.5 (4)	61.4 (3)

* Cell entries indicate mean W (for all individuals in the cell).

primarily in determining the presence of a structural effect may not be especially interested in whether a spurious individual-level effect appears as a result of his failure to hold group characteristics strictly constant. He will, on the other hand, be seriously concerned as to whether the structural effect he isolates is a spurious one caused by failure to hold individual characteristics constant. The techniques outlined below are not exhaustive, nor are they spelled out in fine detail. Our purpose is to open a number of avenues which may be useful in dealing with the problems raised above.

More precise matching of the individual variable.—The need for holding individual effects constant when comparing "high Z_{gp}"

and "low Z_{gp}" groups suggests that individuals be matched more closely on the individual independent variable (Z). It should be noted that the fairly crude matching achieved when Z is dichotomized represents a very great improvement over the situation which would exist if no attempt whatever were made to match individuals according to Z. However, as we have demonstrated, the dichotomy may not be sufficient. The larger the number of categories, of course, the greater the accuracy in matching; however, a "point of diminishing returns" is soon reached as the matching becomes more precise and as the number of cases falling within each category is reduced. The optimum number of categories to be used in any particular situation must be determined by the researcher.

Once the researcher has determined the number of categories into which to divide variable Z, he can proceed as in the Blau technique; he will, however, use an $N \times 2$ rather than Blau's 2×2 table. Certain of the cells in such a table might be empty; these, as well as their counterparts in the opposite column, would have to be abandoned. The remaining cells will provide an estimate of structural effects with individual effects held (more or less) strictly constant. Returning to our random data, the application of this modification (using a 7×2

FIG. 3.—Dependent variable W as related to individual variable Z and group variable Z_{gp} (hypothetical example—Davis *et al.* technique).

table rather than a 2×2 one) completely eliminates the spurious structural effect shown in Table 2. It may not, however, eliminate spurious individual effects.

The modified technique described above can be extended further so as to cover a broader range of scale points along the horizontal axis (Z_{gp}) in a manner suggested by Davis *et al.* This is preferable to the dichotomous analysis for several reasons. First, the dichotomy is usually inefficient statistically. Second, the use of a sufficient number of categories along the horizontal (Z_{gp}) dimension would hold group characteristics strictly constant and thus avoid the problem of spurious individual effects. Finally, the broader range of cases along the horizontal axis may lead to richer possibilities of analysis, increasing the likelihood of detecting the direct, inverse, and contingency effects discussed by Blau, or the various relationships in the typology outlined by Davis *et al.* Returning once again to our random data, the use of a 7×8 table rather than a 2×8 table would convert Figure 3 into a series of seven essentially horizontal lines, correctly indicating the presence of an individual, but not a structural, effect. However, the use of such a large number of cells drastically reduces the number of cases within each cell, so this variation will be appropriate only when the over-all number of cases is quite large.

Correlational methods.—Given a breakdown into N levels of the individual variable (Z) as described in the preceding section, it would be possible to determine the presence of structural effects by correlating Z_{gp} and W at each of the N levels of Z. This requires that each individual be assigned a Z_{gp} score according to the group in which he is located as well as his own individual W score. In the case of our Monte Carlo data, we would have seven separate correlation coefficients (corresponding to the seven levels of individual variable Z). These correlations would not provide information about individual-level effects. Such effects might be detected through the use of intragroup correlations, that is, by correlating Z and W separately within each group (thereby holding group effects constant).

Each of the above correlational procedures involves holding one variable constant while measuring the relationship between two others. If the particular data to be analyzed meet the necessary statistical requirements, the technique of partial correlation might achieve the same result. This could have the advantage of simplicity and precision. A structural effect could be measured in terms of the correlation between Z_{gp} and W with Z partialed out. An individual effect would be determined by the correlation of Z and W with Z_{gp} partialed out.[10]

A more thorough analysis of the dependent variable W using Z and Z_{gp} as the independent variables could be carried out through multiple-regression techniques. In such an approach, the change in W expected with a unit change in Z_{gp} provides a measure of the structural effect, and the change in W expected with a unit change in Z provides a measure of the individual effect. It is very important in applying either this technique or that of partial correlation to remember the assumption of linearity upon which they are based. Unless the relationships between Z, Z_{gp}, and W are linear, the results of these analyses can be very misleading. However, it may sometimes be possible when the relationships are curvilinear to employ transformations, such as Z^2, log W, $(Z_{gp})^2$, and the like, to achieve the necessary linearity.[11]

[10] Thanks are due to Peter Blau for suggesting this possibility. Hubert M. Blalock, Jr., explains that "the partial correlation coefficient can be interpreted as a *weighted average* of the correlation coefficients that would have been obtained had the control variable been divided into very small intervals and separate correlations computed within each of these categories" (*Social Statistics* [New York: McGraw-Hill Book Co., 1960], p. 332).

[11] The multiple regression approach is somewhat related to L. A. Goodman's "Some Alternatives

The correlational techniques described thus far are all concerned with predicting the dependent variable (W) at the individual level. Another approach to detecting structural effects makes use of aggregate data such as those in the $N \times M$ table described in the preceding section. Given such a table, the correlations between Z_{gp} and *mean* W can be determined at each of the N levels of Z. In the case of the 7×8 table derived from our Monte Carlo data, we would have seven correlation coefficients (corresponding to the seven levels of individual variable Z). Each correlation would be based upon eight cells, with each cell referring to a certain level of Z_{gp} and the *mean* of the dependent variable W for all individuals located in that cell.

Several cautions should be borne in mind in applying this method. First, while correlations based upon *mean* data can provide information about the overall presence or absence of a structural effect, they cannot be used to estimate how much of the variance in *individual-level* W can be related to Z_{gp}. Second, correlations based upon aggregate data are not directly comparable to intragroup correlations since different N's and different groupings of the data are used; accordingly, their relative magnitudes do not indicate a relative strength of structural as compared to individual effects. Third, a correlation based upon a small number of data points (eight in our illustration) is subject to a great deal of variation due to chance, although this may be somewhat reduced when each of the points is based upon averages. Accordingly, any conclusion concerning the presence or absence of a structural effect should probably be based upon the overall pattern of correlations.[12] On the positive side, the use of aggregative instead of individual data may provide a more stable and accurate estimate of the true effect across groups, since each data point represents the observation of a number of individuals, thus eliminating a large portion of the random variance which occurs at the individual level.[13]

to Ecological Correlation," *American Journal of Sociology*, LXIV (1959), 610–25 (see esp. pp. 623–25); and Dean Harper's Ph.D. dissertation ("Some New Applications of Dichotomous Algebra to Survey Analysis and Latent Structure Analysis" [Columbia University, 1961]).

SOME FURTHER CONSIDERATIONS

A number of problems remain which apply to the original methods of Blau and of Davis *et al.* as well as to the modifications outlined in the preceding section.

The problem of overlap.—It can be seen through examination of Figure 1 and Table 1 that the N's in the four cells of the table are likely to be unequal, depending upon the extent to which the distributions of individual scores within the respective groups overlap. The N's in the four cells approach equality as the two distributions approach each other. But as this statistically desirable condition is approached, the data become meaningless as a basis for demonstrating structural effects; that is, structurally the groups are the same (on the independent variable) when the distributions coincide exactly. On the other hand, as the groups become more and more distinct, it is less and less possible to tell whether or not group effacts are present. The N's in cells 1 and 3 become zero when the two distributions do not overlap at all. This implies, in

[12] A weighted average correlation combining all of the correlations for each Z level may sometimes be justified as a summary measure. In some cases it may be reasonable to derive a weighted average regression curve from the 7 (or N) curves, and a single, more stable correlation may be computed from this (see, e.g., A. S. Tannenbaum and C. G. Smith, "The Effects of Member Influence in an Organization: Phenomenology versus Organization Structure," *Journal of Abnormal and Social Psychology*, 1964 [in press]). While a single correlation obscures distinctions between the types of group compositional effects suggested by Davis *et al.* (*op. cit.*, p. 219), it can indicate a general overriding trend of the data.

[13] Selvin and Hagstrom, *op. cit.*

terms of the scatter diagram of Figure 2, a correlation between Z and Z_{gp} approaching 1.00. It is ironic that this situation, which seems conceptually most felicitous for the discovery of structural effects, precludes their detection by the methods under consideration.

Deviants.—A further qualification can be seen from Figure 1 and Table 1. Individuals in cells 1 and 3 are deviants within their respective groups (at least with respect to their scores on the independent variable), and their responses may be influenced by that fact alone. Thus, when we compare individuals in cell 2 with those in cell 1 we may be comparing "average" members in one group with "deviant" members in another. The same problem applies in the comparison of cells 3 and 4. The importance of this problem cannot be ascertained easily. One can hope that it is not a serious source of contamination in most cases, although we know that deviants are likely to be affected dfferently by group experiences than are average members. The researcher would probably do well to consider its possible effects in terms of the particular variables being analyzed.

Selection.—The manner in which members are selected into groups may influence the relationship between Z_{gp} and W and may create in this way a spurious structural effect. For example, members of fraternities with high average intelligence (Z_{gp}) may have higher grade-point averages (W) than members of low average intelligence fraternities, even when individual intelligence (Z) is held strictly constant. This finding might be interpreted as indicating that being in a group of intelligent students creates better performance. Suppose, however, that certain fraternities maintain a policy of stressing high academic standing. Such a policy could lead to the selection of members directly on the basis of grades. Since intelligence and grades tend to be related, fraternities with such policies would be relatively high in average intelligence, thus producing the spurious structural relationship between average intelligence (Z_{gp}) and grade-point average (W), while holding individual intelligence (Z) constant.

It is probably worth keeping this problem in mind when interpreting group effects, since selection is a common phenomenon in social life. It is not unusual for individuals to join groups whose members are like themselves. Furthermore, even if selection *into* a group is random, selection *out* may be systematic, leaving a non-random selection behind. The various bases for selection may differ from case to case, and the corresponding interpretation of group effects would have to differ accordingly. Obviously, the problem can be completely eliminated in laboratory studies where groups are constructed by random procedures. Many field situations too would seem reasonably safe. The selection processes employed in creating formal work groups in industry, for example, are in many cases irrelevant to the particular variables under study, and these groups can be considered reasonably free of the problem. Certain informal and voluntary groups, however, may be more problematic, but this would depend again upon the variables under investigation.

Structural effects, operations versus concepts.—There is some conceptual haziness about variables which somehow are characterizations of both the organization and the individual. Research in group or organization functioning would do well to distinguish effects which are uniquely structural. While it may be easy enough to denote *conceptually* some variables that apply uniquely to structure and have no meaningful counterparts on the individual level, the fact that much social research must fall back upon measures based on individual responses creates a difficulty. While the concepts may be structural, the measures may be contaminated by individual effects. It is for this reason that the Blau method and that of Davis *et al.* are

important approaches to the discovery of structural effects.

It is interesting to note, however, that Blau's original method, which is an operational approach to the definition of structural effects (and consequently structural characteristics), precludes from consideration, according to Blau, "those aspects of social structure which are not manifestations of frequency distributions, such as the form of government in the community."[14] This type of variable, however, is obviously of great interest to the social researcher. Furthermore, the Blau method *can* be helpful in approaching this type of variable if it is employed not simply as a means of operationally defining structural variables and effects, but as a means of helping to ascertain whether the instrument chosen to measure a structural variable is in fact measuring such a characteristic.

We would like, therefore, to maintain the important distinction between a structural concept and a structural measure. While the concept, for example, may refer to aspects of the organization such as "chain of command," "flexibility," or "distribution of control," which are not manifestations of frequency distributions, the measures may very well be based on distributions, that is, on the responses of individual members.[15] Measures of these structural concepts would be subsumed under Kendall and Lazersfeld's unit datum of Type V where "the unit item characterized the group only" and where "no information is introduced about a single individual."[16] We add simply that, while no information may be introduced about a single individual, information may be introduced *by* individuals. It is for this reason that the Blau method and that of Davis *et al.* can prove helpful.

Structural variables should be chosen first on the basis of their theoretical meaningfulness. Measurement is a second step, and tests of relationships between these variables and others are a third. Measurement of a pure structural effect in this sense might then be gauged by the occurrence of a difference between groups according to one of the above methods and a zero difference within groups. This is, with some modification, the Type II effect described by Davis *et al.* Conceptually, we would attempt to approach in this way the effect of a structural variable which has no meaningful counterpart on the individual level—although all of our measures are obtained at that level.

SUMMARY AND CONCLUSIONS

Since measures of group and organization variables are often based on responses of individuals, it is sometimes difficult to know whether the effects observed are due to structure or due simply to individual characteristics. Blau has suggested a useful approach to this difficulty, but one that appears to contain two problems: (1) it fails to hold individual characteristics strictly constant and thereby makes it possible to obtain spurious structural effects; (2) it fails to hold group characteristics

[14] Blau, "Structural Effects," *op. cit.*, p. 192.

[15] See, e.g., Ellis L. Scott, who is concerned with the causes of error in the perception of the "chain of command" (*Leadership and Perceptions of Organizations* [Research Monograph No. 82 (Ohio State University, Columbus: Bureau of Business Research, Ohio State University, 1956)]). Basil Georgopolous and Arnold S. Tannenbaum measure organizational flexibility by averaging responses of organization members to questions designed to provide estimates of this variable ("A Study of Organizational Effectiveness," *American Sociological Review*, XXII [October, 1957], 534–40). Martin Patchen is concerned with the validity of measures, based on member responses, of distribution of control in organizations ("Alternative Questionnaire Approaches to the Measurement of Influences in Organizations," *American Journal of Sociology*, LXIX [July, 1963], 41–52).

[16] Patricia L. Kendall and Paul F. Lazarsfeld, "Problems of Survey Analysis," in Robert K. Merton and Paul F. Lazarsfeld (eds.), *Continuities in Social Research* (Glencoe, Ill.: Free Press, 1950), pp. 133–96. See also Selvin and Hagstrom's discussion (*op. cit.*) of aggregative and integral properties of groups and their distinction between members as respondents and as informants.

strictly constant, making possible the occurrence of spurious individual-level effects. A technique similar in some respects to Blau's has been developed by Davis *et al.*; this method is susceptible only to problem (1).

We have proposed several modifications of the Blau and the Davis *et al.* methods, making use of more precise matching and correlational techniques. Each of these modified methods involves certain advantages and limitations, and the researcher may want to employ them in combination or modify them further to suit his particular purposes.

Several additional problems have been considered including the effect of deviants, overlap of distributions among groups, and selection. We were also concerned about the purpose of the original methods discussed here, namely, defining structural or compositional effects (and, by implication, structural variables) *operationally*. In the authors' opinion this is not an adequate substitute for the conceptual definition of structural variables; conceptualization should come first. The application of the above techniques could then serve the very useful function of determining whether or not the operations employed can be justified as measures of structural characteristics and effects.

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EXPOSURE TO CHILD-REARING EXPERTS: A STRUCTURAL INTERPRETATION OF CLASS-COLOR DIFFERENCES[1]

ZENA SMITH BLAU

ABSTRACT

Negro mothers expose themselves less than white mothers to child-rearing literature and other sources of information, regardless of class position and, in most instances, of educational level, but they are more often favorable toward experts. Ambivalence toward experts rises with exposure to their writings in all class-color groups, while hostility is inversely related to exposure among whites but not among Negroes. In a social milieu where reading the writings of experts is prevalent, individuals who do not follow the pattern feel constrained to justify their deviance by deprecating experts, but this is not the case where the pattern is rare. Differences in the prevalence of this pattern among middle-class whites and Negroes are conditioned by another structural variable—the proportion of stationary and mobile members among them. The significance of these differences for acculturation processes is discussed.

The writings of experts bearing on diverse realms of behavior, transmitted by the mass media, constitute a major mechanism for the diffusion of new information and ideas in contemporary societies. As yet, however, we have achieved little systematic understanding of the social processes by which exposure to this source of innovation comes about, or about the related problem of the processes that lead to the adoption of the ideas advocated.

In a recent article reviewing the present status of research concerning the diffusion of innovation Katz, Levin, and Hamilton comment that "very few studies have been done on the basic problem of comparing the ways in which different kinds of structural arrangements within a group condition the diffusion of a given item."[2] They go on to suggest some of the ways in which social structures may condition the diffusion process. Their idea that social structure implies, among other things, the existence of boundaries which differentiate "the frequency and character of social relations"[3] and thus constitute barriers to diffusion comes very close to the central problem with which the present paper deals—the analysis of selected structural attributes of class-color groups and how they facilitate or hinder the exposure of their members to the writings of a body of experts in the realm of child-rearing. More specifically, the substantive problem is to provide a structural interpretation of a pattern of differences between Negro and white middle- and working-class mothers in their exposure to the writings of child-rearing experts, in their attendance at child-care classes, and in their attitudes toward experts.

Child-rearing studies have repeatedly shown that the class position and educational level of mothers condition their exposure to diverse sources of formal and informal information and advice.[4] Although

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[2] Elihu Katz, Martin L. Levin, and Herbert Hamilton, "Traditions of Research on the Diffusion of Innovation," *American Sociological Review*, XXVIII (April, 1963), 248.

[3] *Ibid.*, p. 247.

[4] See, e.g., Martha Sturm White, "Social Class, Child Rearing Practices, and Child Behavior," *American Sociological Review*, XXII (December, 1957), 704–12; Melvin L. Kohn, "Social Class and

these findings were based, for the most part, on samples of white women, there was every reason to believe that they would apply as well to Negro women of similar class position and educational level.

Indeed, color differences in the realm of child-rearing had largely ceased to be a matter of specific research interest after Davis and Havighurst reported, in a pioneering study published in 1946, that they found few differences in the child-rearing practices of Negro and white mothers who occupied similar class positions.[5] Although their study did not deal with the problem of exposure to experts' writings it seems to have been assumed by the researchers who followed them that the pattern of findings would extend to this kind of behavior as well. However, in the last decade several studies of class differences in the child-rearing patterns of white mothers have reported findings which contradict those of Davis and Havighurst.[6] Whatever the reason for these discrepancies, they suggest the need for taking a new look at the problem of class-color differences in child-rearing, and especially for bringing to bear on this question some of the new modes of sociological analysis that have been developed since the appearance of the early Chicago study.

The study of which the present paper is a part does not replicate the content of the Davis and Havighurst study, but it has a similar sample design.[7] A quota sample of 224 mothers, selected on the basis of race, class position,[8] and parity, was interviewed during the period of confinement on the maternity floors of three large, centrally located hospitals in Chicago during 1961–62. One section of the interview schedule contained a series of questions about the extent and nature of respondents' exposure to various mass media and to child-rearing content in these sources. Some of these data are presented in the analysis that follows.

EXPOSURE TO CHILD-REARING LITERATURE

An index of the extent of exposure to child-rearing literature was obtained by combining the scores assigned to answers to three questions: the frequency with which respondents read child-rearing articles (1) in their daily newspapers and (2) in magazines, and (3) whether they have read Dr. Benjamin Spock's book, *Baby and Child Care*.[9] "High" exposure

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Parent-Child Relationships: An Interpretation," *American Journal of Sociology*, LXVIII (January, 1963), 471–80; and esp. Urie Bronfenbrenner, "Socialization and Social Class through Time and Space," in Eleanor E. Maccoby, Theodore M. Newcomb, and Eugene L. Hartley (eds.), *Readings in Social Psychology* (New York: Henry Holt & Co., 1958), pp. 400–425, which presents a review of many child-rearing studies and a provocative discussion of how reading the experts may promote changes in child-rearing.

[5] Allison Davis and Robert J. Havighurst, "Social Class and Color Differences in Child Rearing," *American Sociological Review*, XI (1946), 698–710.

[6] E.g., Robert R. Sears, Eleanor E. Maccoby, and Harry Levin, *Patterns of Child Rearing* (Evanston, Ill.: Row, Peterson & Co., 1957), and White, *op. cit.*; for an interpretation of these inconsistencies see Bronfenbrenner, *op. cit.*

[7] The original design called for fifty cases each of middle-class and working-class white and Negro mothers, one third having had only one child and the rest having had more than one. However, we could not locate as many Negro middle-class mothers as planned (although we remained in the field longer in an attempt to do so), and decided instead to obtain more interviews with women in the other three class-color categories in order to prevent undue shrinkage of the sample. Conclusions based on so small a sample, particularly of Negro middle-class mothers, are admittedly tentative and are presented merely as hypotheses which still need to be tested on a more adequate sample.

[8] The index of class position is based on husband's occupation. Respondents whose husbands are engaged in non-manual occupations are classified as middle class, and those whose husbands are in manual occupations are defined as working class.

[9] The individual questions were cross-tabulated and scored in the following way: a score of 1 each was given if a respondent regularly read child-rearing articles in a daily newspaper or in any magazine mentioned, and a score of 2 was given

(a score of 3 or 4) signifies regular readership of child-rearing articles in at least one of the mass media, and of Spock's book. "Medium" exposure (a score of 1 or 2) indicates regular readership of such articles in at least one of the mass media, or only of Dr. Spock's book; and "low" exposure (a score of 0) indicates that a respondent has not read Spock and does not ordinarily read child-rearing articles.[10]

Table 1 shows that the extent of exposure to child-rearing literature is influenced by both the class position and color of mothers. White mothers expose themselves more to this kind of literature than Negro mothers, both in the middle and in the working class, and exposure is more prevalent in the middle class than in the working class, independent of skin color. In other words, the woman with no regular exposure to child-rearing literature is highly exceptional in the white middle class, considerably less so in the Negro middle class and white working class, but in a majority in the Negro working class.[11]

The difference between Negro and white mothers in *extent* of exposure to child-rearing content stems in part from the difference between them in the *kind* of media to which they expose themselves, specifically their use of Dr. Spock's book. Separate analysis of the proportions who have read this book shows that among white middle-class mothers the over-

TABLE 1

CHILD-REARING MEDIA EXPOSURE SCORES IN FOUR CLASS-COLOR GROUPS

(Per Cent)

Exposure	Middle Class		Working Class	
	White	Negro	White	Negro
Low	13	32	38	56
Medium	39	47	30	38
High	48	21	32	6
Total per cent	100	100	100	100
No. of cases	(83)	(19)	(56)	(66)

to those who had read Dr. Spock's book, on the assumption that the latter covers at least as wide a range of content and has at least as much impact as the other two sources taken together. Respondents were also asked about their exposure to the well-known pamphlet, *Infant Care*, published by the U.S. Children's Bureau, but so few had read it (33 respondents) that it was not included in the exposure index. Two open-ended questions concerning other sources read by respondents yielded even fewer returns.

[10] No value judgment that mothers ought to read child-rearing literature is implied here, nor are any a priori assumptions made about the impact of this literature on the child-rearing practices of mothers variously located in the social structure. The latter problem will be dealt with in a forthcoming paper.

[11] The Negro-white differences in exposure to child-rearing content are not due to differences in extent of exposure per se to newspapers and magazines. Analysis of scores on a mass-media exposure index, based on the frequency of newspaper and magazine reading, indicates that the exposure of respondents varies primarily with class position and only slightly with color. Thus the proportions of respondents who read both a newspaper and a magazine regularly, or one regularly and the other occasionally, are 82 per cent among whites and 79 per cent among Negroes in the middle class and 59 per cent and 54 per cent, respectively, in the working class. Also of interest is our finding that none of the Negro respondents exposes herself *exclusively* to Negro publications. Only five respondents read the *Chicago Defender*, the local daily Negro newspaper (which, incidentally, does not carry a column on child care) and all these also read at least one of the four daily Chicago newspapers.

whelming majority (77 per cent) have read the book, but in the Negro middle class the proportion is strikingly smaller (32 per cent); indeed it is lower than in the white working class (48 per cent). The smallest proportion of Spock readers is found among Negro working-class mothers (12 per cent).[12]

It is well known, of course, that book readership is more widespread in the middle class than in the working class, particularly among the better educated. Higher education, in turn, is more widespread among whites than among Negroes in the middle class. For example, among middle-class respondents in our study only 8 per cent of the whites but 37 per cent of the Negroes have not completed high school, and 69 per cent and 21 per cent, respectively, have had some college education. In the working class, on the other hand, the level of education of white and Negro mothers is virtually identical. Fifty-five per cent in each color group have not completed high school; and 9 per cent and 8 per cent, respectively, have had some college education. But the differences in exposure to Dr. Spock's book between Negroes and whites persist, for the most part, even when educational background of respondents is controlled. Table 2 shows that at each educational level in both classes the proportion of mothers who have read Spock is higher among whites than among Negroes, although in the middle class the size of the differences between the two color groups diminishes considerably as educational level rises. In the working class, the difference in readership between Negroes and whites is greatest among high-school graduates. It can also be seen that, while each of the three variables—class, color, and educational level—independently affects exposure to Spock's book, the magnitude of differences between Negro and white mothers in most cases is greater than between mothers of comparable educational background in the two classes,

TABLE 2

PERCENTAGE WHO READ SPOCK, BY CLASS, COLOR, AND EDUCATIONAL LEVEL

Class and Color	Education*						Difference	
	Grades 8–11 (1)		High-School Graduate (2)		Attended College (3)		Col. (2) Minus Col. (1)	Col. (3) Minus Col. (2)
Middle class:								
White	57	(7)	68	(19)	82	(57)	11	14
Negro	0	(7)	37	(8)	†		37	....
Difference	57		31		....		....	....
Working class:								
White	39	(31)	60	(20)	60	(5)	21	0
Negro	8	(36)	12	(35)	40	(5)	4	28
Difference	31		48		20		....	....

* N's are given in parentheses.

† Three of the four Negro middle-class respondents who attended college have read Dr. Spock's book.

[12] Respondents were first asked whether they had ever heard of Dr. Spock's book. The proportions who had never heard of the book were only 5 per cent in the white middle class, but 37 per cent in both the Negro middle class and the white working class, and 65 per cent in the Negro working class. It is interesting that, although identical proportions of white working-class and Negro middle-class women knew about the book, fewer in the latter group had read it.

or between respondents at different educational levels within the same class. It is noteworthy that in the middle class the differences in the proportions of Negro and white mothers who have read Spock diminish considerably as educational level increases. But in the working class the largest difference between Negro and white mothers occurs among the high-school graduates, because the proportion of white mothers who have read Spock is as high in this group as among those with some college education, whereas among Negro mothers this proportion is considerably higher among the college-educated than among high school graduates. In other words, readership of this source of child-rearing information varies more with education among Negro than among white mothers. But it is apparent that educational differences do not account for the large variance between the two color groups with respect to readership of Spock's book.[13]

ATTENDANCE AT MOTHERS' CLASSES

The pattern of differences between Negro and white mothers in exposure to child-rearing literature also extends to their attendance at mothers' classes, which many hospitals run for the benefit of maternity patients. Patients are ordinarily invited to attend classes during either their pregnancy or confinement, and in some cases at both times. The decision to attend or not is left up to the patient with the result, as a number of studies have shown, that middle-class women attend these classes more frequently than working-class women.[14]

Respondents in the present study were asked: "Have you ever attended any classes or groups dealing with the care of infants either before or since you have had your baby?" Mothers who had not attended classes were asked: "Were you ever approached or invited to join such a group or class?" These questions were asked to ascertain whether differences in attendance might reflect class or color bias on the part of hospital personnel rather than self-selection on the part of respondents themselves.

Table 3 shows that higher proportions of white than of Negro mothers attended classes, regardless of class position, and that attendance varied little with class

TABLE 3

ATTENDANCE AT MOTHERS' CLASSES, BY CLASS AND COLOR

(Per Cent)

Invited	Attended	Middle Class		Working Class	
		White	Negro	White	Negro
Yes	Yes	52	21	42	21
	No	13	63	18	38
No	No	35	16	38	36
No answer	No	0	0	2	5
Total per cent		100	100	100	100
No. of cases		83	19	56	66

[13] Similar differences occur between Negro and white respondents of like class position and educational background on the composite index of exposure (see Table 6).

[14] See, e.g., D. Mann, L. Woodward, and N. Joseph, *Educating Expectant Parents* (New York: Visiting Nurse Service, 1961), and A. Yankauer, W. Boek, E. Shaffer, and D. Clark, "What Mothers Say about Childbearing and Parent Classes," *Nursing Outlook*, VIII (October, 1960), 563–65.

position. The differences between the two color groups evidently do not reflect discriminatory treatment of Negro maternity patients, since the proportion of those not invited to join classes is no higher among Negro than among white respondents.

ATTITUDES TOWARD EXPERTS

The question arises whether the pattern of differences between Negro and white mothers in their exposure to formal sources of information may simply reflect different evaluations on their part of the importance of expert advice in the realm of child care and child-rearing. One might expect that the greater exposure of white mothers, particularly those in the middle class, to various sources of information reflects a belief on their part that they can thereby enhance the effectiveness of their behavior in the maternal role, while Negro women may expose themselves less to such sources because they are less inclined to share this opinion. The data, however, contradict this assumption.

Respondents were asked: "Some mothers feel it's important to find out what the experts (like doctors, psychologists, etc.) have to say about raising children while others don't think that is necessary. What do you think?" Responses were classified as generally favorable (e.g., "experts have more knowledge or experience"), unfavorable (e.g., "I don't believe in raising children by the book" or "mothers know best"), or ambivalent (e.g., "it does not hurt to get their ideas, but I'll use my own judgment"). Table 4 shows, contrary to expectation, that favorable attitudes toward child-rearing experts are expressed more frequently by Negro mothers in both the middle and the working class. Negative sentiments, on the other hand, occur slightly more often among whites. Ambivalent attitudes are more frequent among middle-class women, both white and Negro, than among those in the working class.

TABLE 4

ATTITUDE TOWARD EXPERTS, BY CLASS AND COLOR

(Per Cent)

Attitude toward Child-rearing Experts	Middle Class		Working Class	
	White	Negro	White	Negro
Favorable	45	63	52	62
Ambivalent	23	16	12	5
Unfavorable	31	21	32	27
NA	1	0	4	6
Total per cent	100	100	100	100
No. of cases	83	19	56	66

That Negro women express favorable attitudes toward child-rearing experts more often than white women, regardless of class position, but typically expose themselves less to such informational sources seems contradictory and might even be dismissed as simply another instance of the known tendency of respondents in low-prestige groups to express agreement more readily, regardless of item content, in the interview situation.[15] But some recent evidence of a similar order suggests another explanation, which turns

[15] See, e.g., Gerhard E. Lenski and John C. Leggett, "Caste, Class, and Deference in the Research Interview," *American Journal of Sociology*, LXV (March, 1960), 463–67.

out to be more fruitful for understanding the dynamic interplay of attitudes and behavior toward experts among mothers in different social contexts.

In a recent National Opinion Research Center study of public attitudes toward medical care, Feldman found that people who have more contact with physicians are also more critical of them than those with less contact.[16] More recently, in a study of attitudes toward fluoridation in a Massachusetts community, Gamson and Schuman reported that respondents who accord physicians high-prestige rankings in comparison with other professionals also express hostile sentiments toward them more frequently than those who give them lower rankings.[17] One explanation for this ambivalence suggested by the authors is that "the very standards that lead to high prestige may caus physicians to be judged against criteri that are exceedingly difficult to meet. Th stronger a respondent feels about the im portance of such standards the more h is likely to accord prestige to physician as against other occupations but to judg physicians severely by these same stand ards."[18]

By the same token, white mothers ma be more prone than Negro mothers to ex press ambivalence toward child-rearin experts precisely because they depend mor on them for guidance, as indicated by th fact that they expose themselves more to th writings of experts. Indeed, a comparison o mothers' attitudes toward experts accordin to mothers' exposure scores in each of th four class-color groups lends support t this interpretation (see Table 5). It show that in each class-color group, the propor tion who express ambivalence toward ex perts is greater among respondents wh have high- or medium-exposure score than among those with low-exposur scores. The original differences noted i Table 4 between Negro and white mother

TABLE 5

PER CENT AMBIVALENT AND PER CENT UNFAVORABLE TOWARD EXPERTS, BY EXPOSURE SCORE, CLASS, AND COLOR

Exposure	Middle Class				Working Class			
	White		Negro		White		Negro	
Per cent of total with medium and high scores........	87		68		63		44	
	Per Cent Ambivalent							
Low...	18	(11)	0	(6)	10	(21)	0	(37)
Medium and high..	24	(72)	23	(13)	14	(35)	10	(29)
	Per Cent Unfavorable							
Low........	55	(11)	17	(6)	43	(21)	33	(37)
Medium and high......	28	(72)	23	(13)	26	(35)	21	(29)

[16] Jacob J. Feldman, "What Americans Think about Their Medical Care," American Statistical Association, Proceedings of the Social Statistics Section Meeting (December, 1958).

[17] William A. Gamson and Howard Schuman, "Some Undercurrents in the Prestige of Physicians," *American Journal of Sociology*, LXVIII (January, 1963), 463–70.

[18] *Ibid.*, p. 469.

virtually disappear in the middle class, and become smaller in the working class, among respondents who expose themselves to experts' writings.

Among those with low exposure to this literature, on the contrary, the differences in ambivalence and particularly in negativism between white and Negro women become *more* pronounced. Unfavorable sentiments are voiced more often by mothers who do not read the experts in three of the four groups—all except the middle-class Negroes. But among mothers with low exposure negative attitudes are expressed considerably more often by whites, particularly in the middle class, than by Negroes. This suggests that the woman who does not "read the experts" but is located in a social milieu where this practice is prevalent feels called upon to justify her deviance by denigrating experts. Thus, in the white middle class where the pattern of reading child-rearing literature is most prevalent, women with low-exposure scores are most often negative toward experts.[19] In the white working class where this pattern is less widespread women with low exposure scores exhibit negative attitudes correspondingly less often. Exposure to experts' writings is least prevalent among Negro mothers in the working class. Consequently, Negro women who do not read this literature do not feel constrained to rationalize their indifference to expert opinion by denying its value. This difference in social context may well explain why Negro women with lower exposure scores express hostility toward experts considerably less often than their white counterparts.

CLASS MOBILITY AND COLOR DIFFERENCES

The pattern of high exposure to the writings of child-rearing experts is more prevalent among white middle-class mothers than among Negro middle-class mothers with a similar amount of formal education (see Table 6).[20] In fact, even white working-class women who have not completed high school have high-exposure scores nearly as often as women with more education in the Negro middle class. Differences in the constitution of the middle classes and the working classes in the two color groups, and their implications for acculturation to middle-class modes of behavior, may help to explain the differences noted above.

Reliance on experts' writings is part of a larger complex of orientations and modes of behavior that differentiate the child-rearing patterns of middle-class mothers from those in the working class in white society.[21] We would therefore expect to find this pattern more prevalent among women of middle-class origin than among those of working-class origin who have moved into the middle class.[22] Table 7

[19] The proportion of mothers in the Negro middle class who express negative sentiments is lower than in the white middle class, as expected. But contrary to expectation, it is also lower than in the other strata.

[20] The sample contains four cases of Negro middle-class mothers who have had some college education. Only one of them has a high-exposure score compared to 58 per cent among *white* middle-class respondents who have attended college.

[21] That this practice is in fact more prevalent in the white middle than working class has been shown by a number of studies besides this one. See, e.g., White, *op. cit.*, and Kohn, *op. cit.* The latter study also contains a suggestive interpretation of these observed patterns of differences as reflections of differences in the value systems that prevail among the two strata.

[22] Class origin is defined by father's occupation when the respondent was sixteen years old. Respondents are classified as stationary members of the middle class if their fathers and husbands are in non-manual occupations, and as upwardly mobile members if their father did manual work but their husband does non-manual work. In the working class, downwardly mobile respondents are those whose fathers did non-manual work but whose husbands are manual workers, and stationary respondents are those whose fathers and husbands are manual workers. There is some variation in the age composition of these groups. Thus, the proportion over twenty-five years old in the white middle class is 58 per cent among stationary respondents and 45 per cent among the upward mobiles; in the white working class this proportion

shows that high exposure is in fact considerably more frequent among the stationary members of the white middle class (59 per cent) than among the upwardly mobile (32 per cent). Her newly won middle-class status does not automatically lead a woman to emulate the less visible forms of behavior that prevail among her established class peers. The acculturation process in this realm of behavior, as in others, requires opportunities for association with established members of the middle class in the course of which social pressure can be exerted upon the new members to adopt middle-class ways. Since the stationary members constitute the majority in the white middle class, such social opportunities would seem to be readily available to the upwardly mobile woman. And the fact that the tenure of the stationary members in the middle class has been of longer duration and that they are apt to be better educated further enhances their ability to influence the incoming members of their group. Thus, the pattern of reliance on experts could be expected to spread by degrees among the upwardly mobile as they acquire longer tenure in their new position, resulting in closer conformity to the behavior of their stationary peers.

A comparison of the exposure scores of upwardly mobile white women with those of similar origin who have remained in the working class constitutes a crude test of this hypothesis. Although there is little difference in high exposure, the proportion with some exposure (high or medium) is considerably greater among the upwardly mobile (89 per cent) than among stationary members of the working class (55 per cent), which indicates that the former have assimilated the middle-class pattern to some degree—although not to the full extent, since *high* exposure is less widespread among them than among the stationary members of the middle class. Thus, the data lend support to our hypothesis concerning the acculturation effects that accompany upward mobility into the white middle class.

The constitution of the Negro middle class differs sharply from that of the white middle class, and therein may lie the explanation, at least to some degree, of the differences in the extent to which their members expose themselves to child-rearing literature. Owing to the long history of pervasive economic and social discrimination against Negroes in our society the size of the established Negro middle class has traditionally been much smaller than its white counterpart, both in absolute and in relative terms. Since World War II, however, employment opportunities for Negroes in non-manual occupations have increased with a corresponding increase in the numbers who have recently moved from the working class into the middle class.[23] Indeed, in our sample fully 90 per cent of the middle-class Negro women come from working-class backgrounds, in contrast to 35 per cent in the white middle class. Owing to their insignificant number,[24] the stationary members of the Negro middle class are not in a position to exert any appreciable effect on the

is 30 per cent among stationary respondents and 27 per cent among downward mobiles; and among Negroes it is 24 per cent among the upward mobiles and 44 per cent among stationary working-class respondents. However, the pattern of differences in exposure shown in Table 7 persists even when age is controlled (except that there are too few cases of older upwardly mobile Negroes to make meaningful comparisons).

[23] This is not meant to imply, of course, that the size of the Negro middle class approaches that of the white middle class, even relative to the total Negro population, but only that the proportion of the Negro middle class with working-class origins is larger than the corresponding proportion in the white middle class. The analysis assumes that this is true for the Chicago population at large as well as for our sample, which is admittedly small and not representative.

[24] The tiny number of stationary middle-class Negroes (2) and of downwardly mobile working-class Negroes (1) in our sample makes impossible some comparisons corresponding to those in the white group and results in some unavoidable gaps in the analysis.

TABLE 6

PERCENTAGE DISTRIBUTION OF EXPOSURE SCORES, BY CLASS, EDUCATION, AND COLOR

EDUCATION AND COLOR	EXPOSURE			TOTAL N
	High	Medium	Low	
	Middle Class			
Grades 8–11:				
White	29	29	42	7
Negro	0	43	57	7
High-school graduate or higher:				
White	50	40	10	76
Negro	33	50	17	12
	Working Class			
Grades 8–11:				
White	29	19	52	31
Negro	6	30	64	36
High-school graduate or higher:				
White	36	44	20	25
Negro	6	47	47	30

TABLE 7

PERCENTAGE DISTRIBUTION OF EXPOSURE SCORES, BY CLASS, COLOR, AND MOBILITY

CLASS POSITION AND ORIGIN	PER CENT	EXPOSURE				TOTAL N*
		High	Medium	Low	Total	
		White				
Middle class:						
Stationary	65	59	28	13	100	53
Upward mobile	35	32	57	11	100	28
Working class:						
Stationary	79	29	26	45	100	42
Downward mobile	21	36	55	9	100	11
		Negro				
Middle class:						
Stationary	10	....	†	....	†	2
Upward mobile	90	24	41	35	100	17
Working class:						
Stationary	98	7	38	55	100	60
Downward mobile	2	....	†	....	†	1

* The class origin of ten respondents could not be ascertained, and these cases are excluded from the analysis.

† Among the Negro respondents are only two cases of stationary middle-class members and one case of a down-

behavior of the upwardly mobile members of their class. And since the barriers of segregation allow for little, if any, informal association between members of the two color groups the upwardly mobile Negro woman is also cut off from the influence exerted by stationary members of the white middle class.

As a result, the acculturation of Negro upwardly mobile women to the modes of behavior that prevail in the middle class is likely to proceed at a slower pace than in the case of upwardly mobile whites, and this is exemplified by the differences between them in the extent of their exposure to the writings of child-rearing experts (see Table 7). In contrast to only one-tenth of the upwardly mobile whites, over one-third of the upwardly mobile Negroes have low exposure to this kind of literature.[25]

Analysis of the composition of the white working class also helps explain why even in this stratum exposure to experts' writings is more widespread than in the Negro middle class, despite the fact that the latter contains a larger proportion of better educated women. Although the large majority (79 per cent) of white working-class respondents are stationary members of their stratum, the rest (21 per cent) were reared in the middle class and subsequently moved down into the working class. A comparison of the exposure scores of the downwardly mobile and stationary members of the white working class (see Table 7) shows that a higher proportion (91 per cent) of the former have high or medium scores than of the latter (55 per cent). And it is the presence of this downwardly mobile contingent that largely accounts for the greater prevalence of high exposure in the white working class than in the Negro middle class. For in the latter group the proportion with low-exposure scores (35 per cent) is less than among the stationary members of the white working class (45 per cent). But downwardly mobile white working-class mothers expose themselves considerably more to experts' writings than upwardly mobile Negro middle-class women, an indication that former social ties with her middle-class family and friends are maintained to some extent by the downwardly mobile woman who thereby remains subject to middle-class influences to a greater degree than the upwardly mobile Negro middle-class woman. But the data also suggest that the impact of middle-class influence on downwardly mobile women has waned, since only 36 per cent of them have high exposure to child-rearing literature in contrast to 59 per cent of their former class peers, those who have retained their middle-class status.

However, although the downwardly mobile white mother shows evidence of being negatively influenced by her association with her new class peers, she may also act, to some degree, as a carrier of middle-class patterns of behavior to the stationary members of the working class with whom she develops social ties. For the stationary members of the white working class expose themselves more to child-rearing literature than stationary members of the Negro working class, despite the fact that the educational level of the members of the two strata is similar. But in the Negro working class in our sample there are virtually no downwardly mobile respondents (2 per cent) and consequently here the chances of exposure to middle-class social influences are most limited.

SUMMARY AND CONCLUSIONS

Exposure to informational sources in the realm of child-rearing was shown to be more widespread among white than among Negro mothers, regardless of class position. This pattern of Negro-white differences persists even when respondents'

[25] When age is controlled this difference becomes even more pronounced among younger women (under twenty-six): only 27 per cent of the upwardly mobile whites compared to 77 per cent of the upwardly mobile Negroes have low-exposure scores.

educational level is taken into account. But Negro women tend to express favorable sentiments toward child-rearing experts more often than white women, despite the fact that they expose themselves less to their writings. Simultaneous analysis of these two variables revealed that in all four class-color groups women with high or medium exposure were more prone to express ambivalence toward experts than those with little or no exposure. But an interesting pattern of differences was observed in the incidence of negative attitudes between the two color groups: among women with low exposure, whites were more often negative toward experts than Negroes, but no such differences were noted among women with higher exposure. This suggests that the prevalence of a pattern of behavior in a group conditions the relationship between behavior and attitudes among its individual members. Where reading child-rearing literature is a widespread practice, as it is among white mothers, the very prevalence of this pattern within the group operates as a pressure toward conformity upon the individual. In *this* social context, *not* to read the experts constitutes a deviant act, and women who do not conform to the pattern of exposure feel constrained to justify their deviance by denigrating experts. But in a group where this practice is rare, as is the case among Negro mothers, the woman who does not read the experts is under no social or psychological pressure to provide rationalizations for her abstinence.

Analysis of the differences in the proportions of stationary and upwardly mobile members in the two middle classes helps to explain why the pattern of reading child-rearing literature is less widespread among Negro women, even the better educated Negroes, than among whites. The existence of a stationary majority in the white stratum creates numerous opportunities for upwardly mobile women to become exposed to middle-class modes of behavior such as "reading the experts." And association with these better educated, more prestigeful members in their stratum constitutes the source of social pressure through which new members become acculturated to these middle-class ways. In contrast, the Negro middle class contains an overwhelming majority of upwardly mobile members. Thus the new member has fewer opportunities than her white counterpart for exposure to, and assimilation of, middle-class modes of behavior within her own color group, and the barriers of segregation forestall her exposure to these influences through association with the stationary members of the white middle class. Indeed, even white working-class mothers may have a better chance of exposure to middle-class influences through their association with the downwardly mobile members of their stratum. But the Negro working-class mother is removed from even this source of influence, since there are virtually no downwardly mobile women in her stratum.

That economic discrimination and social segregation are major impediments to the acculturation of the Negro masses to urban middle-class culture is well known. Our analysis helps to specify *how* these practices operate to delay this process even among middle-class, better educated Negroes. Cognizant as we all are of the existence of a dual stratification system, we tend to think of the Negro and white class systems as similar though separate. Our findings suggest that there may be a number of structural differences between them that differentiate not only the rate with which middle-class norms diffuse among their members but also the amount of strain that accompanies the acculturation process. These differences in social structure mediate the effects that upward mobility and higher education exert on the attitudes and behavior of their individual members, not only in the realm of child-rearing, but probably in other realms of behavior as well. In other words, although the existence of a dual stratification system constitutes a grave social

liability to a society that espouses democratic values, it constitutes an opportunity, while it persists, for the social scientist to make systematic comparisons between two class systems within a common cultural framework and thereby to gain a better understanding of how specific structural variables, present to a different degree in the two class systems, condition the diffusion of various kinds of norms, practices, and innovations among the members similarly situated within them. By the same token, it would undoubtedly prove fruitful to study the impact of desegregation on these processes by comparing their effects on Negroes who have gained opportunities for informal association with their white counterparts and on those similarly located in their class system for whom such opportunities still do not exist.

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CORRELATES OF PREJUDICE: SOME RACIAL DIFFERENCES AND SIMILARITIES[1]

DONALD L. NOEL AND ALPHONSO PINKNEY

ABSTRACT

Data from 1,430 white and 805 Negro residents of four American cities are analyzed to ascertain racial differences and similarities in the correlates of race prejudice. There is no difference between Negroes and whites regarding the relation of *social-distance* prejudice to education, sex, age, marital status, interracial contact, and authoritarianism. In contrast, occupational status and social participation relate negatively to anti-Negro prejudice but are not significantly related to anti-white prejudice. Differences between whites and Negroes in the rates of prejudice and authoritarianism are also noted. Explanations for each of the findings reported are discussed.

The literature of sociology abounds with studies examining the attitudes of native white Americans toward Negroes, but few sociologists have concerned themselves with the attitudes of Negroes toward whites. However, the practical need for studies of anti-white prejudice among Negroes is clear. With the acceleration of desegregation programs in all aspects of American life it becomes increasingly important that we know how Negroes feel about whites just as we know how whites feel about Negroes.

In view of the paucity of studies in this aspect of intergroup relations, and in an effort to compare Negroes and whites insofar as prejudice is concerned, we have analyzed data collected as part of the Cornell Studies in Intergroup Relations from a sample of 1,430 whites and a sample of 805 Negroes. The respondents approximate random samples of the white and Negro populations of four American cities—Bakersfield, California; Elmira, New York; Savannah, Georgia; and Steubenville, Ohio. These four cities were selected to represent widely differing "social climates" for intergroup relations. In this paper we are not concerned with community comparisons, however, but with *racial* comparisons. In the absence of previous comparative studies of this type we will stress the racial differential in support provided for the following generalizations derived from the existing literature on race prejudice:

1. Socioeconomic status is negatively related to prejudice.
2. Age, marital status, and sex are not related to prejudice in a consistent manner.
3. Social participation (i.e., church attendance) is positively related to prejudice.
4. Equal-status contact with out-group persons is negatively related to prejudice.
5. Authoritarianism is positively related to prejudice.

As the basic measure of prejudice in this study we use a modified social-distance scale. All respondents in both the Negro and white samples (with the exception of whites in Savannah) were asked the following questions:

Do you think you would ever find it a little distasteful:

1. to eat at the same table with a white person (Negro)?
2. to dance with a white person (Negro)?
3. to go to a party and find that most of the people are white (Negro)?
4. to have a white person (Negro) marry someone in your family?

In light of the closer approximation to racial caste in the South a set of items in-

[1] We are indebted to the Cornell Studies in Intergroup Relations for the data used in this paper. The Cornell Studies, conducted between 1948 and 1952, were supported by grants from the Rockefeller Foundation and directed by John P. Dean, Edward A. Suchman, and Robin M. Williams, Jr. An earlier version of this paper was read at the 1961 meeting of the American Sociological Association.

volving less intimate interaction was used to measure prejudice among Savannah whites. This index includes Item 1, above, plus "to shake hands with a Negro," "to sit beside a Negro on a bus," and "to have a Negro family living next door to you."

The responses to the four-item set constitute the basic measure of anti-Negro prejudice, but in measuring anti-white prejudice the "party" item was deleted in order to obtain a three-item score which conforms to the pattern of a Guttman scale. The break between high and low prejudice also varies slightly between samples. Thus in Savannah a Negro must give an affirmative reply to two or all three of the index items to be considered high in prejudice, but in the other three cities a Negro is considered high in prejudice if he affirms even one of the items. Among the whites, those respondents who affirm three or all four of the index items are considered high in prejudice. All other persons are considered low in prejudice. Parenthetically, it is noteworthy that 41 per cent of the Negroes interviewed received the lowest possible prejudice score (i.e., answered *all* of the index items negatively) and only 17 per cent received the highest possible score (i.e., answered *all* of the index items positively), whereas among the whites only 5 per cent received the lowest possible score and 48 per cent received the highest possible. Even when appropriately adjusted for the previously mentioned difference in number of scoring categories, this finding indicates a racial difference which is highly significant both statistically and substantively. Moreover, the difference is clear-cut at all socioeconomic levels (see Tables 1 and 2).

SOCIOECONOMIC STATUS AND PREJUDICE

Early studies of the relationship between status (variously indexed) and anti-Negro prejudice indicated that class differences in prejudice were minute, but more recent studies indicate that the two variables are inversely related.[2] This inconsistency reflects partly the use of different indexes of status,[3] and thus in our racial comparisons of the correlates of prejudice we will examine both education and occupation.

The great majority of the existing studies of the relationship between anti-Negro prejudice and amount of formal education show that these variables are negatively correlated,[4] and this relationship is confirmed by the present data. Among both races education—categorized according to whether the respondent attended college, high school, or only grammar school—has a highly significant inverse relationship to prejudice (Table 1).[5] The association of

[2] See Arnold Rose, "Studies in Reduction of Prejudice" (Chicago: American Council on Race Relations, 1947) (mimeographed) for a summary of the early studies. Among more recent studies see F. R. Westie, "Negro-White Status Differentials and Social Distance," *American Sociological Review*, XVII (1952), 550–58; H. C. Triandis and L. M. Triandis, "Race, Social Class, Religion, and Nationality as Determinants of Social Distance," *Journal of Abnormal and Social Psychology*, LXI (1960), 110–18; and Melvin Tumin, Paul Barton, and Bernie Burrus, "Education, Prejudice and Discrimination," *American Sociological Review*, XXIII (1958), 41–49.

[3] See John Harding, Bernard Kutner, Harold Proshansky, and Isidor Chein, "Prejudice and Ethnic Relations," in G. Lindzey (ed.), *Handbook of Social Psychology* (Cambridge, Mass.: Addison-Wesley Press, 1954), p. 1039.

[4] This must be qualified, however, in light of a recent re-analysis of over a score of studies which indicates that the relation of education to (anti-Jewish and anti-Negro) prejudice varies with the dimension of prejudice under consideration. The author reports, in contrast with the present finding, that the better educated are *more* likely to be prejudiced insofar as rejecting intimate relations with Negroes and Jews is concerned. On the other hand, the better educated are less likely to endorse stereotypes (see C. H. Stember, *The Effect of Education on Prejudice against Minority Groups* [New York: Institute on Human Relations, American Jewish Committee, 1960]).

[5] The various subgroup differences in the proportion prejudiced reported in this paper have been tested for statistical significance by the use of the non-parametric χ^2 technique. A difference will be defined as significant only when the distributions differ at the 0.05 level or beyond. In all cases we have applied a two-tailed test of sig-

increasing education with a decreasing proportion prejudiced is uniform among Negroes, but among whites the effect of education is relatively slight until the college level is reached.[8] The present data do not, however, allow us to determine whether a specified increase in formal education actually reduces prejudice or whether the more educated are simply more circumspect about revealing their prejudices,[7] but it is clear that the more education the less the likelihood that either Negroes or whites will manifest prejudice.

In contrast, our second index of status—occupation—yields a marked racial difference (Table 2). When occupations are classified as high (professionals and propri-

nificance corrected for continuity whenever $df = 1$ as recommended by A. L. Edwards, *Experimental Design in Psychological Research* (New York: Rinehart & Co., 1950), pp. 64, 82, 99 n.

[8] This finding is similar to the one reported by Tumin *et al., op. cit.,* based on their study of a sample of North Carolina whites.

[7] See L. A. Kahn, "The Organization of Attitudes toward the Negro as a Function of Education," *Psychological Monographs*, LXV, No. 131 (1951), 1–39.

TABLE 1

PREJUDICE BY RACE AND EDUCATION

Prejudice	Education* (Per Cent)					
	Negro			White		
	High	Medium	Low	High	Medium	Low
High	33	47	60	54	75	83
Low	67	53	40	46	25	17
N	(92)	(299)	(412)	(280)	(769)	(367)
	$\chi^2=27.5$, $df=2$, $p<.001$			$\chi^2=72.4$, $df=2$, $p<.001$		

* Those respondents who attended or graduated from college are considered high in education, those who attended or graduated from high school are considered medium in education, and those who attended grammar school only are considered low in education.

TABLE 2

PREJUDICE BY RACE AND OCCUPATION

Prejudice	Occupation* (Per Cent)					
	Negro			White		
	High	Medium	Low	High	Medium	Low
High	53	40	50	66	73	80
Low	47	60	50	34	27	20
N	(38)	(63)	(261)	(109)	(273)	(187)
	$\chi^2=2.47$, $df=2$, $p<.30$			$\chi^2=7.4$, $df=2$, $p<.05$		

* The reduction of sample size for both Negroes and whites resulted because not all the respondents were employed. Also the Elmira samples are not included in this table as occupational data are not available for the sample of Negroes in that city.

etors), medium (white-collar and skilled), and low status (semiskilled and unskilled), we find among whites that the higher the occupational status, the lower the proportion prejudiced. Among Negroes, however, the relationship between occupation and prejudice tends to be curvilinear. Thus there is no difference between high-status and low-status Negroes in the proportion prejudiced, but a considerably smaller proportion of the middle-status group score high in prejudice. Previous studies of the relationship between occupation and prejudice reveal inconsistent findings among both races. Thus Martin and Westie and Westie and Westie report that high occupational status is associated with low prejudice among whites,[8] while Hunt reports that white laborers are more likely to favor mixed housing than are whites of higher occupational status.[9] Similarly, Westie and Westie report a slight inverse linear relation between occupational status and anti-white prejudice among Negroes, while Cothran reports a curvilinear tendency similar to our finding.[10]

Cothran predicted and interpreted his finding in terms of the mobility ceiling imposed on upper-status Negroes by the caste barrier. This restriction on mobility is conducive to frustrations, leading to prejudice, which are not encountered by middle-status Negroes. The Cornell data provide a partial test which fails to support this interpretation. That is, on two of three items which index frustration, high-status persons are *less* likely to manifest frustration than middle-status persons. Although the frustration explanation may yet prove to be valid, it seems worthwhile to suggest an alternative explanation.

Williams has suggested that antiminority responses are most common "among those classes which are most vulnerable to competition from the minority."[11] Inasmuch as low-status whites frequently experience direct competition with Negroes while middle- and high-status whites rarely do, our finding concerning whites is consistent with this proposition.[12] On the other hand, it seems quite likely that both Negro laborers and Negro professionals and proprietors experience more direct competition with whites than do middle-status Negroes. We suspect that white-collar Negroes in particular are insulated from competition with whites by virtue of employment *within* the Negro community. The available data support this interpretation inasmuch as only 17 per cent of the twenty-four white-collar Negroes in our sample score high in prejudice, whereas high prejudice characterizes 54 per cent of the thirty-nine middle-status Negroes who are skilled workers. Thus our findings concerning the relationship between occupational status and prejudice among Negroes are also consistent with Williams' proposition.

AGE, MARITAL STATUS, SEX, AND PREJUDICE

The literature reveals such contradictory findings regarding the relationship between prejudice and age, sex, and marital status that firm generalizations cannot be made at the present time.[13] We suspect, however, that many of these contradictions could be

[8] J. G. Martin and F. R. Westie, "The Tolerant Personality," *American Sociological Review*, XXIV (1959), 521–28; and F. R. Westie and M. L. Westie, "The Social-Distance Pyramid: Relationships between Caste and Class," *American Journal of Sociology*, LXIII (1957), 190–96.

[9] C. L. Hunt, "Private Integrated Housing in a Medium Size Northern City," *Social Problems*, VII (1959–60), 196–209.

[10] T. C. Cothran, "Negro Conceptions of White People," *American Journal of Sociology*, LVI (1951), 458–67.

[11] Robin M. Williams, Jr., *The Reduction of Intergroup Tensions* (New York: Social Science Research Council, 1947), p. 59.

[12] Obviously the competition theory of prejudice must be supplemented by other theories to account for the high probability of prejudice among whites at all socioeconomic levels (especially as compared to anti-white prejudice among Negroes).

[13] For example, see Rose, *op. cit.*, and G. W. Allport, *The Nature of Prejudice* (Cambridge, Mass.: Addison-Wesley Press, 1954), pp. 79–80.

resolved if the relationships were more rigorously analyzed by employing test variables, exploring unique facets of the sample(s) involved, and considering the dimension of prejudice relevant to each finding. Analyzing the present data in this fashion suggests some interesting hypotheses.

TABLE 3

PREJUDICE BY RACE AND MARITAL STATUS

Prejudice	Negro (Per Cent)			White (Per Cent)		
	Single	Married	Other*	Single	Married	Other*
High	39	52	59	66	72	83
Low	61	48	41	34	28	17
N	(103)	(524)	(171)	(117)	(1,120)	(191)
	$\chi^2=10.4, df=2, p<.01$			$\chi^2=12.7, df=2, p<.01$		

* Includes those who are separated, divorced, or widowed.

TABLE 4

PREJUDICE BY RACE AND AGE

Prejudice	Negro (Per Cent)			White (Per Cent)		
	Young	Middle	Old	Young	Middle	Old
High	49	54	54	74	70	77
Low	51	46	46	26	30	23
N	(302)	(340)	(146)	(473)	(589)	(346)
	$\chi^2=2.0, df=2, p<.40$			$\chi^2=5.0, df=2, p<.10$		

As regards marital status, single persons are significantly the least likely to be prejudiced among both whites and Negroes (Table 3). Moreover, this relationship remains when education is held constant. Thus we are led to speculate, along lines suggested by Durkheim, that single persons are less well integrated into the group; and therefore, if we assume that the norms of both the Negro and the white community endorse prejudice, they are less likely to be prejudiced. This explanation does not, however, account for the significantly greater likelihood of prejudice among whites who are separated, divorced, or widowed than among whites who are married. The variation in prejudice by marital status persists when age is held constant. Indeed when the respondents are categorized as young (18–34), middle-aged (35–54), and old (55 and over), the overall relationship between age and prejudice is not significant among either Negroes or whites (Table 4). However, among the whites the middle-aged are significantly (.05) less often prejudiced than the old, while the young are intermediate in proportion prejudiced and not significantly different from either the middle-aged or the old.

The relationship between sex and prejudice is highly significant in both samples (Table 5). Females are much more likely

to manifest social-distance prejudice than are males.[14] Moreover, this difference is *not* eliminated by consecutive control on a variety of important variables including education, interracial contact, marital status, and social participation. In each case the sex difference remains clear at all "values" of the control variable and significant in at least one partial.

In a recent study Pettigrew found that southern white females were somewhat more likely to be prejudiced than white males, and he suggested that women as the "carriers of culture" reflect the mores more directly than males and therefore should be expected to be more prejudiced.[15] He deduced, on the same basis, that they would attend church more frequently, and he found this to be the case. Pettigrew's reasoning is clear, but our data call his key assumption (i.e., that women are the "carriers of culture") and, subsequently, his interpretation into question. Among the Negroes females are both more often prejudiced and more likely to attend church regularly than males, as Pettigrew would predict, but among the whites a greater proportion of males than of females attend church regularly. Moreover, females (Negro and white) who attend church are somewhat less likely to be prejudiced than females who do not attend and, among the whites, males who do not attend church are somewhat more likely to be prejudiced than females who do attend. Finally, the "carriers of culture" hypothesis cannot account for the fluctuation in the relation between sex and prejudice dependent upon the dimension or index of prejudice involved. The marked sex difference among Negroes based on the social-distance index of prejudice is reversed when we use a single hostility item ("Sometimes I hate white people") as the index of prejudice. Similarly, white males are significantly more likely than white females to agree that "Generally speaking, Negroes are lazy and ignorant." Five other stereotype items involving various out-groups reveal no significant differences by sex among Negroes or whites.

TABLE 5

PREJUDICE BY RACE AND SEX

PREJUDICE	NEGRO (PER CENT)		WHITE (PER CENT)	
	Female	Male	Female	Male
High	58	44	77	68
Low	42	56	23	32
N	(444)	(361)	(752)	(677)
	$\chi^2 = 14.6, df = 1, p < .001$		$\chi^2 = 13.3, df = 1, p < .001$	

The fact that the greater likelihood of prejudice among females is confined to *social-distance* prejudice suggests a possible explanation for this sex difference. Differences in the amount and intimacy of interracial contact do not account for the variation in prejudice by sex, but males probably also experience a much greater variety of interracial contacts than females do. It seems likely that the intergroup contacts of females typically occur in a rather restricted role change—that is, they are

[14] This is consistent with the findings of two other recent studies of social-distance prejudice (J. B. Edlefsen, "Social Distance Attitudes of Negro College Students," *Phylon*, XVII [1956], 79–83, and E. S. Bogardus, "Race Reactions by Sexes," *Sociology and Social Research*, XLIII [1959], 439–41).

[15] T. F. Pettigrew, "Regional Differences in Anti-Negro Prejudice," *Journal of Abnormal and Social Psychology*, LIX (1959), 28–36.

generally confined to roles which are defined in functionally specific and relatively authoritarian terms. Role relationships exclusively of this type might reasonably be expected to promote a negative attitude toward intergroup *contacts* and a desire to avoid further contacts, including the types specified in our index of prejudice.[16]

SOCIAL PARTICIPATION AND PREJUDICE

The existing research findings pertaining to the relationship between social participation and prejudice generally suggest that these two variables are positively associated.[17] However, the relation of socioeconomic status to prejudice and to participation suggests that they should be negatively related[18] Although we use the same index of participation (i.e., church attendance) as was used in the previous studies, the present data yield findings more compatible with the hypothesized negative relationship (Table 6). Thus whites who attend church frequently (i.e., every Sunday or almost every Sunday) are significantly less likely to be prejudiced than those who attend infrequently. However, among the Negroes there is no difference in the proportion prejudiced between those persons who attend church frequently and those who attend infrequently. The variability of the existing findings demonstrates the importance of distinguishing those churchgoers who are extrinsically religious and therefore likely to be prejudiced from those who are intrinsically religious and therefore likely to be non-prejudiced.[19]

Since the present data indicate that religion reduces anti-Negro but not anti-white prejudice, it appears that intrinsic religiosity is more characteristic of white than of Negro *church attenders*. This inference is not inconsistent with the evidence that there is no over-all racial difference in (intrinsic) religiosity because a significantly ($p < .001$) larger proportion

[16] We are indebted to Ernest A. T. Barth and Robin M. Williams, Jr., for suggesting this explanation of the sex difference in social-distance prejudice. This explanation is also compatible with the recent analysis by D. Stewart and T. Hoult suggesting that the number of roles mastered is inversely related to authoritarianism (see their "A Social Psychological Theory of the Authoritarian Personality," *American Journal of Sociology*, LXV [1959], 274–79).

[17] T. W. Adorno, E. Frenkel-Brunswik, D. J. Levinson, and R. N. Sanford, *The Authoritarian Personality* (New York: Harper & Bros., 1950), p 220; R. L. Simpson, "Factors in the Attitudes of Two Minority Groups toward Each Other" (unpublished M.S. thesis, Cornell University, 1952), p. 77; and Harding *et al.*, *op. cit.*, p. 1039. Pettigrew, *op. cit.*, reports a positive relation between church attendance and prejudice among southern whites but no significant relation among northerners. In contrast, a negative relation between church attendance and prejudice is reported by A. L. Rosenblum in his "Ethnic Prejudice as Related to Social Class and Religiosity," *Sociology and Social Research*, XLIII (1959), 272–75. Most recently J. D. Photiadis and J. Biggar have reported that church participation (as measured by church attendance *and* participation in church activities) is negatively related to prejudice—see their "Religiosity, Education, and Ethnic Distance," *American Journal of Sociology*, LXVIII (1962), 666–72.

[18] The relation of status and prejudice was discussed above while the positive relation between status and participation has been widely noted. Ten studies supporting this relationship are cited in J. C. Scott, Jr., "Membership and Participation in Voluntary Associations," *American Sociological Review*, XXII (1957), 315–26.

[19] The concepts of extrinsic and intrinsic religiosity and their relation to prejudice are elaborated by G. W. Allport in his *Personality and Social Encounter* (Boston: Beacon Press, 1960), pp. 257–67. The intrinsically religious subordinate themselves to their religion while the extrinsically religious subordinate their religion to self-interest. Photiadis and Biggar (*op. cit.*) have shown that extrinsic religiosity is positively related to prejudice as Allport theorizes, but the relationship disappears when personality variables (such as authoritarianism and status concern) are held constant. These variables, however, may be viewed as either concomitants *or causes* of extrinsic religiosity. As an alternative to the extrinsic-intrinsic interpretation of the relation between church attendance and prejudice, William J. Chambliss has suggested that the influence of religion on prejudice is a function of the degree to which the individual church supports, ignores, or attacks definitions of the outgroup which are normative in the relevant racial community.

of Negroes attend church.[20] Indeed, we should expect a larger proportion of Negroes to attend church and a larger proportion of *the attenders* to be extrinsically motivated because of the variety of functions fulfilled primarily or exclusively by the church in the Negro community.[21]

This interpretation suggests that the racial difference in the relation of prejudice and church attendance is a special case not to be generalized to the relation between prejudice and other types of social participation. However, the racial difference remains, although considerably attenuated, when we use membership in organizations as our index of participation (Table 7). Whites who are organization members are less likely to be prejudiced than are non-members, but the relationship does not quite attain significance

[20] Studies by E. T. Prothro and J. A. Jensen indicate that religiosity (indexed by a scale measuring attitude toward the church) is not a racial but a regional characteristic (see their "Comparison of Some Ethnic and Religious Attitudes of Negro and White College Students in the Deep South," *Social Forces*, XXX [1952], 426–28).

[21] On the functions and importance of the church in the Negro community see G. E. Simpson and J. M. Yinger, *Racial and Cultural Minorities* (rev. ed.; New York: Harper & Bros., 1958), pp. 581–89.

TABLE 6

PREJUDICE BY RACE AND CHURCH ATTENDANCE

PREJUDICE	CHURCH ATTENDANCE* (PER CENT)			
	Negro		White	
	Frequent	Infrequent	Frequent	Infrequent
High	49	51	64	72
Low	51	49	36	28
N	(347)	(304)	(378)	(521)
	$\chi^2=0.25, df=1, p<.70$		$\chi^2=6.0, df=1, p<.02$	

* Respondent is classified as a frequent attender if he reports that he attends church every Sunday or almost every Sunday and as an infrequent attender if he attends only occasionally or not at all. The Elmira samples are not included as church attendance data are not available for the sample of whites in that city.

TABLE 7

PREJUDICE BY RACE AND ORGANIZATION MEMBERSHIP

PREJUDICE	ORGANIZATION MEMBER* (PER CENT)			
	Negro		White	
	Yes	No	Yes	No
High	48	55	66	76
Low	52	45	34	24
N	(397)	(408)	(735)	(688)
	$\chi^2=3.7, df=1, p<.06$		$\chi^2=14.1, df=1, p<.001$	

* Respondent is considered to be an organization member if he belongs to one or more social, religious, occupational, fraternal, or service clubs or organizations.

among Negroes. The difference persists when persons who belong to religious organizations only are classified as non-participators. However, the relationship is non-significant in all three education partials among the Negroes, and it continues significant only among the college-educated whites. This leads us to conclude that the racial difference in the relation of prejudice to social participation is spurious.

INTERRACIAL CONTACT AND PREJUDICE

A voluminous literature indicates that equal-status contact is associated with relatively low anti-Negro prejudice among whites, and one recent study reports the same relationship with respect to anti-white prejudice among Negroes.[22] To use the Cornell data to test this generalization we initially made the assumption that "friends" would be equal in status. Respondents in both samples were asked whether there were any white people (or Negroes) in their neighborhood, in their organizations, at their place of work, or in any other area of face-to-face contact whom they considered to be among their good or best friends. A person was considered to have experienced equal-status interracial contact if he claimed one or more whites (or Negroes) in one or more of these areas as a good or best friend.

As expected, whites who reported interracial contact of this nature were much less likely to to be high in prejudice than those who did not report such contact (Table 8). In contrast, Negroes who reported friendships with whites were *not* significantly less likely to be prejudiced than those without white friends. The difference found, although in the expected direction, could have occurred by chance nine times in one hundred. The theoretical and practical implications of this finding prompted further consideration of the contact-prejudice relationship among Negroes.

If Negroes view their white friends as "different" from most white people, we could hardly expect contact with these friends to have a significant effect on anti-white prejudice. Thus, the absence of the expected relationship between contact and prejudice might simply reflect operation of the exemption mechanism. However, this interpretation is not supported by the data. Among Negroes who reported that their closest white contact was a friend, the proportion *low* in prejudice is slightly *greater* among those who said the friend was "different" than among those who viewed him as a "typical" white. The ambiguity of the friendship concept provides a second possible explanation for the failure of our contact index to significantly differentiate the prejudice groups. It may be that the individual Negro applies different standards of friendship in classifying white and Negro contacts. This would involve a positive evaluation of a white person qualified (implicitly or explicitly) by the phrase "for a white person."[23] This interpretation is directly supported by the fact that Negro respondents did, on occasion, explicitly verbalize this qualification, and it draws indirect support from further analysis based on the use of a second index of equal-status contact.

Respondents in both samples were asked if they had ever done anything of a social

[22] E. Work, "The Prejudice-Interaction Hypothesis from the Point of View of the Negro Minority Group," *American Journal of Sociology,* LXVII (1961), 47–52. For a summary of the studies of majority group persons see D. M. Wilner, R. P. Walkley, and S. W. Cook, *Human Relations in Interracial Housing* (Minneapolis: University of Minnesota Press, 1955), esp. pp. 155–61. The equal-status contact–low-prejudice association represents a special case of the basic proposition of human social behavior formulated by G. C. Homans in *The Human Group* (New York: Harcourt, Brace & Co., 1950), esp. pp. 110–17.

[23] Among whites this double standard is not likely inasmuch as the average white does *not* repeatedly encounter hostile, discriminatory Negroes capable of constituting a reference point of such prominence as to make any non-hostile Negro appear to be a "friend."

nature (e.g., going to a sports event or visiting in each other's homes) with whites (or Negroes) contacted in the neighborhood, in organizations, at work, or elsewhere. The number of whites responding affirmatively was insufficient to warrant further analysis, but over one-fourth of the Negroes responded affirmatively for at least one of the four areas. When we hold this "social" contact constant, we find that "friendship" contact has absolutely no effect upon anti-white prejudice among Negroes—that is, the previously clear-cut *direction* of the relationship is eliminated. This reinforces the belief that Negroes apply different standards of friendship to in-group and out-group members. More important, however, is the finding that Negroes who have experienced "social" contact with whites are significantly less likely to be prejudiced than are Negroes who have not experienced this type of contact. The racial difference in the relation of contact to prejudice is, then, more semantic (i.e., related to the meaning of friendship) than real.

TABLE 8

PREJUDICE BY RACE AND CONTACT

PREJUDICE	NEGRO (PER CENT)		WHITE (PER CENT)	
	Yes	No	Yes	No
	"Friends"*			
High	55	62	61	75
Low	45	38	39	25
N	(316)	(262)	(149)	(962)
	$\chi^2=3.0, df=1, p<.10$		$\chi^2=22.0, df=1, p<.001$	
	Social Contact			
High	41	56	(Number having such contact insufficient for analysis)	
Low	59	44		
N	(219)	(586)		
	$\chi^2=13.5, df=1, p<.001$			

* The Bakersfield samples are not included in this part of the table, since "friendship" data were not collected from the sample of whites in that city.

AUTHORITARIANISM AND RACE

Up to this point we have discussed racial differences and similarities with respect to sociological correlates of prejudice. Now we compare Negroes and whites regarding the relationship of prejudice and authoritarianism, inasmuch as a positive relationship between these variables is well established among whites but the relationship has rarely been studied among Negroes.[24] When we use a series of single-

[24] G. A. Steckler's study of an undisclosed number of psychology students in seven Negro colleges yielded a significant correlation of .55 between scores on the F-scale and on an eighteen-item scale of anti-white prejudice (see "Authoritarian Ideology in Negro College Students," *Journal of Abnormal and Social Psychology*, LIV [1957], 296–399).

item indicators of authoritarianism, our data consistently confirm the expected positive relationship between prejudice and authoritarianism among both whites and Negroes. Although not always significant, the direction of the relationship is invariably positive and the majority of the items do significantly differentiate the prejudiced from the unprejudiced.

Several investigators have noted that Negroes score higher than whites on the *F*-scale.[25] We therefore departed from our focus on prejudice in order to check this finding and available explanations in light of the Cornell data. Only one item indexing authoritarianism was asked of Negroes *and* whites in all four cities sampled: "Children should obey *every* order their parents give *without question* even if they think the parents are wrong." We selected this item as our primary index of authoritarianism solely to assure comparability. The data presented in Table 9 indicate that Negroes are indeed significantly more likely to give the authoritarian ("agree") response to this item.

Smith and Prothro in their study of Negro and white southern college Freshmen suggested that the greater authoritarianism among Negroes is a product of the biracial social system which exposes Negroes, *especially in the South,* to experiences conductive to a high degree of authoritarianism.[26] This explanation implies that the rate of authoritarianism will be highest among southern Negroes and the race difference greatest in the South. Neither of these expectations is confirmed. The rate of authoritariansm among Negroes does not vary by community, and although the *racial* difference in authoritarianism is highly variable by community, the difference is least pronounced—indeed, non-existent—in the southern community. A second item in both Savannah samples ("Prison is too good for sex criminals. They should be publicly whipped or worse") also fails to differentiate Negroes and whites in that city. Inasmuch as the biracial social system is most fully institutionalized in the South, these findings directly contradict the explanation suggested by Smith and Prothro. Thus we are led to propose an alternative explanation for the apparent racial difference in authoritarianism.

TABLE 9

AUTHORITARIANISM BY COMMUNITY AND RACE

CHILD OBEY*	Community (Per Cent)								Total (Per Cent)	
	Bakersfield		Elmira		Savannah		Steubenville			
	Negro	White	Negro	White	Negro	White	Negro	White	Negro	White
Agree	57	34	62	36	56	54	54	48	57	42
Disagree	43	66	38	64	44	46	46	52	43	58
N	(227)	(319)	(150)	(529)	(288)	(294)	(140)	(288)	(805)	(1,430)
	$\chi^2=27.8$, $df=1$, $p<.001$		$\chi^2=28.9$, $df=1$, $p<.001$		$\chi^2=0\ 07$, $df=1$, $p<\ 80$		$\chi^2=1.4$, $df=1$, $p<.30$		$\chi^2=47.4$, $df=1$, $p<.001$	

* Based on responses to the item, "Children should obey *every* order their parents give *without question* even if they think the parents are wrong."

[25] See, e.g., C. U. Smith and J. W. Prothro, "Ethnic Differences in Authoritarian Personality," *Social Forces,* XXXV (1957), 334–38; W. J. MacKinnon and R. Centers, "Authoritarianism and Urban Stratification," *American Journal of Sociology,* LXI (1956), 610–20; and Steckler, *op. cit.*

[26] Smith and Prothro, *op. cit.,* p. 338.

Although Adorno *et al.* stress the belief that authoritarianism is "in the culture" and therefore should be most common among those segments of the population that conform most to the culture (e.g., middle-class persons),[27] their treatment of social-structural variables such as socioeconomic status has been severely criticized.[28] Christie states that it appears well established that *F*-scale scores vary inversely with indexes of cultural sophistication such as education, and he adds that the lower class has realistic justification for a "jungle" world view.[29] More recently Lipset in re-analyzing survey data from several countries has concluded that a series of social and psychological attributes of the working-class environment acts as intervening links to strongly predispose working-class persons to authoritarianism.[30] Our findings are consistent with these interpretations inasmuch as socioeconomic status is inversely related ($p < .01$) to authoritarianism among both Negroes and whites. Thus it seems likely that the

TABLE 10

AUTHORITARIANISM BY SOCIOECONOMIC STATUS AND RACE*

CHILD OBEY†	HIGH		MEDIUM		LOW	
	Negro	White	Negro	White	Negro	White
	Education‡					
Agree	28%	33%	47%	44%	68%	55%
Disagree	72	67	53	56	32	45
N	(85)	(200)	(225)	(454)	(345)	(247)
	$\chi^2=0.41, df=1, p<.60$		$\chi^2=0.45, df=1, p<.60$		$\chi^2=9.9, df=1, p<.01$	
	Occupational Status§					
Agree	34%	36%	49%	42%	59%	56%
Disagree	66	64	51	58	41	44
N	(38)	(109)	(63)	(273)	(261)	(187)
	$\chi^2=0.02, df=1, p<.90$		$\chi^2=0.76, df=1, p<.50$		$\chi^2=0.25, df=1, p<.70$	

* The Elmira samples are not included in this table as necessary data on occupation are not available for the sample of Negroes there.

† See Table 9, n. *.

‡ High education includes (some) college, medium includes (some) high school, and low education includes grammar school or less.

§ High occupational status includes professionals, proprietors and managers; middle status includes white-collar and skilled workers; low status includes semiskilled, unskilled, and service workers.

[27] Adorno *et al., op. cit.*, pp. 229, 975, and elsewhere.

[28] See the chapter by H. H. Hyman and P. B. Sheatsley and the chapter by R. Christie in R. Christie and M. Jahoda (eds.), *Studies in the Scope and Method of "The Authoritarian Personality"* (Glencoe: Free Press, 1954), esp. pp. 112–17 and 172–82.

[29] *Ibid.*, pp. 172 and 175.

[30] S. M. Lipset, "Democracy and Working Class Authoritarianism," *American Sociological Review*, XXIV (1959), 482–501.

higher rate of authoritarianism among Negroes is a function of their heavier concentration in the lower class.[31]

The data lend considerable support to this interpretation (Table 10). The *racial* difference is eliminated among the matched-status partials in five cases out of six when education and occupation are used as separate indicators of status. Negroes remain significantly more likely to manifest authoritarianism only among those persons whose education is limited to grammar school. While we can conclude only that authoritarianism *in child-rearing expectations* is more a function of socioeconomic status than of race,[32] our findings encourage the belief that social class accounts for an important part of the racial variance in general authoritarianism.

This interpretation appears to be consistent with Smith and Prothro's finding, inasmuch as their white respondents came primarily from middle-class homes whereas the Negro respondents were generally from the lower-class. However, they did not compare the races with class held constant nor did they indicate whether there were significant within-race class variations in authoritarianism. Other investigators have suggested that authoritarian items have different meanings to middle-class and working-class persons and that we must therefore be very cautious in interpreting the inverse relationship between social class and authoritarianism.[33] Nevertheless, and for whatever reasons, lower-status persons are more likely to endorse authoritarian items than are persons of higher status. Thus differential class distribution constitutes a very plausible explanation for racial differences in "authoritarianism."

[31] An excellent discussion of the economic and educational position of Negroes compared with whites in American society is provided by E. Ginzberg, *The Negro Potential* (New York: Columbia University Press, 1956).

[32] This is consistent with an earlier finding that actual child-rearing *practices* are much more a function of social class than of race (see A. Davis and R. Havighurst, "Social Class and Color Differences in Child-rearing," *American Sociological Review*, XI [1946], 698–710).

[33] See Christie in Christie and Jahoda (eds.), *op. cit.*, p. 175, and F. Riessman and S. M. Miller, "Social Class and Authoritarianism," as abstracted in the 1957 American Sociological Society's *Abstracts of Annual Meeting Papers*.

CONCLUSIONS

Racial differences in the correlates of anti-white and anti-Negro social-distance prejudice are essentially two: (1) occupational status bears a negative linear relation to prejudice among whites, but among Negroes the relation *tends* to be curvilinear with prejudice least common among middle-status Negroes; (2) social participation, as indexed by both church attendance and organization membership, is negatively related to anti-Negro prejudice but is not significantly related to anti-white prejudice. The explanations of these differences are highly dissimilar. The difference concerning occupation is theoretically significant as it is explicable in terms of race-class variations in exposure to out-group competition. Among whites competition with Negroes is inversely related to occupational status owing to the skewed distribution of Negroes in the class structure. Among Negroes, however, professionals and proprietors as well as skilled and unskilled workers are generally exposed to out-group competition while white-collar Negroes are generally insulated from such competition. Thus within each race the greater the probability of out-group economic competition, the greater the probability of out-group prejudice.

The racial difference concerning social participation as a correlate of prejudice is associated with Negro-white differences in education and religion. When education is held constant, the negative relation between organization membership and anti-Negro prejudice is eliminated in two of the three education partials and, therefore, the racial difference disappears. Since education is inversely related to prejudice in

both samples, the original difference primarily reflects a stronger correlation between education and organization membership in the white community than in the Negro community.

The difference involving church participation reflects the greater importance of the church in the Negro community. Lacking opportunities to participate in other institutional sectors of American society, Negroes have turned to the church as an alternative structure which provides opportunity for self-expression, a setting for social activities, and a means of facilitating adjustment to a harsh environment.[34] This suggests not only that Negroes are more likely than whites to attend church (as the Cornell data confirm) but also that among the whites and Negroes *who attend church* proportionately more Negroes will be extrinsically motivated in their attendance. Thus, since prejudice is highly functional for the extrinsically religious, Negro church participants will be prejudiced more often relative to Negro non-participants than will white participants relative to white non-participants. We cannot expect the relation between church participation (attendance) and prejudice to be the same in the two populations unless the proportion of extrinsic religious participants is the same.

[34] Simpson and Yinger, *op. cit.*, pp. 582–86, and N. Babchuk and R. V. Thompson, "The Voluntary Associations of Negroes," *American Sociological Review*, XXVII (1962), 647–55.

Finally, we conclude that the similarities in the correlates of anti-Negro and anti-white prejudice far outweigh the differences. Subsequent studies might contribute more by focusing on those variations in the correlates of prejudice which are dependent upon the dimension rather than the kind (e.g., anti-white, anti-Negro, etc.) of prejudice under consideration. The varying relation of sex to different dimensions of prejudice amply illustrates that dimensional analysis is essential to the construction of a comprehensive explanation of intergroup prejudice.

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ORGANIZATIONAL CONTROL STRUCTURE AND MEMBER CONSENSUS[1]

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ABSTRACT

The relationships between varying patterns of organizational control and member consensus were investigated in thirty-two geographically separate units of a nationally organized delivery company. The hypotheses predicted that high rank-and-file control relative to that of upper echelons and a high amount of control exercised by members at all echelons would be positively associated with member consensus. The results fail to confirm the first hypothesis. The second hypothesis is supported—a high amount of total control is positively associated with both work-group consensus and hierarchical consensus. The results were seen as part of a larger organizational power syndrome.

INTRODUCTION

Recent research in several organizations, including a clerical organization, several union locals, a service organization, and a voluntary association, has indicated that the manner in which control is structured, at least as reported by members, is related to organizational effectiveness.[2] These studies suggest the importance in some organizations of high rank-and-file control relative to leadership control and, more generally, the importance of a high amount of control exercised by members at all echelons in the organization. The interpretations offered of these findings suggest that these patterns of control may be conducive to high organizational effectiveness, in part, through the uniformity with respect to organizational standards and policies which they promote. Likert, for example, has suggested that significant influence exercised by persons at all levels, the leaders as well as the rank and file, provides the basis of the effective co-ordination of organizational activity. Such co-ordination is derived, in part, from the shared goals and agreement on the means to these goals which this pattern of control promotes. Similarly, the exercise of control by lower echelons is likely to bring with it greater acceptance of jointly made decisions as well as an increased sense of responsibility and motivation to further the goals of the organization. Such motivational effects are very likely to be reflected in increased uniformity concerning the decisions and goals of the organization. A relationship between a high amount of control exercised by persons at all echelons ("high total control") and member uniformity (as a criterion of organizational norms) was suggested in a study of local unions.[3]

[1] This article was prepared as part of a program of research concerned with control in social organizations under a grant from the Carnegie Corporation of New York to the Survey Research Center, Institute for Social Research, the University of Michigan. We acknowledge the valuable suggestions of Arnold S. Tannenbaum, Robert L. Kahn, Dorwin Cartwright, and Stanley E. Seashore, and the assistance of Roberta Ann Levenbach and Dora Cafagna.

[2] Nancy C. Morse and Everett Reimer, "The Experimental Change of a Major Organizational Variable," *Journal of Abnormal and Social Psychology*, LII (1956), 120–29; Arnold S. Tannenbaum, "Control Structure and Union Functions," *American Journal of Sociology*, LXI (1956), 536–45, and his "Control and Effectiveness in a Voluntary Organization," *American Journal of Sociology*, LXVII (1961), 33–46; Rensis Likert, "Influence and National Sovereignty," in *Festschrift for Gardner Murphy*, ed. John G. Peatman and Eugene L. Hartley (New York: Harper & Bros., 1960), pp. 214–27. For summaries of this research see Clagett G. Smith and Arnold S. Tannenbaum, "Organizational Control Structure: A Comparative Analysis," *Human Relations*, XVI (1963), 299–316; Arnold S. Tannenbaum, "Control in Organizations: Individual Adjustment and Organizational Performance," *Administrative Science Quarterly*, VII (1962), 236–57; and Rensis Likert, *New Patterns of Management* (New York: McGraw-Hill Book Co., 1961).

[3] Tannenbaum, "Control Structure and Union Functions," *op. cit.*

Furthermore, amount of total control and member uniformity were related to "union power," that is, effectiveness. The hypothesis was offered that a high level of total control is part of an organizational power syndrome including uniformity and effectiveness.

These interpretations seem to suggest one particularly significant process explaining the efficacy of these patterns of control in promoting high organizational performance, namely, the co-ordination and regulation of member behavior with respect to organizational norms. The resulting uniformity derives its significance from the fact that it is basic to the concerted member effort underlying effective organizational performance.[4] It is the purpose of this paper to consider further the relationships of patterns of control to member uniformity and to evaluate their implications for organizational effectiveness.

THEORY AND HYPOTHESES

I. CONTROL STRUCTURE

"Control" refers to any process by which a person (or group or organization of persons) determines or intentionally affects what another person (or group, or organization) will do. In organizations this process may include formal aspects, such as formulating policy and making decisions, exercising authority in implementing decisions, and applying rewards and sanctions for conformity or deviance. It may also include informal mechanisms and techniques, such as non-legitimated pressures, informal discussion, and decision-making.

The "structure of control" designates the relatively enduring pattern of influence within an organization. Most generally, this consists of the pattern of influence of persons or groups upon the organization. This entails, in large part, influence *between* persons or groups of persons within the organization. We shall employ as a measure of aspects of control structure the technique of the "control graph," which has been discussed and illustrated in a number of earlier publications.[5] The horizontal axis of the graph represents the hierarchical levels of an organization from the top to the bottom. The vertical axis represents the amount of control exercised by those at each of these hierarchical levels, that is, how much influence each of these levels has in determining the behaviors in question, such as the actions of the organization or certain behaviors of members. Two aspects of organizational control described by the control curve are (1) the hierarchical distribution of control, represented by the shape or slope of the curve and (2) the total amount of control exercised by all levels in the organization, represented by the average height of the curve.[6] A curve which rises with hierarchical ascent is negatively

[4] The importance for organizational functioning of such variables as member consensus and reciprocal role expectations has been suggested in a number of studies. Basil Georgopoulos, e.g., found aspects of the "normative system of the organization" such as "normative complementarity" and "group consensus" to be significantly related to organizational productivity ("The Normative Structure of Social Systems: A Study of Organizational Effectiveness" [unpublished doctoral dissertation, University of Michigan, 1957]).

[5] Arnold S. Tannenbaum, "The Concept of Organizational Control," *Journal of Social Issues*, XII (1956), 50–68; Arnold S. Tannenbaum and Basil Georgopoulos, "The Distribution of Control in Formal Organizations," *Social Forces*, XXXVI (1957), 44–50; Arnold S. Tannenbaum and Robert L. Kahn, "Organizational Control Structure: A General Descriptive Technique as Applied to Four Local Unions," *Human Relations*, X (1957), 127–40; Tannenbaum, "Control and Effectiveness . . . ," *op. cit.*

[6] It is important to distinguish between these two aspects of control since they are conceptually distinct variables. A high amount of total control could occur under conditions of either relatively high rank-and-file influence or relatively high leader influence. Conversely, relatively high rank-and-file influence or high leader influence could each imply either a high or low amount of total control. Furthermore, these two aspects of control have previously been found to vary independently (see, e.g., Tannenbaum, "Control and Effectiveness . . . ," *op. cit.*).

sloped and might be said to fit the "autocratic" prototype, while one declining with hierarchical ascent is positively sloped and describes the "democratic" model. A low, flat curve, indicating relatively little control by any level would illustrate a "laissez faire" situation, while a high curve, indicating a high level of control by all levels, fits the "polyarchic" model.

In addition to specifying the pattern of influence of the various levels upon the organization in general, the control graph further permits a more specific description of organizational control in terms of patterns of influence existing between members of various levels. This pattern of influence may be specified both in terms of exercising control ("active control") and of being controlled ("passive control"). Thus the amount of control which persons at a given level exercise over those at other levels may be ascertained, as well as the extent to which persons at a given level are controlled by those at other levels. This permits a description of where in the hierarchy a given level directs its control, as well as the determination of the sources from which control over any given level originates.

2. CONTROL AND UNIFORMITY

The relationship between control and member uniformity has been traditionally subsumed under the concept of social norm. This concept can be defined simply as the continuous uniformity in expectations, attitudes, or behavior within a group (or organization) regarding an activity developed and maintained by processes of control.[7] Central to this definition of norms is the premise that they are a function of control. While this constitutes the basic premise of the present formulation, the subject of our inquiry is more specifically the relationships of varying patterns of organizational control to member uniformity. We shall be concerned with uniformity in perceptions and attitudes which will be referred to as "consensus." The focus will be upon consensus within the work group and between members of the work group and those at higher echelons in the organization. Two general hypotheses can be stated:

HYPOTHESIS I: Consensus within the work group and between members and supervisors will be related directly to the degree to which the control curve is positively sloped.

HYPOTHESIS II: Consensus within the work group and between members and supervisors will be related directly to the amount of total control.

The first hypothesis is a restatement in terms of the control graph that "democratic control" will be conducive to a system of shared norms. The rationale for this hypothesis is based on several processes. Rank-and-file involvement in decision-making, especially in a society that extols democratic values, tends to foster conditions of identification, motivation, and loyalty to the organization. Such effects result, in part, from the satisfaction that individuals may derive from participation in decision-making, that is, exercising significant control contributes to their sense of importance and personal worth. It may also provide important pragmatic or material rewards to the members, and it may be expected to enhance attraction and loyalty of rank-and-file members to the work group. Loyalty to the work group, coupled with involvement and identification with the organization, should give rise to increased uniformity with respect to organizational and work-group standards. They tend also to promote a high level of participation and a greater amount of accurate communication and influence, permitting

[7] This definition is similar in some respects to that offered by Georgopoulos, *op. cit.*, and is consistent with the recent interpretation of social norms presented by Floyd H. Allport ("A Structuronomic Conception of Behavior: Individual and Collective. 1. Structural Theory and the Master Problem of Social Psychology," *Journal of Abnormal and Social Psychology*, LXIV [1962], 3-30).

members to see what the norms of the organization and the work group are, as well as facilitating their determination. Further, relatively high rank-and-file influence in decision-making may permit members to develop policies and practices which represent the interests of a fairly broad segment rather than merely the interests of the leaders, and thus may further enhance acceptance of these decisions by both the rank-and-file members and the leaders.

While high rank-and-file control relative to that of the leadership may have these positive consequences under certain conditions, other authors have pointed up the necessity of control from above to insure efficient organizational functioning. Despite its detrimental effects, "hierarchical control" (negative slope) is viewed as necessary to insure shared organizational norms, effective co-ordination, and concerted member effort. Indeed high rank-and-file control relative to that of the leaders (i.e., positive slope) may result in a lack of consensus and conflict between echelons, if the rank-and-file members act simply in terms of their own self-interests, do not possess the skill to exercise control effectively, or do not accept the contributions of members at higher echelons.[8] The hypothesis as formulated assumes that these circumstances are not present.

The second hypothesis offers an approach to the dilemma stated above by considering the necessity of control by upper echelons *together* with the favorable effects of control by the rank-and-file members.[9] This hypothesis states that a high amount of control exercised by persons at all levels in the organization will contribute to high member consensus within the organization. The hypothesis is based on a set of interrelated processes accompanying a high amount of total control previously elaborated by Tannenbaum and Likert.[10] Part of these processes derive from the high rank-and-file influence *per se* inherent in a high level of total control, and thus the predicted effects in promoting high consensus are similar to those specified in Hypothesis I.

Likert suggests that the efficacy of a high amount of total control in an organization may be explained in terms of the existence of an "effective interaction-influence system," that is, a system in which there is high reciprocal influence and free communicative exchange throughout the organization. Such an interaction-influence system permits members to understand clearly what the norms of the organization are, as well as fostering their *joint* determination and enforcement. Furthermore, this process provides the basis of the effective co-ordination of organizational activity, in part, by facilitating the integration of the interests of both the rank-and-file members and the leaders. As a consequences, there is wider acceptance of policies and practices, and co-operative relations between members at different levels tend to be enhanced. This is likely to be reflected in a set of shared norms, in the form of means and goals, adherence to which tends to be "promotively interdependent" for all the parties involved.[11] Furthermore, utilizing

[8] For a further discussion of this dilemma of "democratic control" see Norman R. F. Maier, *Principles of Human Relations* (New York: John Wiley & Sons, 1952); Philip Selznick, "Foundations of the Theory of Organizations," *American Sociological Review*, XII (1948), 25–35; and Tannenbaum, "Control and Effectiveness . . . ," *op. cit.*

[9] This hypothesis assumes that control is "non-zero sum." The control of one level is not exercised at the expense of the other level; rather both levels may exercise substantial control—the "influence pie" is not fixed.

[10] Tannenbaum, "Control Structure and Union Functions," *op. cit.*, and his "Control and Effectiveness . . . ," *op. cit.*; Likert, "Influence and National Sovereignity," *op. cit.*, and his *New Patterns of Management*.

[11] However, Mayer N. Zald finds that correctional institutions which pursue mixed goals or treatment goals tend to have a power structure that is distributed among all staff members; this is not the case in the more custodial institutions. Furthermore, there is more conflict in the former type of insti-

the contributions of both the members and the leaders provides the basis for better policies and decisions which, in turn, are likely to result in higher organizational effectiveness. Consequently, acceptance of such decisions tends to redound to the advantage of both members and leaders, because of the greater stock of disposable rewards accruing to the more effective organization.

In summary, it is these sets of interrelated processes arising from, or associated with, a positively sloped distribution of control and/or a high amount of total control which provide the bases for expecting relationships between these patterns of control and member consensus.

PROCEDURE

1. RESEARCH SITE

The present study is based on a survey of a nationwide service organization having operations in several metropolitan areas of the United States.[12] The primary function of the organization is to transport and deliver articles from central locations to homes. Each area is organized as a "plant" with two or more major divisions, and each division has several operating units or "stations." A typical station has a station manager, a supervisor, an assistant supervisor, several leaders who work at night, and about twenty-five drivers who work days delivering packages on their respective routes. The stations are geographically separate from one another, each one serving an exclusive territory. They are quite similar in facilities, operating policies, work methods, and procedures, but differ considerably in performance and somewhat in size. Thirty-two such stations, representing five company plants, together including twelve hundred employees, comprise our population.

2. DESCRIPTION OF VARIABLES

The measurements of control structure and consensus are based on questionnaire data obtained from supervisory and non-supervisory employees in each of the thirty-two stations. The average questionnaire return rate for supervisory personnel was 97 per cent and for non-supervisory 87 per cent. No station having a return rate lower than 75 per cent of its non-supervisory members is represented in the sample.

a) *Control measures*.—The following operations were employed to obtain measures of the two independent variables, slope of the control curve and amount of total control. Respondents in each station were asked:

"In general, how much say or influence do you feel each of the following groups *has* on what goes on in your station: (1) Your station manager? (2) The other supervisors in your station? (3) The men in your station?" Response categories were "little or no influence," "some influence," "quite a bit of influence," "a great deal of influence," and "a very great deal of influence."

The amount of control exercised by each level within a station was computed by averaging separately the judgments of members and officers regarding each of the levels. Measures of the slope of the control curve for each station were derived by computing the slope of a best-fit straight line of the control exercised by the three hierarchical levels. This consisted of computing the average of the

tution than in the latter. In effect, his findings suggest that when each group has different basic interests high total control leads to low consensus ("Organizational Control Structures in Five Correctional Institutions," *American Journal of Sociology*, LXVIII [1962], 335–45; "Power and Conflict in Correctional Institutions," VII [1962], 22–49). Our hypothesis assumes the existence of unitary goals and an absence of basic conflict. The tenability of this assumption will be investigated in the subsequent analysis.

[12] For a more extensive description of the research site see Georgopoulos, *op. cit.*; Likert, *New Patterns of Management;* and Bernard P. Indik, Basil Georgopoulos, and Stanley E. Seashore, "Superior-Subordinate Relationships and Performance," *Personnel Psychology*, XIV (1961), 357–74.

algebraic differences between the amounts of influence reported to be exercised by successive levels. The split-half reliability of this index is 67.[13] The amount of total control for each station was simply the sum of the amounts of control reported to be exercised by the three hierarchical levels. The split-half reliability of this index is .84.

In order to illuminate the processes by which slope and amount of total control might have their hypothesized effects, the control *exercised by* the rank and file itself and the extent to which the rank-and-file members *were controlled by* other echelons were measured. The following question was employed to measure the control exercised by the rank-and-file members:

"In general, how much say or influence do the men in your station have on what the following groups do in the company: (1) On what your station manager does? (2) On what the other supervisors in your station do? (3) On what the men in your station themselves do?"

The influence of each level was rated by respondents, again members and officers separately, on a five-point scale ranging from "little or no influence" to "a very great deal of influence." The influence of the men upon station managers and upon the other supervisors in the station was summed to provide an index of the influence of the men upon the upper (supervisory) levels. The split-half reliability of this index is .54. The split-half reliability of the measure of the influence exercised upon the men by the rank-and-file members themselves is .53.

Respondents also rated the influence of the station manager and of the other supervisors upon "what the men do in the company," again on a five-point scale identical to the other control questions. The index of the combined influence of the upper levels upon the men has a split-half reliability of .80.

The various measures of control are subject to two interpretations since they are based on the judgments of organization members. One might interpret them as representing perceptions of control rather than objective descriptions of control structure. Or one might interpret them as providing reasonably valid data on existing control patterns even though it is clear that they are subject to error.[14] Previous research employing the control graph has shown meaningful differences in several types of organizations, as well as significant relationships between indexes of control structure and independent criteria of organizational functioning,[15] suggesting that the data do have some validity as measures of control structure.[16] Moreover, the significant relationships to be reported in the present investigation support this same conclusion.

b) *Consensus measures*.—The organizational activities selected in measuring consensus pertained to work standards, member support of the organization, supervisory behavior, and control. These are activities concerning which norms might be expected to exist since they are relevant to the functioning of the organization

[13] The split-half reliability for this index, as for the others to be reported, is the reliability for group data. Respondents in each station were divided into two random groups, and a correlation was computed between the scores of each group on the index for the thirty-two stations. The Spearman-Brown formula was then applied to the correlation to correct for size of population.

[14] The split-half reliability of our various indexes of control is certainly not as high in every case as would be desired. Particularly, we cannot be sure that the relatively lower reliability of the three measures of rank-and-file control may not operate to limit their correlations with the consensus measures. However, as will become apparent from the results to be reported, there is not a general tendency in this direction.

[15] Smith and Tannenbaum, *op. cit.*

[16] For a fuller discussion of the measurement of organizational structure see the extensive review by Allen H. Barton, *Organizational Measurement* (New York: College Entrance Examination Board, 1961).

and important to its members.[17] The following questions were used to derive measures of consensus:

1. How do you feel about the standards or time schedules set up for your job? [Five alternatives, from "they are very fair for my job," to "they are not at all fair for my job."]
2. How do you feel about the morale in your station? [Five alternatives, from "excellent" to "very poor."]
3. How good is your immediate supervisor in planning, organizing, and scheduling work ahead of time? [Five alternatives, from "excellent" to "very poor."]
4. To what extent do you have confidence and trust in the supervisors in your station? [Five alternatives, from "to a very great extent" to "I don't trust them at all."]

In addition, respondents were asked a fifth question about the amount of control which they felt should ideally be exercised in the station: "In general, how much say or influence do you think each of the following groups *should have* on what goes on in your station?" These groups included the station manager, the other supervisors, and the men, and the influence desired for each was rated on a five-point scale identical to the "actual" influence question previously described. With the exception of the two questions dealing with supervision, both non-supervisory and supervisory employees responded to these questions.

Consensus in the work group was determined by computing the inverse of the variance of responses of nonsupervisory employees within each station for each of the five questions. The three measures of agreement for the fifth question were averaged to form a general measure of work-group consensus regarding the "ideal" pattern of control.

It is important to note that the mean responses of rank-and-file members to all the questions on which the consensus measures are based occur in all stations fairly well within the middle of the scale. (For most of the questions the ranges are from 2.00 to 3.60 on a five-point scale; the most extreme ranges on two questions are from 1.70 to 4.00.) Furthermore no standard deviation on any question for any station exceeds 0.5 of a scale point. Consequently, the possibility is slight that variations in consensus are attributable to a "ceiling effect," that is, that they artificially reflect the fact of extreme scale responses and simply represent average responses to these questions.

Measures of consensus between members at the rank-and-file and the supervisory levels were also derived, using the same set of questions as before with the exception of the two questions pertaining to supervision. Indexes computed for each station by determining the extent of agreement between the *average* responses of rank-and-file members on the one hand and those of the supervisors on the other to each of the three questions resulted in three measures of hierarchical consensus, regarding work standards, "morale," and the "ideal" pattern of control. The last of these measures was again an over-all measure arrived at by averaging the indexes for each of the three echelons. The measures of hierarchical consensus, as thus operationalized, not only permitted the determination of the extent of agreement between the lower and upper echelons in a station but also indirectly provided information concerning the direction or content of the norms of the work group relative to those at higher echelons.

Supervisors tend to be somewhat higher or more positive in their responses than non-supervisory employees. This tendency might operate to create a ceiling effect so that the measures of hierarchical consensus

[17] The necessity of considering behavioral areas which have relevance to the organization and its members is emphasized by Allport, *op. cit.*, who contends that particular norms will develop which have consequences for the operation of a structure (i.e., a group or organization) in which the members have some degree of involvement.

would simply reflect the average responses of the rank-and-file members. In turn, this would tend to inflate the relationships between the independent variables of control structure and the measures of hierarchical consensus since both sets of measures would be based, in effect, upon the averaged judgments of the same group of respondents. However, the measures of hierarchical consensus do not strongly reflect the average responses of the nonsupervisory employees. The correlations of average rank-and-file responses with each of the measures of hierarchical consensus while positive are all insignificant. Nevertheless, in view of this possible ceiling effect and the fact of these positive correlations, we cannot entirely rule out the possibility that the relationships between the independent variables and the measures of hierarchical consensus might be interpreted in terms of such a statistical dependency.

In addition to the measures of actual consensus, measures of the consensus which the respondents perceived to exist in their station were also derived. Measures of perceived consensus were based on the following questions:

To what extent do people in the different jobs in your station see eye-to-eye on things about the everyday operations of your station? [Five alternatives, from "complete agreement" to "no agreement."]

On the whole, would you say that in your station there is any tension or conflict between the following pairs of groups? [The pairs of groups included "employees *and* supervisors," "employees *and* higher management," "supervisors *and* higher management," and "the union *and* the management." For each pair, alternatives ranged on a five-point scale from "a great deal of tension" to "no tension at all."]

TABLE 1

INTERCORRELATIONS AMONG CONSENSUS MEASURES

	1	2	3	4	5	6	7	8	9
Work-group consensus:									
1. Work standards......									
2. Morale..............	−.11								
3. Adequacy of supervisory planning........	−.20	.53							
4. Trust and confidence in supervisors........	−.21	.34	.41						
5. Influence desired for various levels........	.05	.50	.40	.12					
Hierarchical consensus:									
6. Work standards......	−.33	.10	.17	.29	.02				
7. Morale..............	−.15	.30	.06	.07	.05	.08			
8. Influence desired for various levels........	−.25	.03	.14	−.06	.27	.19	.00		
Perceived consensus:									
9. Everyday operations..	−.21	.71	.37	.55	.37	.20	.37	.05	
10. Absence of conflict...	−.32	.66	.54	.60	.28	.22	.30	.03	.85

For the second question, ratings for the four pairs of groups were summed together to provide a general index of conflict within the station. Ratings for the first question and the net ratings for the second question were each averaged, members and officers separately, to provide two measures of perceived consensus within the station.

Table 1 presents the intercorrelations among the ten consensus measures. As seen from the generally low intercorrelations, consensus as we have measured it is not a unitary phenomenon; rather, it has many specific components. The aver-

age of intercorrelations among the measures of work-group consensus is only .18; among the measures of hierarchical consensus, it is only .06. The measures of work-group consensus correlate on the average with the measures of hierarchical consensus only .18. Consensus in the work group and consensus between members at different echelons appear to be distinct phenomena. In effect, where work-group norms exist they are not consistently in agreement with those of upper echelons. Conversely, hierarchical consensus does not necessarily imply the presence of work-group norms complementing those of higher echelons. The measures of perceived consensus largely reflect work-group consensus concerning morale and supervision and hierarchical consensus concerning morale.

As might be expected, the several measures of consensus are negatively associated with size of the station. In view of this confounding effect, the relations between control and consensus will be examined with size, specifically log size, held constant through partial correlation techniques. Any relationship obtained cannot therefore be attributable to the effects of size.

c) Effectiveness measures.—As noted, the control-consensus relationship will be examined with reference to its implications for organizational effectiveness. Organizational effectiveness is defined as the extent to which an organization, given certain resources and means, achieves its objectives without incapacitating its means and resources and without placing undue strain upon its members.[18] Two measures of organizational effectiveness are employed, the first measuring achievement of organizational objectives and the second measuring member satisfaction. The first, "station productivity," was operationalized in terms of objective productivity measures provided by the company. These are based on the total time required to accomplish standard units of work. Specifically, the measure is expressed in units of time consumed by the worker below or above that specified by the standard. The average productivity of all drivers in a station during the month preceding the field study was taken to represent the organizational productivity of that station. An index of member satisfaction is based on the average of respondents' estimates of the "morale" of their station, using the same question regarding morale as was used in measuring consensus. It is taken as reflecting the absence of strain upon the members and the degree to which the human resources of the organization are being preserved.

RESULTS

The two hypotheses predicting relationships between degree of positive slope and amount of total control on the one hand and member consensus on the other assume that slope and total control are independent of each other. However, a high negative correlation ($r = -.67$) exists between degree of positive slope and amount of total control in the stations under study.[19] A high level of total control obtains in these stations through high managerial control relative to that of the rank and file; conversely high rank-and-file control relative to that of the supervisors is associated with the low amount of control exercised by the various levels in the station. In order to provide independent tests of Hypotheses I and II, partial correlations of degree of positive

[18] This definition follows that of Georgopoulos and Tannenbaum, who have considered the validity of the concept in the same organization as that in which the present investigation is conducted (Basil S. Georgopoulos and Arnold S. Tannenbaum, "A Study of Organizational Effectiveness," *American Sociological Review,* XXII [1957], 534–40).

[19] In a study of the League of Women Voters the correlation between degree of positive slope and amount of total control was insignificant, $r = .14$ (Tannenbaum, "Control and Effectiveness . . . ," *op. cit.*).

slope with work-group and hierarchical consensus were computed holding amount of total control constant; partial correlations of amount of total control with these measures of consensus hold degree of positive slope constant. These partial correlations are presented in Table 2.

TABLE 2

PARTIAL CORRELATIONS OF DEGREE OF POSITIVE SLOPE AND AMOUNT OF TOTAL CONTROL WITH MEMBER CONSENSUS

Member Consensus	Control Structure	
	Degree of Positive Slope*	Amount of Total Control†
Work-group consensus:		
Work standards	−.02	−.17
Morale	−.03	.50§
Adequacy of supervisory planning	−.27	.11
Trust and confidence in supervisor	.36‡	.65§
Influence desired for various levels	−.43	−.13
Hierarchical consensus:		
Work standards	.06	.38‡
Morale	.06	.23
Influence desired for various levels	−.37	−.17
Perceived consensus:		
Everyday operations	.37‡	.66§
Absence of conflict	.13	.53§

* Partial correlations between member consensus and degree of positive slope; log size and amount of total control held constant.

† Partial correlations between member consensus and amount of total control; log size and degree of positive slope held constant.

‡ Significant at .05 level of confidence, one-tailed test.

§ Significant at .01 level of confidence, one-tailed test.

Hypothesis I is not substantiated. Degree of positive slope is not generally associated with either work-group consensus or hierarchical consensus. The results suggest even that degree of positive slope may be associated in some instances with lack of consensus, specifically concerning ideal control. It is apparently under conditions of high influence of the managers relative to that of the rank-and-file members (i.e., negative slope) that norms regarding the control which members feel the various levels should exercise are likely to occur. High rank-and-file influence relative to that of the managers does have the effect of promoting the perception that people at different levels see "eye-to-eye" concerning the operation of the station irrespective of whether in fact they do.

The results suggest that a high amount of total control is the effective pattern conducive to both work-group and hierarchical consensus, furnishing partial support for Hypothesis II. Considered from the point of view of specific behavioral areas, the results by no means give unequivocal support for the hypothesis. Amount of total control is related to work-group consensus concerning morale and trust in the supervisor—areas of particular importance to the workers. It is related to hierarchical consensus concerning work standards—an issue of vital significance for the effectiveness of the station. And in part because of consensus with respect to so basic an issue as the fairness of work standards, high total control is associated with the perception of consensus regarding the everyday operation of the station and with a felt absence of conflict between people at various echelons.

Table 3 provides information illuminating the meaning of degree of positive slope and amount of total control. The influence of the various echelons upon the station and the specific influences existing between the work groups and other levels are correlated with degree of positive slope and amount of total control. Again, because of the significant negative correlation between degree of positive slope and amount of total control, partial correlations between slope and the measures of specific influence are computed *holding* total control constant; correlations involving total control hold slope constant. As expected, degree of positive slope is highly negatively related to the influence of the station manager and highly posi-

tively related to the influence of the men on the station.[20] However, degree of positive slope is related neither to the influence of the work group upon various levels, including the work group itself, nor to the influence upon the work group from upper levels. The fact that positive slope is not associated with a high degree of control *between* members at different levels may explain the absence of the predicted relationships between positive slope and the measures of consensus, that is, high positive slope is not associated with an effective pattern of mutual influence involving rank-and-file members and those at higher echelons which would be expected to facilitate consensus.

In contrast, amount of total control suggests a pattern of high influence within the station. Amount of total control is highly correlated with influence of all echelons upon the operation of the station. High degrees of influence by the men, the supervisors, and the station managers tend to go together in this situation.[21] The correlation between the influence of the station manager and that of the supervisors is .65; the influence of the station manager and that of the men correlate .83; however, the correlation between the influence of the supervisors and that of the men is .05.[22] Of particular significance for the achievement of consensus, amount of total control is associated with a system of high mutual influence between members at different echelons, and specifically with the extent to which the workers exercise control upon the work group itself ($r = .72$). In effect, high total control means high self-determination by the workers (but not at the expense of the supervisors).

TABLE 3

CORRELATIONS OF SPECIFIC INFLUENCES WITH DEGREE OF POSITIVE SLOPE AND AMOUNT OF TOTAL CONTROL

SPECIFIC INFLUENCES	CONTROL STRUCTURE	
	Degree of Positive Slope* r_p	Amount of Total Control† r_p
Influence of station manager on station	−.85‡	.98‡
Influence of supervisor on station	.02	.67
Influence of men on station	.72‡	.94‡
Influence on men from upper levels	−.06	.55
Influence of men on upper levels	.21	.39§
Influence of men on men	.21	.72‡

* Partial correlations between specific influences and degree of positive slope; amount of total control held constant.

† Partial correlations between specific influences and amount of total control; degree of positive slope held constant.

‡ Significant at .01 level of confidence, two-tailed test.

§ Significant at .05 level of confidence, two-tailed test.

In an attempt to further interpret the manner in which the pattern of organizational control is related to member consensus in these stations, the various measures of specific interlevel influences involving the work group were correlated with the measures of work-group and hierarchical consensus. The results are presented in Table 4. Consensus derives in part from the influence of the supervisors upon the workers It is apparently in response to such "hierarchical control" that work-group norms develop concerning morale, supervision, and the control de-

[20] This finding does not strictly derive from the operations defining slope. High control of one level *relative* to another does not necessarily imply high control of that level in an absolute sense; in fact *both* levels could be exercising a low amount of control.

[21] This would not be expected to occur invariably in all organizations. For example, in the League of Women Voters, the intercorrelations among the amounts of influence of the various levels upon the League are insignificant (see Tannenbaum, "Control and Effectiveness . . . ," *op. cit.*).

[22] The absence of a positive relationship between the influence of the "supervisors" and the men is understandable in terms of the formal role of the supervisors in these stations. The supervisors are assistants to the station manager and, in terms of formal authority, have a relatively ambiguous role vis-à-vis the rank-and-file members.

sired. Such hierarchical control also has the effect of promoting perceived consensus throughout the station, even though in fact it may not exist. However, the direct influence upon the men by the supervisors is not sufficient in itself to guarantee consensus between the workers and the supervisors. In contrast, and consistent with the findings obtained with regard to positive slope, the control of the men upon the upper level is not associated with either work-group or hierarchical consensus. Such influence may have positive motivational effects for the workers, resulting in a feeling that consensus does exist in the station, but it is not sufficient to promote actual consensus. Rather, the results indicate that it is the extent to which the rank-and-file members exercise control upon the station and particularly upon the work group itself which facilitates the development of a high degree of consensus in these stations. When high mutual influence among the workers is coupled with some say by the workers concerning the operation of the station, it constitutes a partial basis for at least a minimum of agreement between the workers and the supervisors, namely, with respect to the critical issue of work standards.

Table 5 assesses the implication of the control-consensus relationship for organizational effectiveness. The results summarize, in part, those previously reported by Likert, Georgopoulos, and Smith and Tannenbaum.[28] As seen from the findings, the control-consensus relationship that emerges is part of a larger pattern characteristic of the effective station. Not only are amount of total control and general consensus in the station related, but each factor also has significant implications for

TABLE 4

CORRELATIONS OF SPECIFIC INFLUENCES WITH MEMBER CONSENSUS*

Member Consensus	Specific Influences			
	Influence on Men from Upper Levels	Influence of Men on Upper Levels	Influence of Men on Men	Influence of Men on Station
Work-group consensus:				
Work standards	.16	−.15	−.12	−.26
Morale	.46†	.15	.62†	.59†
Adequacy of supervisory planning	.18	.07	.33‡	.22
Trust and confidence in supervisor	.40†	.43†	.57‡	.61†
Ideal influence desired for various levels	.33‡	.05	.33‡	.07
Hierarchical consensus:				
Work standards	.11	.09	.29‡	.32‡
Morale	.22	−.04	.14	.24
Ideal influence desired for various levels	.21	.05	.23	−.02
Perceived consensus:				
Everyday operations	.41†	.41†	.54†	.61†
Absence of conflict	.38‡	.37‡	.45†	.59†

* All correlations of specific influences with member consensus are partial correlations holding log size constant.

† Significant at the .01 level of confidence, two-tailed test.

‡ Significant at the .05 level of confidence, two-tailed test.

[28] Likert, "Influence and National Sovereignity," *op. cit.*; Georgopoulos, *op. cit.*; and Smith and Tannenbaum, *op. cit.*

the effectiveness of the station. Amount of total control is highly correlated with both member morale ($r = .72$) and station productivity ($r = .43$). General station consensus is also significantly correlated with member morale ($r = .32$) and station productivity ($r = .45$).[24] Together, amount of total control and general station consensus yield a high prediction of station productivity; the multiple correlation is .54. The multiple correlation of total control and general station consensus with member morale is even higher, .72. The high-producing station, in contrast to the low-producing station, is characterized by high total control, high member consensus, and high member morale. In contrast, while positive slope may have significant implications for the morale of the members ($r = .55$), the findings have shown that it is unrelated to consensus in the station. It may be for this reason that positive slope is not related to the productivity of these stations. It is not conducive to a high level of order and uniformity which is associated with the high-producing station. The fact that high total control is associated with such uniformity may explain to a large degree its significant implications for the productivity of these stations.

TABLE 5

INTERCORRELATIONS AMONG MEASURES OF CONTROL STRUCTURE, CONSENSUS, AND ORGANIZATIONAL EFFECTIVENESS

Variable	1	2	3	4
1. Positive slope*				
2. Total control†	−.67‡			
3. General station consensus§	−.22	.35‖		
4. Member morale	.55#	.72#	.32‖	
5. Station productivity	.14	.43#	.45#	.34**

* All correlations involving positive slope are partial correlations, holding total control constant (with the exception of r_{12}).

† All correlations involving total control are partial correlations, holding positive slope constant (with the exception of r_{12}).

‡ Significant at the .01 level, two-tailed test.

§ The measure of general station consensus is based on an index derived by pooling the five measures of work-group consensus and the three measures of hierarchical consensus. All correlations involving general station consensus are partial correlations, holding log size constant.

‖ Significant at the .05 level, one-tailed test.

\# Significant at the .01 level, one-tailed test.

** Significant at the .05 level, two-tailed test.

DISCUSSION

I. THE CONTROL-CONSENSUS RELATIONSHIP

In general, the findings suggest that the pattern of control which tends to be associated with member consensus in this organization is that predicted by Hypothesis II: a high amount of control exercised by members at all echelons, leaders as well as rank-and-file members. High total control tends to be conducive to consensus both within the work group and between the rank-and-file and the supervisory levels. However, considered more specifically, the findings are not uniform in their support of the hypothesis. They suggest that high total control facilitates consensus among rank-and-file members with respect to particularly salient and significant aspects of the work situation such as morale or feelings about the supervisor, areas concerning which the workers

[24] We cannot be entirely sure that the relationships between total control and positive slope on the one hand and member morale on the other are not inflated to some extent. The relationships may be somewhat spurious because they are based on the judgments of the same respondents and may to some degree reflect a "halo effect" or response set.

could also most readily reach an understanding and obtain agreement. Furthermore, high total control tends to promote consensus between rank-and-file members and those at higher echelons with respect to those critical areas, particularly work standards, which are highly relevant to the operation of the station and concerning which procedures might be expected to exist for reaching consensus. Amount of total control is unrelated to either work-group or hierarchical consensus regarding ideal control. This may be due simply to the unreliability of the measure or to the fact that ideal control is not a pertinent area for the development of consensus. Or it may be that it is more difficult to reach consensus about such a controversial area despite its relevance to the operation of the station.[25] Moreover, consensus in the essential areas mentioned is reflected in the perception that people in different positions see eye-to-eye regarding the operation of the station and in a felt absence of conflict between members at various echelons. Thus amount of total control, in part because of the actual basic agreement that it establishes, tends also to foster the feeling of co-operative relations in the station. The latter, in turn, may act to reinforce such basic agreement.

The findings further suggest that high total control is efficacious in promoting member consensus in these stations because it is associated with significant influence by the rank-and-file members upon the operation of the station. They have a part in determining organizational norms and consequently may be expected to be more motivated to accept them. This is substantiated, in part, by the significant relationship between total control and the morale of the members and by the significant relationships between the influence of the rank and file upon the operation of the station and the measures of work-group and hierarchical consensus. Equally important, under conditions of high total control, the direction provided by an effective leadership is utilized in determining consensus as well as the control of the rank-and-file members. Such *joint* control has the effect of facilitating an integration of interests and promoting a shared, acceptable system of norms.

Furthermore, a high amount of total control is conducive to member consensus because it is accompanied by an effective system of high mutual influence *within* the work group and *between* members at the rank-and-file and supervisory levels. The system of control is effective because it reflects *both* significant control by the supervisors upon the rank-and-file members and substantial control by the rank-and-file members upon the work group. The data suggest that it is the control of the rank-and-file members themselves within the work group which contributes to an important extent to work-group and hierarchical consensus. It is understandable that such control by workers themselves would particularly facilitate acceptance of the norms of the work group. When complemented by structure which the supervisors provide, there is the insurance that the control by the rank-and-file members within the work group also functions to enhance consensus between the rank-and-file and supervisory levels. In effect, given this system of control, promoting acceptance of the norms of the work group also has the result of reinforcing acceptance of the norms of upper echelons.

Finally, high total control is associated with the acceptance by the rank-and-file members of this pattern of organizational control as legitimate. A significant positive correlation exists between amount of total control and an index of the extent of correspondence between the actual control exercised by the various levels and that desired for these levels based on member responses ($r = .35$, $p < .05$). Such

[25] The relevance of consensus regarding control to organizational performance is demonstrated by the finding, previously reported, of significant positive correlations between member-officer agreement concerning ideal control and productivity in this organization (Smith and Tannenbaum, *op. cit.*).

acceptance facilitates the exercise of effective leadership as well as encouraging the influence of the rank-and-file members in determining and enforcing consensus.

In contrast, while high control by the rank and file *relative* to that of higher echelons may have positive motivational effects for the former and promote the perception of consensus in these stations, it is not in itself a sufficient condition to promote consensus in the work group or to guarantee that the norms of the work group will actually complement those of higher echelons. The absence of the predicted relations between positive slope and member consensus may run counter to the argument of many observers who have discussed the positive effect of "democratic" control or "democratic" decision-making (if by these terms we mean relatively high rank-and-file as compared to leader influence) without considering their full implications in terms of *both* the distribution and amount of control exercised. The insignificant results obtain in these stations primarily because positive slope is not associated with an effective interaction-influence system. High rank-and-file control relative to that of the supervisors is not associated with a high level of control within the work group, which was found to be a highly significant determinant of member consensus, nor is it associated with any appreciable rank-and-file control upon the supervisors or manager. It is furthermore associated with a low amount of control by the supervisors upon the station and the work group. Hence an effective leadership which might provide structure and direction to the relatively high rank-and-file control, and thereby promote and expedite consensus, is absent. This lack of effective leadership under conditions of positive slope is accentuated by the fact that relatively high rank-and-file control may be accompanied by a rejection of the existing pattern of control, that is, by work group norms regarding ideal control counter to those of the supervisory levels. This may have the effect of reducing the significance of the control exercised by either the rank-and-file or supervisory levels.

While the findings appear to support one of the two major hypotheses, a note of caution should be injected. In our conceptualization we have implied certain causal relationships between control structure and member consensus. However, in view of our cross-sectional data, we are unable to specify with certainty the direction of the relationship. It may be a reciprocal one. Not only may the system of control determine member consensus, but the nature and extent of member consensus may condition the type of organizational control.[26] It is readily conceivable that a system of work-group norms counter to those of a higher echelon might increase the tendency toward hierarchical control, that is, negative slope. Or lack of consensus within an organization may be so dysfunctional that a low amount of control is exercised by the various conflicting groups.

2. CONTROL, CONSENSUS, AND ORGANIZATIONAL EFFECTIVENESS

The thesis of this investigation has been that the effects of certain patterns of control on organizational performance derive partially from the uniformity with respect to organizational standards and policies which these patterns of control promote. In turn, the regularity, orderliness, and predictability deriving from such uniformity were viewed as being essential to the concerted action underlying the highly effective organization.

[26] This is suggested by James D. Thompson and Arthur Tuden, "Strategies and Processes of Organizational Decision," in *Comparative Studies in Administration*, ed. Pittsburgh University Administration Science Center (Pittsburgh: University of Pittsburgh Press, 1959), pp. 195–216. They hypothesize that agreement or lack of agreement about different types of issues in the decision process calls for different strategies of decision-making, which in turn dictates different patterns of control.

Our findings indicate that this is indeed a tenable explanation for the effects of total control upon organizational performance which we have found in these stations. The significant exercise of control by both members and leaders leads to a high degree of identification and involvement in the organization. All organization members are more motivated to develop a set of shared policies and practices, to accept jointly made decisions, and to act on behalf of the organization. The system of high mutual influence which this pattern of control signifies provides an opportunity for members and leaders to reconcile their interests and facilitates an atmosphere of co-operation. This further bolsters common loyalties and promotes shared objectives which are reflected in the wider acceptance of organizational norms. The conditions thus exist for effective decision-making and improved co-ordination in carrying out organizational objectives in a concerted manner. Finally, it may be inferred that the joint contributions of members and leaders facilitate better and more acceptable policies and decisions insuring their translation into concerted action of an adaptive nature characteristic of the highly effective organization.

The findings also suggest that "democratic" control (i.e., positive slope) does not have the predicted effect of promoting high organizational performance in these stations partially because it does not promote a system of shared organizational norms which we have found to be associated with the high-performing station. While this pattern of control may lead to high rank-and-file morale, it does not appear to promote basic identification with organizational objectives and practices or motivated action leading to high performance. It appears that in this organization high rank-and-file control relative to the leaders may have the effect of members' acting simply in terms of their own self-interests and not accepting the contributions of the leaders. Furthermore, this pattern of control is not associated with a system of high reciprocal influence in which structure is provided by the leaders. Such structure would help to insure a system of shared organizational norms. In the absence of shared organizational norms and a system of high mutual influence (i.e., high total control) to regulate and co-ordinate member action with respect to these norms, it is not surprising that democratic control is not conducive to high organizational performance.

It is conceivable that a positively sloped distribution of control might lead to a system of shared norms and consequently concerted action on behalf of the organization in a different type of organization with different organizational conditions.[27] This might occur in a "mutual benefit" type of organization, such as some voluntary associations, where the interests and objectives of members and leaders are more fully shared, and where decision-making is of a judgmental nature.[28] Other necessary conditions would include a prevailing ideology sanctioning "democratic" control and a formal structure, including authority and decision-making, that would facilitate the control of the rank-and-file members. If such a structure is also associated with high mutual influence between the members and the leaders, the basis is provided to achieve the necessary co-ordination and to translate rank-and-file control into effective action. In the present organization such conditions do not exist and their absence may account in large part for the lack of the predicted effects of positive slope.

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[27] In the League of Women Voters, a significant positive relationship occurs between positive slope and organizational effectiveness (Tannenbaum, "Control and Effectiveness . . . ," *op. cit.*).

[28] See Thompson and Tuden, *op. cit.*

A FUNCTIONALIST REAPPRAISAL OF PARETO'S SOCIOLOGY[1]

JOSEPH LOPREATO

ABSTRACT

An examination of Pareto's sociology from the viewpoint of functional analysis reveals a highly developed functionalism which bears a remarkable resemblance to present-day versions of it, and particularly to the Mertonian one. It appears, however, that Pareto was particularly successful in avoiding some of the pitfalls common in functional analysis. The credit is probably attributable to Pareto's use of a mechanistic rather than an organismic model, and to his focus on society as a social system at a specific state of equilibrium. The argument presented in this paper would suggest that in the enduring controversy about functional analysis a re-examination of Pareto's sociology from this perspective is likely to prove very useful.

It has been suggested that the past of sociology is still too much with us.[2] In some respects this is both true and unfortunate. In others it is quite untrue and equally unfortunate. In the mid-1930's Pareto's sociology[3] burst like thunder on the American sociological scene. Yet today, when many other scholars of his own and earlier vintage exercise a commanding influence on American sociologists, Pareto's own contributions are largely unknown or minimized.

Only two aspects of his work are rather generally recognized as serviceable articles of the sociological equipage. The first is his conception of society as a system tending to maintain an equilibrium. The second involves his analysis of the circulation of the elites. An additional aspect is sometimes mentioned, but almost accidentally despite the fact that in Pareto's sociology it is closely linked to the other two. And here we begin to encounter interpretations which are frankly puzzling. I am referring to the suggestion, occasionally seen in the literature, that there is in Pareto's sociology an embryo of "functionalism."

PARETO'S FUNCTIONALISM IN THE LITERATURE

For instance, Buckley finds "implicit functionalism in Pareto's thought" and proposes that "We may recognize a further external stimulus to functionalism, though less influential in this respect, in the work of the Italian Vilfredo Pareto."[4]

Martindale, too, discerns a trace of functionalism in Pareto, and many other things, also. In addition to labeling him a "positivistic" and "voluntaristic" "organicist," one who "approves" the use of "the most brutal force to crush innovation" and who theorizes that "disorder" is due to "a little group of agitators,"[5] Martindale informs us that Pareto was "a transitional figure between organicism and sociological functionalism."[6]

Parsons, who in addition to being an illustrious functionalist is also one of Pareto's major American interpreters, sees no

[1] I am deeply indebted to Kurt H. Wolff for truly thoughtful and constructive criticisms of an earlier draft of this paper.

[2] Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1957), p. 3, quotation from Alfred North Whitehead.

[3] The work referred to in this article is Vilfredo Pareto's *The Mind and Society* (4 vols.; New York: Harcourt, Brace & Co., 1935). For brevity's sake, the work will sometimes be referred to as the *Treatise*.

[4] Walter Buckley, "Structural-Functional Analysis in Modern Sociology," in H. Becker and A. Boskoff (eds.), *Modern Sociological Theory* (New York: Dryden Press, 1957), p. 238.

[5] Don Martindale, *The Nature and Types of Sociological Theory* (Boston: Houghton Mifflin Co., 1960), p. 531.

[6] *Ibid.*, p. 466.

functional approach at all in Pareto's sociology. Thus it is that his objective in *The Social System* is to "carry out Pareto's intention" *to delineate the social system*, following the "structural-functional" approach, "which is quite different from that of Pareto."[7]

Merton, too, is rightly regarded as one of the foremost functionalists. Yet, despite his intention to "interweave our account with a systematic review of some of the chief conceptions of functional theory," he makes no reference at all to Pareto in his famous essay on "manifest and latent functions."[8]

A welcome, but perhaps unnoticed, dissent is raised by Kingsley Davis who, in discussing the distinction between the manifest and the latent function, declares that "It was developed with extreme thoroughness in Pareto's discussion, in 1916, of individual utility and utility to, of, and for the community."[9]

PARETO'S FUNCTIONALISM

It is my principal purpose in this paper to demonstrate that a functional analysis in Pareto's sociology is one of its major focal points. This task involves an examination of the critically relevant concepts and a firm awareness of possible discrepancies between what Pierce has called the "explicit meaning" and the "implicit meaning" of those concepts.[10] That is to say that in discussing the array of concepts that spell out a functional analysis in Pareto's sociology, it is necessary to distinguish technical terms from common words with an incidentally congruous use.

Even before this, however, it would be helpful to specify in what sense of the word Pareto's approach is functional, for *functionalism is not a unitary* and clearly articulated perspective in social inquiry; as has been repeatedly pointed out, there are various schools of thought that might lay claim to the title "functionalist." For obvious reasons it is not possible to present a comparison between Pareto and leading exponents of these various schools of thought. Rather, let it be stated only that in my opinion Pareto's functionalism is most similar to the widely known form of functionalism that is best represented by Robert K. Merton. For this reason, an occasional comparative reference will be made to this scholar in order to facilitate an understanding of the argument on Pareto.

So far as I have been able to discover, the basic functionalist differences between these two sociologists are three.

1. Merton has shown a preference for the analysis of concrete social structures such as ceremonials, political machines, bureaucracy, patterns of values, and science. On his part, Pareto was concerned mainly with the formulation of a general functional theory of social structure. Again, Merton has preferred to give a perfecting glance to a few existing "functional conceptions," codify them, and demonstrate their increased usefulness in the explanation of various concrete social structures. Pareto, instead, emphasized the formal properties of theory construction; consequently, the most striking result was what the logician Northrop has justly called an "abstract theory" of social structure.[11]

2. While the delineation of the system is the basic starting point of Pareto's functional analysis, Merton appears to take the system as given by the generally accepted hypothesis of interdependence. His preference is in the direction of estab-

[7] Talcott Parsons, *The Social System* (Glencoe, Ill.: Free Press, 1951), p. vii.

[8] Merton, *op. cit.*, chap. i.

[9] Kingsley Davis, "The Myth of Functional Analysis as a Special Method in Sociology and Anthropology," *American Sociological Review*, XXIV (December, 1959), 765.

[10] Albert Pierce, "Durkheim and Functionalism," in Kurt H. Wolff (ed.), *Émile Durkheim, 1858–1917* (Columbus: Ohio State University Press, 1960), p. 154.

[11] F. S. Northrop, *The Logic of the Sciences and the Humanities* (New York: Macmillan Co., 1947), p. 272.

lishing the open-system proposition that any item is subject to an analysis of its consequences for larger structures in which it is implicated, provided that it be social, that is, repetitive and standardized.

3. Merton has taken inspiration from the theory of the physiologist Cannon, with all the problems that this involves, for the specification of what Nagel has called "the vital functions" as "defining attributes" of social structures.[12] Pareto, instead, made use of the more restricted theory of equilibrium which, as Northrop rightly observes,[13] Pareto borrowed from the chemist Willard Gibbs, having familiarized himself with this theory in his previous work on equilibrium in heterogeneous physical systems. This difference, as we shall see, had important consequences for the functional analyses of the two scholars.

FUNCTION: UTILITY

Pareto's functionalism is an integral part of his view of society as a system tending toward an X state of equilibrium. (See especially 2067–69.)[14] It is this perspective that inevitably leads him to ask what effects such phenomena as the distribution of a given "element" in the society, its change, and its combination and interdependence with other elements may have for a given state of equilibrium in the system and, eventually, for subunits therein. It is no accident, therefore, that Pareto formally introduces the concept of function in connection with his discussion of the organization of the social system. For him, function, or "utility," as he usually prefers to call it, is one of the major "properties of the social system." He writes:

> A system of material atoms and molecules has certain thermic, electrical, and other properties. So a system made up of social molecules also has certain properties that it is important to consider. One among them has been perceived, be it in a rough and crude fashion, in every period of history—the one to which with little or no exactness the term "utility" or "prosperity," or some other such term, has been applied. We must now dig down into the facts to see whether something definite can be found underlying these vague expressions, and its character determined [2105].

After proposing that, to get a more exact picture, one has to state just what norms he intends to follow in determining the entities that he is trying to define, Pareto proceeds to give a technical definition of utility as follows:

> Once we have fixed upon the norms we elect to follow in *determining a certain state as the limit that an individual or a community is assumed to approach*, and once we have given numerical indices to the different states that more or less approximate the limit state, so that the state closest to it has an index larger than the index of the state farthest removed, we can say that those indices are indices of a state X. Then, as usual, for the purpose of avoiding the inconvenience of using mere letters of the alphabet as terms . . . and for no other reason, we shall apply the term "utility" to the entity X just described [2111]. [Italics added.]

In more concrete terms, the concept of utility has two basic related meanings, suggested by Pareto's recognition that society may be viewed both as an aggregate of human beings and as a social organization.

1. It may refer to an effect of contribution that a given phenomenon makes to the maintenance, or to the achievement, of a given goal or "satisfaction," real or assumed, of individuals or groups. Since the utility is viewed as flowing from a particular goal, it would be proper to speak of utilities as "manifest" or "latent functions," depending on whether or not the utility is in direct correspondence with the precedent goal.

[12] Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace & World, 1961), p. 527.

[13] Northrop, *op. cit.*, p. 266.

[14] For the sake of convenience, and whenever possible, reference to Pareto will be made in the text in this form, where the number in parentheses refers to the section number in *The Mind and Society*.

2. Utility may refer to any significant, intentionless, and unobvious effect that a phenomenon has (or is capable of having under appropriate circumstances) on the maintenance of a stated, approximate condition in the social system in which the phenomenon occurs. As Pierce keenly observes in his enlightening essay on the functionalism of Durkheim, on this second meaning of the term, "The 'function' of a social phenomenon proves, in the last analysis, to be its causal contribution to the existing state of affairs."[15]

INTENTION AND FUNCTION: PURPOSE AND UTILITY

It may be noted that the difference between the two meanings above may be viewed also in terms of the presence or absence of any purpose involved in the phenomenon which yields the function. As is shown also by several paradigms and numerous discussions present in the *Treatise,* this fact bears witness to Pareto's acute sensitivity to the distinction between purpose and utility. Indeed in his discussion of "theories," that is, propositions or verbal statements, the first distinction he makes early in the *Treatise* is that between purpose and utility. In Pareto's own words:

> Given the proposition A = B, we must answer the following questions:
>
> 1. *Objective aspect.* Is the proposition in accord with experience, or is it not?
> 2. *Subjective aspect.* Why do certain individuals assert that A = B? And why do other individuals believe that A = B?
> 3. *Aspect of utility.* What advantage (or disadvantage) do the sentiments reflected by the proposition A = B have for the person who states it, and for the person who accepts it? What advantage (or disadvantage) does the theory itself have for the person who puts it forward, and for the person who accepts it? [14].

The question of objective aspect is linked to the distinction between "logical" and "non-logical" action. It is therefore a question regarding the relative efficiency of action, a conception that eventually suggests to Pareto notions of minima, maxima, and optima. Put otherwise, it would help quantify the concept of utility and would sharpen such concepts used by him as "harmful effect" and "beneficial effect" or such concepts as "eufunction" and "dysfunction" still in use among more recent functionalists.

The subjective aspect refers to what some contemporary sociologists call "subjective dispositions." Pareto himself uses various terms, for example, "ideal," "motive," and "purpose," and discusses a variety of types of purposes, of which the following two are the major ones:

1. There is, first, the "real purpose." This is a conscious force impelling to clearly definable action that would result in clearly intended consequences.

2. There is, next, what may be called an "instrumental purpose." This differs from the real purpose mainly in that it is usually unreachable but often necessary in order to achieve the real purpose. For instance, in market transactions we often find that in order to receive what is considered a fair price, the seller seeks more for his product than he expects to obtain from the buyer. What Pareto is doing here, of course, is to raise the question, what is the purpose of a purpose? This is in harmony with his concern with "discovering the substance underlying outward forms," a theme basic to all of his search.

Indeed, his discussion of types of purpose is to a considerable extent intended to sensitize the reader to the difficulty of taking human intentions into account in the analysis of social action. All too frequently, overelaborated strata of apparent intentions conceal the real intentions, wherever these exist at all, and render difficult, if not impossible, the task of linking action with intention and, of course, intention with function. (See especially 1867–96.) Pareto, therefore, would find it difficult to accept such a statement as

[15] Pierce, *op. cit.,* p. 159. This meaning corresponds also to Nagel's sixth meaning of "function" (*op. cit.,* p. 525).

the following in which Merton reveals a clearly "voluntaristic" strain: "At some point, functional analysis invariably assumes or explicitly operates with some conception of the motivation of individuals involved in a social system."[16]

But for still another reason Pareto would not agree with the above proposition. As we can surmise from the definitions of utility presented above, Pareto clearly distinguishes between social action and social form. In the analysis of the former, the concern with motivation is often appropriate. In the analysis of the latter, it is usually superfluous or irrelevant, for the social form does not bear passively the operation of the elements that determine it. On the contrary it has a degree of autonomy of its own and, consequently, given the condition of interdependence, a degree of influence on them as well. Thus Pareto states:

> When . . . a society is organized under a certain form that is determined by the other elements, it acts in its turn upon them, and they, in that sense, are to be considered as in a state of interdependence with it [2061].

The social form, therefore, is capable of producing effects, or utilities, irrespective of human intentions.[17]

UNITS SUBSERVED BY FUNCTIONS: UTILITY FOR WHOM?

Having made the distinction between purpose and utility, Pareto now makes a clarification with respect to the heterogeneity of social units to be considered in appraising a given utility. It is well to let Pareto himself speak in some detail:

> The important thing, first of all, is to distinguish cases according as we are thinking of the individual, the family, a community, a nation, the human race. And not only are the utilities of those various entities to be considered; a further distinction has to be drawn between their direct utilities and the utilities that they derive indirectly through their mutual relationships. So, disregarding other distinctions that it might be of advantage to make, and keeping to such as are absolutely indispensable, we find ourselves obliged to deal with the following varieties:
>
> a. *Utility to the Individual:*
>   a-1. Direct
>   a-2. Indirect, resulting from the fact that the individual is part of a community
>   a-3. Utility to an individual, as related to the utilities to others
>
> b. *Utility to a Given Community:*
>   b-1. Direct utility to communities, considered apart from other communities
>   b-2. Indirect utility, arising by reaction from other communities
>   b-3. Utility to one community as related to the utilities to other communities [2115]

Far from coinciding, these various utilities oftentimes stand in overt opposition (2115). In the functional analysis of a certain phenomenon it is, therefore, necessary to specify the units for which that phenomenon has a given function. For society is "a heterogeneous affair and that fact cannot be ignored" (1882).

But Pareto goes beyond the specification of a range of units for which a given phenomenon can have determinate functions. He takes seriously the concept of interdependence, generally deemed basic to functional analysis, and distinguishes between "direct" utility and "indirect" utility. The former arises, with respect to an individual, for instance, from the context of his own behavior. The latter in turn arises from the fact that, as he is a member of a group, the actions of others may have repercussions for his utilities.

With respect to the analysis of utilities to the community, Pareto argues for a further refinement. He distinguishes between the utility *of* a community and the utility *for* a community. The former refers to the utility to the community viewed as an organization. The latter in turn refers to the utility to the community viewed as

[16] Merton, *op. cit.*, p. 50.

[17] The point made here may be judged also with reference to Durkheim's analogous discussion of "social facts" (see Émile Durkheim, *Les Règles de la méthode sociologique* [Paris: Félix Alcan, 1895]).

a collectivity of individuals. Take, for example, the matter of population increase. As regards prestige and military power, allegedly we approach a maximum utility *of* the community as we increase population size to a point beyond which "the nation would be impoverished and its stock decay." But a smaller population would mean a more commodious level of living, and to that extent we would have a high utility *for* the collectivity, that is, the various individual members of it (2131–35).

Finally, each of the types of utility mentioned in the above paradigm must also be considered with reference to time. What is very beneficial today may well be quite detrimental tomorrow, and vice versa. Thus, for instance, a policy maker must be forever on guard that by eliminating a detrimental social form today he is not in effect destroying a greater future utility. As Pareto says:

> The utility of today is frequently in conflict with the utility of days to come, and the conflict gives rise to phenomena that are well known under the names of providence and improvidence in individuals, families, and nations [2119].

MANIFEST AND LATENT FUNCTIONS: INTENDED AND FUNCTIONAL EFFECTS

In brief, for Pareto the chain of utilities of social actions and forms are frequently long and very tortuous. Of all considerations arising essentially from this circumstance, one deserves particular attention. It refers to what has in recent years come to be known as the "latent function." As Kingsley Davis has justly suggested, this concept is highly developed in Pareto's sociology. The idea emerges forcefully from three basic contexts.

1. As is well known, one of Pareto's major tasks was to demonstrate the preponderance of non-logical reasoning in society. This task he accomplished by way of his means-end scheme. A close examination of this scheme reveals that the utilization of inappropriate means to given goals is one possible source of the latent function. If the means employed for the achievement of certain goals are inappropriate, it follows that whatever effects these means may have, they are incidental or unintended.

2. Having demonstrated the preponderance of non-logical reasoning in society, Pareto was naturally led to ask why it was so widespread and accepted. The query in turn led him to distinguish between "the experimental truth" of an argument and its "social utility," adding, "Correlation of the social utility of a theory with its experimental truth is, in fact, one of those *a priori* principles which we reject" (72). Indeed, in considering various types of theories cross-classified in terms of (1) accord with experience, (2) accord with sentiments, (3) acceptance-rejection, and (4) utility, he explained the widespread acceptance of non-logical reasonings in terms of the utility that they have for various units in the society or for the social system as a whole (14). And this is patently an expression of the latent function.

3. Finally, Pareto's focus on the latent function was a natural by-product of the hypothesis of interdependence in the social system.[18] For if the units constituting the system are interdependent, the repercussions of action or movement in one unit combine with movements in other units, modifying these to some extent and thus producing certain effects which in some degree depart from the effects that would have been observed if the individual units had been autonomous.[19]

The following statement, in which Pareto is discussing difficulties in law-making, concisely conveys his notion of latent function as an effect which is incidental to a specific purpose:

[18] A passing reference to this point was made in the preceding treatment of the indirect utility.

[19] For a brilliant discussion of "cycles of interdependence," see Pareto, *op. cit.* (2203–78). For an excellent concrete example, see especially his discussion of "industrial protection" (2208–22).

If . . . in working for a given objective, one is in a position to influence interests and sentiments, to modify them, the modification may have, in addition to the effects desired, other effects that are not in the least intended; so that one still has to consider both *the intended and the incidental effects* and see just what the social utility of their resultant will be [1864]. [Italics added.]

NET BALANCE OF FUNCTIONAL CONSEQUENCES: NET UTILITY

The recognition of various types and degrees of utility eventually suggests to Pareto a very important concept which seems identical to the concept of "net balance of functional consequences,"[20] so skilfully utilized by Merton to put to rest deeply intrenched but erroneous postulates of functional analysis. Pareto's own concept, which hardly requires comment, comes out most clearly in the following statement, from which the reader may also surmise that when the concept is applied to the social system, it represents in fact a state of equilibrium in the system:

Net utility. Taking account of the three types of utility noted in the case of a single individual [see paradigm above], we get as a result the net utility that the individual enjoys. He may, on the one hand, suffer a direct damage and on the other hand, as a member of a community, secure an indirect advantage; and the latter may be so great as to more than offset the direct damage, so that in the end there is a certain gain for a remainder. So for a group. If we could get indices for these various utilities, and take their sum, we would have the total or net utility of the individual or group [2120].[21]

FUNCTIONAL PREREQUISITES AND FUNCTIONAL ALTERNATIVES

I shall now conclude the examination of Pareto's functional analysis with a treatment of two very important concepts which Merton, after an enlightening critical analysis of the "postulate of indispensability," has separated within the context of a basic confusion and referred to as "functional prerequisites" and "functional alternative."

It may be said at once that there is no place for the concept of functional prerequisite in Pareto's sociology. The avoidance of this notion, which proved so problematic in Merton's analysis, was no doubt facilitated by Pareto's commitment to the theory of equilibrium rather than to an organismic model, wherein the temptation to be concerned with "survival," and therefore with functional prerequisites, is naturally very strong. As I have indicated, in his discussion of the social system Pareto chooses to focus on a specific state of equilibrium in the system. He holds that

If we intend to reason at all strictly, our first obligation is to fix upon the state in which we are choosing to consider the social system, *which is constantly changing in form* [2067]. [Italics added.][22]

Now, if the system is examined in a determinate state, and this state is not viewed as an enduring one, it follows that the concept of functional prerequisite is totally irrelevant. The system changes in any case and assumes progressive "states X1, X2, X3. . . ." Therefore, the theoretically important problems for Pareto are two: (1) the analysis of the "mechanisms" which contribute to the maintenance of the system in an existing state *at a given time,* and (2) the analysis of changes in these mechanisms, or others, which alter the system to a different state. It follows that there is no particular need for the concept of "dysfunction" in order to prove that functional analysis is capable of dealing with social change.

[20] Merton, *op. cit.*, esp. pp. 30–32.

[21] Pareto next goes into a discussion of maxima of utility, a complex problem of both economics and sociology which is too long to discuss here (see especially 2121–39).

[22] For a lucid and cogent discussion of the theoretical necessity to examine a social structure at a specific state of it, see Nagel, *op. cit.*, esp. pp. 527–30. The failure to solve this problem is Nagel's major criticism of Merton in an otherwise favorable reference to him (*ibid.*, p. 530).

If the concept of functional prerequisite played no role in Pareto's functional analysis, the same, however, cannot be said about the concept of functional alternative. Pareto clearly recognized that the mechanisms which impinge on a given state of equilibrium are many and largely, though not always fully, equivalent. The notion is best put to use in his treatment of the mechanisms of which the ruling elite can dispose to delay, though in the long run never to avoid entirely, its fall. They are consequently also basic mechanisms of equilibrium maintenance and change in the social system.

1. The most effective such mechanism would be free and unhindered circulation such that the ruling class would be continuously replenished and invigorated with the ablest individuals available in the society. Strictly speaking, however, this is a mechanism of self-maintenance for "the qualified" and a very effective mechanism of equilibrium maintenance in the system in general. But ruling elites tend to resist circulation.

2. More practically, to defend and maintain itself, the governing class can resort to "diplomacy," fraud, and corruption. Specifically, the governing class finds ways to corrupt and assimilate "most of the individuals in the subject class who show those same talents, are adept in those same arts, and might therefore become the leaders of such plebeians as are disposed to use violence" (2179).

3. Finally, the ruling class can turn to the use of force to maintain its position. Force must be understood to include such techniques as death, persecution, imprisonment, exile, ostracism, and the like. The question of the use of force is too complex to be dealt with adequately here. Briefly, Pareto argued that a certain tendency to use force is a characteristic of all organized groups who have a goal to achieve or a given condition to maintain (2174). As far as the use of force by the ruling class is concerned, he made the following specific observation:

> when a governing class divests itself too completely of the sentiments of group-persistence, it easily reaches a point where it is unfit to defend, let alone its own power, what is far worse, the independence of its country [2191].

Such a governing class loses its usefulness to the system and is on its way to being replaced, often with force. In fact, it may be said that none of the above mechanisms, whether singly or in combination, is lastingly effective. Inevitably the ruling class falls. Very often the fall matures through the fact that the absorption of men strong in "combination-instincts" (the corruptible) into the governing class accentuates, in the long run, "the differences in temperament" between the ruling class and the governed class, "the combination-instincts tending to predominate in the ruling class and instincts of group-persistence in the subject class" (2179). "When that difference becomes sufficiently great," the subject class becomes intransigent and "revolution occurs." The equilibrium is radically altered. The cycle of political utilities begins anew.

CONCLUSIONS

The evidence presented in this paper may suffice to demonstrate that Pareto's sociology does not merely contain an embryo of functionalism but is patently couched in terms that have come to be known under the rubric of functional analysis. Moreover, Pareto's functionalism avoids many of the pitfalls common in functional analysis. To the extent that sociologists are still actively engaged in "the controversy of functionalism,"[28] a rereading of Pareto is likely to prove very useful.

UNIVERSITY OF MASSACHUSETTS

[28] See, e.g., the leading papers on "functional analysis" by Harold Fallding, Wilbert E. Moore, and Melvin Tumin in *American Sociological Review*, Vol. XXVIII (February, 1963).

RESEARCH NOTES

SOCIOECONOMIC STATUS AND FAMILIAL VARIABLES IN MAIL QUESTIONNAIRE RESPONSES[1]

The study of bias introduced by non-returns in mail questionnaire research has survived the either-or and all-or-none foci of the early questionnaire-versus-interview polemics in which the finding of non-returnee bias was frequently a sufficient rationale for throwing out the method with the data.[2] In fact, it could be argued that a typology is now needed to differentiate among the varieties of methods, substantive areas, and research designs associated with findings on respondent–non-respondent differences, particularly when such findings are regarded as potential means for maximizing the number of returns[3] and eventually may make it possible to correct for predictable amounts and kinds of bias when reporting mail-questionnaire data.

The present paper is intended to further the efforts to ascertain and correct for predictable directions and amounts of non-response biases. This intent rests on the assumption that such biases will become measurable conditions of questionnaire research when sufficient descriptive data become available from studies in which the substantive area, the sponsor, the subjects' involvement, and the research methods are comparable and specified.

The data reported here have relevance for mail-questionnaire research in general and implications for mail-questionnaire research on the family in particular. In fact, *mail-questionnaire research would appear, from the present study, to elicit a disproportionately high response from subjects whose self-reported backgrounds in "normal" lower-middle-class families*[4] *skew familial data in the direction of what has become the predominant textbook model of the "nice," "happy," stable, middle-class family.*

SUBJECTS AND PROCEDURES

In 1954, 517 subjects (97 per cent of the senior class in a suburban high school in the San Francisco Bay area) completed the *California Psychological Inventory* (CPI) and a twelve-page questionnaire in a classroom setting. In 1959, these same subjects were asked by mail to complete the CPI again and an additional ten-page questionnaire. Both questionnaires elicited primarily familial information. The subjects were not informed in 1954 about the follow-up study. To provide anonymity, a master list of their names, addresses, and identifying numbers was kept by their teachers; the completed 1959 materials were mailed directly to me by the subjects and collated with 1954 materials by means of identifying numbers.

[1] Very helpful critical comments on an earlier draft of this paper were received from David Gold and Albert J. Reiss, Jr.

[2] For a partial listing of references and issues pertaining to this controversy see Thomas Rhys Williams, "A Critique of Some Assumptions of Social Survey Research," *Public Opinion Quarterly*, XXIII (Spring, 1959), 55–62.

[3] See references in Leo G. Reeder, "Mailed Questionnaires in Longitudinal Health Studies: The Problem of Maintaining and Maximizing Response," *Journal of Health and Human Behavior*, I (Summer, 1960), 123–29.

[4] The operational social-class placement of the subjects was: *upper middle*, both parents attended or completed college and father employed in a professional or managerial position; *lower middle*, both parents completed high school and father was employed in a clerical or sales job; *lower*, neither parent had more than eight years' schooling, and father was employed in a semi- or unskilled job.

Of the 517 subjects studied in 1954, 344 (67 per cent) were presumed to have received the follow-up materials in 1959; 8 were deceased; and the packets of 165 were returned marked "addressee unknown" or "no forwarding address." The large number not located was undoubtedly due to the loss of forwarding addresses when many streets were renamed and houses renumbered in the suburban area where most of the subjects had lived while attending high school.

All CPI and questionnaire data reported in this paper are "before" data, obtained in 1954 but presented here by type of response (see end of Table 1) to the follow up study in 1959. There were 110 subject in the co-operative–response group, includ ing a "first wave" of 76 responding withi the first six weeks, a "second wave" o 23 responding after follow-up letters wer sent at the end of six weeks, and a "thir wave" of 11 responding to the question naire after postcards were sent at the en of three months. Also in the third wav were 80 postcard response subjects, wh returned only the postcard and not th other materials. Examination of CPI an questionnaire data revealed no observabl differences among these three waves.

TABLE 1

GROUP MEAN SCORES (RAW) OBTAINED IN 1954 ON EIGHTEEN CPI SCALES FOR 517 SUBJECTS, BY TYPE OF RESPONSE TO FOLLOW-UP STUDY BY MAIL IN 1959

NAME OF CPI SCALE	GROUPS*					TOTAL INITIAL GROUP IN 1954
	Type of Response in 1959			Total Located in 1959	Not Located in 1959	
	Co-operative Response ($N=110$)	Postcard Response ($N=80$)	No Response ($N=154$)	($N=344$)	($N=173$)	($N=517$)
Class I: Measures of poise, ascendancy, and self-assurance:						
Dominance	22.4	24.5	23.7	23.5	23.6	23.5
Capacity for status	17.3	17.1	16.4	16.9	16.7	16 8
Sociability	23.0	22.4	21.7	22.3	22.1	22 2
Social presence	34.2	34.0	33.1	33.7	33.8	33.7
Self-acceptance	19.5	19.8	19.6	19.6	19.5	19 6
Sense of well-being	35.0	34.7	33.0	34.0	33 7	33 9
Class II: Measures of socialization, maturity, and responsibility:						
Responsibility	29.1	28.3	26.3	27.7	27.3	27 5
Socialization	38.5	37.3	34.6	36.5	35.6	36.2
Self-control	28.4	27.9	24.8	26.7	26.0	26.4
Tolerance	21.0	20.3	18.4	20.0	19.1	19.7
Good impression	16.4	16.2	14.7	15 6	15.4	15 5
Communality	25.6	25.4	24.9	25.2	25.1	25.2
Class III: Measures of achievement potential and intellectual efficiency:						
Achievement via conformance	25.3	24.7	22.3	23.8	23.3	23.6
Achievement via independence	17.6	16.5	15.6	16.5	16.4	16 5
Intellectual efficiency	35.9	35.2	33.6	34.7	34.8	34.7
Class IV: Measures of intellectual and interest modes:						
Psychological-mindedness	10.0	9.5	9.2	9.5	9.2	9.4
Flexibility	9.4	10.3	9.9	9.8	10.0	9.9
Femininity	21.2	20.0	18.5	19.7	19.6	19.7

* *Co-operative response*—completed and returned both the CPI and questionnaire; *postcard response*—completed and returned only a postcard (indicating marital status and related data) sent to them three months after they had received but not returned the CPI and questionnaire; *no response*—returned neither the CPI and questionnaire nor the postcard; *not located*—packet of materials returned by postman, addressee not located.

CPI DATA

Precisely the same rank order of group mean scores occurred on fifteen of the eighteen CPI scales—the co-operative-response (C) group scored highest, followed by the postcard (PC) group, and then by the no-response (NR) group (the PC group always scoring closer to the C than to the NR group). With this rank order of highest scores for the C's and lowest for the NR's, the middle-position scores of the PC's became (spuriously?) the most representative of both the initial population of 517 and the located population of 344.

Although the differences on some scales were quite small, the persistent rank order did indicate a *tendency* for the co-operative respondents to have what is generally considered the more "socially desirable" personality profile. (Within the ranges shown in Table 1, high scores are generally indicative of more positive profiles than are low scores.) Also, a greater *tendency* of the co-operative respondents toward a "nice, conformist" personality type can be inferred on at least two bases: (1) The only three scales on which the co-operative group scored lower than both the postcard and the no-response groups were "dominance," "self-acceptance," and "flexibility."[5] (2) The co-operative group had disproportionately higher scores (and was thereby the least representative of both the initial 517 studied in 1954 and the total 344 located in 1959), on such scales as "socialization," "self-control," and "achievement via conformance."

Another source of inferential support may be derived from the work of Mitchell and Pierce-Jones,[6] which indicates that the CPI appears to measure only four factorially distinct dimensions, rather than the eighteen scales listed. The co-operative group in the present study was the highest-scoring group on all six scales that Mitchell and Pierce-Jones group under the factor they call "Adjustment by Social Conformity" and on all four scales they call the factor of "Super-Ego Strength"; and the co-operative group was the lowest-scoring group on two of the five scales included in the factor of "Social Poise" or "Extroversion." Although the CPI differences were not sufficient to be clearly supportive of such inferences, the persistent direction of these differences was noteworthy and was partially clarified by the questionnaire data.

SOCIOECONOMIC DATA

Previously reported findings show that on a group basis respondents tend to have higher socioeconomic status than non-respondents.[7] Such findings would be supported in the present study if we considered only the mean years of education for the subjects' fathers—11.1 and 10.3 mean years for the co-operative and the no-response groups, respectively (Table 2, n. †). However, when we examine the *distribution* of subjects among the three response groups by given educational and occupational levels (items 3 and 4, Table 2), the co-operative group is seen to overrepresent respondents with fathers at the "completed high school" and at the "clerical and sales" levels, while the no-response group overrepresents respondents with fathers of high educational and both high and low occupational levels. The existence of similar disparate socioeconomic groupings among non-respondents in previous studies may have been obscured by a focus on comparisons between, rather than on distributions within, respondent and non-respondent groups. (The disparate socioeconomic groupings within the no-response group

[5] That the PC's scored highest on these particular three scales seems consistent with attributes ascribable to persons co-operative to the extent of completing and returning postcards, but not so co-operative as to invest 2-3 hours completing questionnaires or so unco-operative as to ignore completely the researcher's request.

[6] James V. Mitchell, Jr., and John Pierce-Jones, "A Factor Analysis of Gough's California Psychological Inventory," *Journal of Consulting Psychology*, XXIV (October, 1960), 453-56.

[7] See references in Reeder, *op. cit.*

TABLE 2

Distributions in 1959 of Located Subjects among Response Groups and of Initial Subjects between Located and Non-located Groups on the Basis of Answers in 1954 to Selected Questionnaire Items*

Selected Questionnaire Items	Located Subjects (N = 344)			Initial Subjects (N = 517)		
	Co-operative Response (32 Per Cent)	Postcard Response (23 Per Cent)	No Response (45 Per Cent)	Located in 1959 (N = 344; 67 Per Cent)		Not Located in 1959 (N = 173; 33 Per Cent)
				N	Per Cent	
Socioeconomic:						
1. Parents' birthplace:						
Both in United States	36	22	43	256	69	31
One in United States	29	24	46	41	62	38
Neither in United States	15	30	55	47	60	40
2. Grandparents' birthplace:						
All four in United States	32	24	43	148	72	28
Two or three in United States	48	14	38	65	66	34
Only one in United States	45	18	36	22	40	60
None in United States	19	28	52	109	69	31
3. Father's education:†						
Completed college	23	29	49	35	67	33
Attended college	28	19	53	43	75	25
Completed high school	47	17	36	70	67	33
Attended high school	32	24	44	97	67	33
Eight years or less	26	27	46	99	63	37
4. Father's occupation:						
Professional, technical, and kindred	30	17	52	23	61	39
Managers, officials, and proprietors	30	19	51	37	71	29
Clerical, sales and kindred	52	16	31	67	74	26
Craftsmen, foreman, and kindred	31	29	40	124	73	27
Operatives and kindred	22	27	52	60	56	44
Service workers	15	18	67	33	57	43
5. Residence moves since birth:						
3 or less	43	29	28	174	79	21
4–9	27	17	56	128	59	41
10 or more	5	17	79	42	54	46
6. Church attendance:						
At least weekly	35	23	42	143	68	32
2–3 times a month	34	21	45	100	69	31
3–12 times a year	26	36	38	61	60	40
Twice a year or less	25	10	65	40	66	34
Familial:						
7. Home situation:						
Unbroken	34	25	41	268	70	30
Broken by divorce	28	17	55	47	52	48
Broken by death	24	17	59	29	63	37
8. Parents' marriage:						
Very happy or happy	33	23	44	208	72	28
Average	38	24	38	90	60	40
Unhappy or very unhappy	17	22	61	46	58	42
9. Parents argue:						
Never or rarely	44	17	39	117	70	30
Sometimes	28	30	42	155	65	35
Frequently	33	13	54	46	68	32
Most of the time	4	27	69	26	58	42

* Percentages add horizontally. See Table 1 for description of groups.

† Mean years of father's education were distributed as follows:

Located subjects:
- Co-operative responses 11.1
- Postcard responses 10.5
- No response 10.3

Initial subjects:
- Located in 1959 .... 10.6
- Not located in 1959 .... 9.8

TABLE 2—*Continued*

Selected Questionnaire Items	Located Subjects (N=344)			Initial Subjects (N=517)		
	Co-operative Response (32 Per Cent)	Postcard Response (23 Per Cent)	No Response (45 Per Cent)	Located in 1959 (N=344; 67 Per Cent)		Not Located in 1959 (N=173; 33 Per Cent)
				N	Per Cent	
Familial—*Continued:*						
10. Relations with parents:						
Good or fair with both	33	24	43	236	68	32
Good or fair with one	33	25	42	97	63	37
Good or fair with neither	....	....	100	11	69	31
11. Discipline, rewards, affection‡						
Mother predominant	41	20	39	148	70	30
Both parents about equal	27	29	44	103	70	30
Father predominant	21	19	60	58	66	34
Other	26	26	49	35	49	51
12. Type of discipline:§						
Predominantly verbal	58	14	28	124	89	11
Mixed	23	37	40	113	62	38
Predominantly physical	11	20	69	107	55	45
13. Birth order:						
Youngest child	42	22	36	77	71	29
Oldest child	33	22	45	138	63	37
Only child	30	16	53	43	73	27
In-between child	23	29	48	86	66	34
14. Respondent's health:						
Excellent or good	35	24	42	266	68	32
Average	22	26	52	50	59	41
Fair or poor	25	14	61	28	74	26

‡ Compounded from six separate questionnaire items. "Mother predominant" means that both before and after the age of thirteen the subject received at least 75 per cent of his discipline, rewards, and affection from mother.

§ Compounded from four separate questionnaire items. "Predominantly verbal" means that both before and after the age of thirteen the subject received such discipline from both parents.

may account for the greater standard deviations on mean scores for the non-respondents than for the co-operative respondents on most of the CPI scales.)

The data on parents' birthplace, residence changes, and church attendance (items 1, 2, 5, and 6) are interpreted here as being consistent with the educational and occupational data in showing that the co-operative-response group overrepresented the more homogeneous if not "stable" families. To put it another way, we may note that, since the co-operative group represented 32 per cent of the located subjects, the first column percentages would all be about 32 per cent if the co-operative group had been representative of the located subjects on the items listed. Instead, however, we find that the co-operative–response group was particularly underrepresentative of subjects (*a*) whose parents (15 per cent) and grandparents (19 per cent) were all foreign-born; (*b*) whose father had either completed college (23 per cent) or not attended high school (26 per cent); (*c*) whose fathers were employed as operatives (22 per cent and service workers (15 per cent); and (*d*) who had made ten or more residence moves since birth (5 per cent).

The fourth and fifth columns of Table 2 enable the reader to scan quickly the items on which some bias was introduced by the particular 344 subjects located from among the initial group of 517. Since the located group accounted for 67 per cent of the initial group, the fourth column of percentages should be about 67 per cent on all items listed. It was anticipated, for example, that the located group would underrepresent subjects with ten or more resi-

dence moves, which it did in accounting for only 54 per cent instead of the expected 67 per cent; but it was not anticipated that this item would differentiate more *among* the located than *between* the located and not-located groups.

FAMILIAL DATA

The data on intrafamily relationships provided the most impressive trend evidence of bias introduced by respondent-non-respondent differences. On items 7, 8, 9, 10, and 12, the co-operative group consistently overrepresented subjects reporting what are generally regarded as positive intrafamily relationships and underrepresented subjects reporting negative relationships. The no-response group consistently over- and underrepresented in reverse from the co-operative group on these items. (The data on respondents' health [item 14] may reflect the importance of physical energy when approximately 2½ hours were required to complete the 472-item CPI and the ten-page questionnaire. The direction of these data might well have been reversed if the study had been focused on health or on medical care, thereby increasing the personal involvement of subjects reporting less than excellent or good health.)

The greatest skewness on familial data occurred on item 12, and has a common-sense interpretation: verbal discipline is more likely to be internalized and to serve in the future as either a negative or positive prod to comply with a "verbal" request to complete a mail questionnaire, than is physical discipline which becomes an ineffectual prod when the subject is able to remain anonymous and out of reach. The data on item 12 may be regarded as consistent with those for items 11 and 13, if we assume that discipline via mothers tends to be verbal and via fathers tends to be physical, and if we assume that a greater proportion of youngest than of other birth-order children are predominantly mother-reared. Within this interpretive framework, the data for items 11, 12, and 13 (Table 2) appear to be consistent with the directional trend of the CPI data and with the observations of others[8] concerning the greater strength of conscience developed as a result of verbal discipline and anaclitic identification with the mother.

When items 11 and 12 were considered together, the combination of "verbal discipline" and "mother administered" did not appreciably increase co-operative response, and the combination of "physical discipline" and "father administered" slightly decreased the no response. Thus, as far as response to this mail questionnaire was concerned, type of discipline appeared to be more important than source of discipline in effecting response.

The further combining of items 11 and 12 with socioeconomic groupings produced marked differences in type of response, but not to the extent produced by type of discipline alone. The relationship between discipline and type of response did not appear spurious in view of the association of each with class. On the other hand, holding discipline constant led to some reduction in the size of the association between class and response, indicating that *to some extent* discipline mediates the relation between class and response—class influences discipline which in turn influences response. Thus, the curvilinear association between co-operative response and the socioeconomic status of the subjects' fathers could be interpreted as follows: Lower-middle-class subjects tend to have received childhood discipline that is predominantly verbal and administered by mother, and to be co-operative respondents; upper-middle-class subjects tend to have had varied combinations in type and source of discipline and to be non-

[8] See Robert R. Sears *et al.*, *Patterns of Child Rearing* (Evanston, Ill.: Row, Peterson & Co., 1957), pp. 362–69, 405–506, 412; and Guy E. Swanson, "Determinants of the Individual's Defenses against Inner Conflict: Review and Reformulation," in John C. Glidewell (ed.), *Parental Attitudes and Child Behavior* (Springfield, Ill.: Charles C Thomas, 1961), pp. 5–41.

respondents; and lower-class subjects tend to have been physically disciplined by father (rejecting mother?) and to be non-respondents.

The present findings indicate that mail-questionnaire research runs the risk of a respondent "sample" overrepresenting subjects who in childhood were "verbally disciplined by mother" (with whatever are the personality and social concomitants), and underrepresenting subjects who in childhood were "physically disciplined by father" (with whatever are the personality and social concomitants). The degree to which this occurred in the present study is evident in the following: The proportion of subjects "verbally disciplined by mother" *increased* from 21 per cent of the 517 initial population to 28 per cent of the 344 located population and to 53 per cent of the 110 co-operative respondents. The proportion of subjects "physically disciplined by father" *decreased* from 23 per cent of the 517 initial population to 19 per cent of the 344 located population and to 7 per cent of the 110 co-operative respondents. Thus, the initial 517 subjects were quite evenly divided between those verbally disciplined by mother (21 per cent) and those physically disciplined by father (19 per cent), but the co-operatives respondents had percentages of 53 and 7, respectively, for verbally disciplined by mother and physically disciplined by father.

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ORGANIZATIONAL PROBLEMS IN TWO TYPES OF VOLUNTARY ASSOCIATIONS[1]

The purpose of this paper is to examine the relationship between a simple means-end typology of voluntary associations and the number of organizational problems faced by such groups.

In their recent book, Blau and Scott advanced a typology of formal organizations and indicated that there are some problems which are characteristic of each type. Voluntary associations correspond roughly to one of Blau and Scott's four types—the "mutual benefit associations."[2] Our intention is to pursue this avenue of thought by trying to delineate in more detail how the nature of organizations relates to the problems they face.

A TYPOLOGY OF VOLUNTARY ASSOCIATIONS

Three dimensions provide the basis for a classification of organizations according to their purposes. They are whether (1) the ends served are internal or external to the group, (2) the achievement of ends is immediate or deferred, and (3) the primary beneficiaries are intended to be members or non-members.[3] These three qualities tend to be highly correlated.

The primary ends of some organizations are found mostly in the existence and activity of the organization itself. This appears to be associated with a more immediate gratification of interests, and with the members being the primary beneficiary of the organization and its activity. This type of voluntary association will be called "consummatory."[4]

The primary ends of other organizations lie beyond the group and its activity and are to be reached by means of the organization. Gratification of interests appears to be less immediate, and in the more extreme cases the formalized intent of the organization may designate non-members as the primary beneficiaries. This type of association will be called "instrumental."

All organizations are *both* means to ends and ends in themselves, and thus never fit completely into one or the other of the "ideal types."[5] Nevertheless, it can be useful to distinguish voluntary associations as being *predominantly* instrumental or consummatory.

THE ORGANIZATIONAL PROBLEMS

Ten problems were selected from among those commonly faced by local, voluntary

[1] This research was supported in part from funds provided by the Wisconsin Agricultural Experiment Station and in part by the Research Committee of the Graduate School of the University of Wisconsin from funds voted by the State Legislature.

[2] Peter M. Blau and W. Richard Scott, *Formal Organizations: A Comparative Approach* (San Francisco: Chandler Publishing Co., 1962), pp. 43–45. Not all voluntary associations are strictly of the mutual benefit type. Our control variable designated as "intended beneficiary" attempts to make this distinction.

[3] A related approach is that of C. Wayne Gordon and Nicholas Babchuk, "A Typology of Voluntary Associations," *American Sociological Review*, XXIV (February, 1959), 22–29, esp. 25–28. For an application of this typology see Nicholas Babchuk, Ruth Marsey, and C. Wayne Gordon, "Men and Women in Community Agencies: A Note on Power and Prestige," *American Sociological Review*, XXV (June, 1960), 399–403; see also Philip Marcus, "Expressive and Instrumental Groups: Toward a Theory of Group Structure," *American Journal of Sociology*, LXVI (July, 1960), 54–59; and George A. Lundberg, Clarence C. Schrag, and Otto N. Larsen, *Sociology* (3d ed.; New York: Harper & Row, 1963), p. 306.

[4] Gordon and Babchuk, *op. cit.*, use the term "expressive" to designate this type of voluntary association. We prefer the term "consummatory," as used, e.g., by Lundberg *et al.*, *op. cit.*

[5] See Gordon and Babchuk, *op. cit.*; and Lundberg *et al.*, *op. cit.*

associations in the course of their maintenance and operation. These problems represent much of the variety of difficulties facing such groups. They are:

1. Improving attendance of the members
2. Obtaining more members
3. Obtaining and developing leadership
4. Obtaining more active involvement of the members in organizational affairs
5. Planning and presenting better programs
6. Being more effective in reaching organizational goals
7. Obtaining better understanding of the organization and its objectives by non-members
8. Obtaining better understanding of the organization and its objectives by members
9. Financing organizational obligations and activities
10. Improving relations with other groups

The general relationship between organizational problems and the type of voluntary association, as conceptualized above, was expected to be that the number of problems encountered in effectively serving the ends of the organization depends upon whether that organization is predominantly an end in itself or a means to some other end.

DATA AND METHOD

Data for this analysis were obtained during the spring of 1962 in a study of voluntary organizations in a rural Wisconsin county. All of the organizations which could be identified were listed for possible inclusion in the study if preliminary information indicated that they (1) had officers, (2) held regular meetings for the entire membership at least twice a year, (3) had a membership primarily of adults, and (4) had no subgroups also meeting the foregoing criteria.

Screening questions were used to check the eligibility for inclusion in the study. This procedure resulted in the exclusion of a number of organizations and yielded a universe of 301. A systematic sample of two-thirds of the organizations was drawn, and personal interviews were held with the chief officer of each for the purpose of obtaining data about the group.[6] Data are available for 191 organizations.

One of the major features of this sample is that it represents a cross-section of the various kinds of voluntary associations throughout a rural county that met the criteria for selection. Thus fraternal, social, religious, patriotic, and other kinds of organizations are included.

Types of organizations were distinguished by responses to a question concerning the major purposes or ends of the organization. The question was asked: Are the major purposes, toward which the organization is actually working, primarily for the enjoyment of meeting together and associating with fellow members, or are they for getting some particular kinds of jobs done as an organization? Seventy-four organizations characterized by the former purpose (39 per cent of the total) were designated as "consummatory" and 109 (57 per cent) were called "instrumental." Eight organizations were not categorized by this typology because of "don't know" responses.

Data regarding the problems were obtained by presenting the respondents with a list of the ten problems mentioned in the previous section. The respondents were asked if their organization faced any of the problems on the list.[7]

It should be recognized that the source of the data (i.e., the chief officer of the organization) has a bearing upon *both* the definition of the problems and the classification of the organizations. It is probable that different respondents would vary somewhat in their views as to the conditions that represent problems for the group, and as to its primary purposes. The question

[6] The interviewing was conducted by trained interviewers through the facilities of the Wisconsin Survey Research Laboratory, University of Wisconsin, University Extension Division.

[7] The list also contained an open category to accommodate any additions, but because only four respondents used it, that category of problems is not included in this analysis.

of how to obtain data regarding an abstract entity is a major methodological difficulty in the study of organizations.

RESULTS

Voluntary associations characterized as being more concerned with meeting to get some job done have more problems than do those which meet simply for the enjoyment and sociability of getting together.

TABLE 1

MEAN NUMBER OF PROBLEMS FACED BY INSTRUMENTAL AND CONSUMMATORY ORGANIZATIONS WITH CONTROLS

Control Variable	Type of Organization			
	Instrumental		Consummatory	
	$\bar{X}$	N	$\bar{X}$	N
No control	3.3	109	2.5	74*
Size:				
Small	2.8	50	2.4	44*
Large	3.8	59	2.7	30*
Holding Office:				
Low	3.7	53	2.5	35*
High	3.0	56	2.4	39*
Beneficiary:				
Members	3.0	23	2.4	43*
Non-members or both	3.4	86	2.5	31*
Age:				
Young	3.4	45	2.2	41*
Old	3.6	47	2.4	24*

* Difference is significant at 0.05 level (two-tail test).

There is a relationship, then, between the type and the mean number of problems faced. Instrumental organizations reported an average of 3.3 problems, compared to 2.5 for the consummatory organizations. The difference is statistically significant at the .05 level[8] (see Table 1).

[8] It should be noted that since we are sampling from a finite population, and since the size of the sample is quite large relative to this population, a correction factor for computing the standard error of the mean has been introduced in accordance with the procedure outlined in Helen M. Walker and Joseph Lev, *Statistical Inference* (New York: Henry Holt & Co., 1953), pp. 167–68.

Does this difference obtain regardless of other characteristics of the organization? In an attempt to answer this question, four characteristics were selected as controls: (1) organizational size, (2) the proportion of members holding office, (3) the intended primary beneficiary of the organizational purpose, and (4) the age of the organization. Each control variable was dichotomized and the relationship between organizational type and number of problems was examined within each control category thus created.[9]

The results remain essentially the same as in the uncontrolled analysis (Table 1). Instrumental voluntary associations report more problems, on the average, than do consummatory organizations regardless of the four control variables. The controls show an influence upon the mean number of problems, chiefly in regard to the instrumental organizations, but this influence does not obscure the entirely consistent difference between organizational types in every control category.

CONCLUSIONS

There are substantial differences among voluntary associations in the number of problems they face. An appreciable degree of regularity in these differences can be observed if the wide variety of voluntary associations are classified according to their nature as predominantly means to an end (instrumental), or ends in themselves (consummatory). When this is done, instrumental organizations face more problems.

[9] The dichotomies were constructed as follows: *size* was divided into "small" (29 members or less), and "large" (30 or more); *proportion of members in office* was divided into "low" (15 per cent or less), and "high" (16 per cent or more); *intended primary beneficiency of the organizational purpose* was divided into "members" and "non-members or both members and non-members about equally"; *age of the organization* was divided into "young" (19 years or less), and "old" (20 years or more).

Although it would be preferable to control these factors simultaneously, the sample size dictated a more modest approach. The control variables were considered consecutively.

Three intriguing questions remain. First, what is the theoretical explanation of *why* the types of organizations differ in the number of problems they face? Second, given that a dichotomy is at best a crude device for classification, what relation between kind of organization and problems would derive from an elaboration and refinement of the classification? And finally, given that these organizational types differ in the number of problems they face, do they likewise differ in the nature of the problems?

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LETTERS TO THE EDITOR

KATZ AND PIRET, "CIRCUITOUS PARTICIPATION IN POLITICS"

January 30, 1964

To the Editor:

Since accepting the surface meaning of verbal expressions of opinion can sometimes lead to drawing naïvely erroneous conclusions, the analysis of Gallup Poll presidential popularity ratings by Katz and Piret ("Circuitous Participation in Politics," the *Journal,* January, 1964) is particularly welcome. Some journalists and politicians have expressed skepticism concerning the meaningfulness of these ratings whenever they move counter to "common-sense" expectations, for example, increased popularity after such failures as the U-2 incident and the Bay of Pigs invasion. As Katz and Piret postulate, such increases are both meaningful and politically significant as expressions of a non-institutionalized form of political participation or involvement.

The political significance of these apparently aberrant shifts in presidential popularity also has implications for the interrelationship between presidential leadership and public participation. This becomes evident when the complete trend of popularity ratings is analyzed, as well as public responses to failures. An examination of the complete trend (for Roosevelt, Truman, Eisenhower, and Kennedy) shows that peaks in popularity occur regularly when the incumbent assumes an active role of national leadership, whether this be by force of circumstance or by personal initiative. Increased popularity after failures and upon assumption of office appears to be special cases of this general rule. Thus, if after the initial rallying around the President after some national crisis, he proves incapable of providing a continuing sense of active leadership, an erosion of popularity should be anticipated. Conversely, during periods of relative public inactivity by the President, there is a characteristic softening of popularity. Interestingly, troughs in presidential popularity appear regularly coincident with off-year congressional elections, that is, at a time when either the President is not in the political foreground or his role tends to be narrowly partisan as a result of his efforts to get his party adherents elected.

Much, though by no means all, of Roosevelt's political effectiveness has been credited to his marshaling of public support by such devices as his fireside chats. Kennedy, in contrast, although he maintained a surprisingly high popularity rating throughout his tenure of office, apparently did so more through force of his personality than through an active appeal to the public on issues of national concern. One wonders if, by concentrating his attention upon the formal legislative processes to his relative neglect of the unorganized public, he lessened the chances for enactment of his program.

This is not to imply that any and all presidential action automatically results in increased popularity. Whenever such action goes counter to the perceived interests and values of any groups, as for example the reaction of southern whites to Kennedy's support for the civil rights of Negroes, support within that group will decline. Nevertheless, to the extent that a President assumes a role of *active public leadership* in the pursual of what are perceived as *national* goals, this leads to the generation of widespread public interest and support as measured by the Gallup

polls of popularity. The presidency, it would appear, is crucial in the determination of how much political apathy will exist. Correlatively, the effectiveness of a President as a political leader is as dependent upon his countering such apathy as upon his skill in operating within formal institutional systems, which is hardly surprising.

IRVING CRESPI

The Gallup Organization, Inc.
Princeton, New Jersey

March 6, 1964

To the Editor:

This is to bring to your attention the publication of a new journal, *Trans-Action: Social Science and the Community*. *Trans-Action* attempts to span the communication gap between the social sciences and the general public. It aims to speed the flow of social science findings, ideas, and thought into the larger community, facilitating their use in decision-making and in everyday life.

Journals and periodicals are invited to reprint the material without charge, with the usual credit line. Such reprinting will further the goal of bringing the social sciences to the largest possible lay audience. The magazine will carry many by-lined articles by social scientists as well as staff-written articles based on research findings from primary sources.

Trans-Action earnestly solicits contributions of well-conceived and well-written articles. They are to be sent to *Trans-Action: Social Science and the Community*, Box 43, Washington University, St. Louis, Missouri 63130.

ALVIN W. GOULDNER
Editor

ERRATA

The *Journal* regrets an error in the article by Edna E. Raphael, entitled "Community Structure and Acceptance of Psychiatric Aid," which appeared in its January, 1964, issue (p. 352). The third line from the bottom of paragraph 2 (col. 1) should have read "1.3 per cent . . ." instead of "13 per cent . . ." as reported.

The correct title of Edward Batson's book is *Contemporary Dimensions of Africa*, not *America*, as erroneously reported by the *Journal* in the January, 1964, issue, page 441.

Martin Gold's book, *Status Forces in Delinquent Boys*, was published by the Institute of Social Research, University of Michigan, and not by the University of Michigan Press, as reported in the March, 1964, issue of the *Journal*, page 568.

BOOK REVIEWS

The Scientific Intellectual: The Psychological and Sociological Origins of Modern Science. By LEWIS S. FEUER. New York: Basic Books, 1963. Pp. vii+441. $10.00.

Lewis S. Feuer attempts to disprove Merton's well-known hypothesis that the Protestant ethic had a decisive positive influence on the development of modern science. With this aim in mind, he re-examines part of the data used by Merton (membership of the Royal Society) and surveys a number of additional instances such as the Copernican revolution, Italian science in the sixteenth and seventeenth centuries, Chinese, French Revolutionary, Jewish, American nineteenth-century science, etc. None of these disproves the existence of a relationship between Protestant origin and scientific leadership during the crucial period of scientific take-off, that is, in seventeenth-century Europe and nineteenth-century America. But according to Feuer the relationship is spurious. Not the religious contents of Protestantism, but "the liberal ingredient added to the Protestant ethic was the vital one for the scientific spirit" (p. 381). To the extent that the same liberal ingredient was added to Catholicism, Judaism, Islam, or any other religion—which, according to the author's exposition, really means that members of those religions became secularized and adopted a "hedonistic-libertarian ethic" characterized by anti-authoritarianism, a generally positive attitude to pleasure, and optimism about the ability of science to make mankind happy—they were not different from Protestants, and vice versa.

The abundant, though somewhat anecdotally supplied, biographical material does not, however, bear out this contention. It shows that the Protestant pioneers of science were not among the religious fanatics, and that many of them were—relative to the age they lived in—lukewarm in the observation of religious practices, uninterested in theology, and independent-minded toward dogma. But there were many exceptions, and the author's explanations of the exceptions (i.e., scientists for whom religion obviously mattered a great deal), though interesting, are not always convincing. But most important of all, Anglicans and Roman Catholics who were farther along the way to secularization and the adoption of a hedonistic-libertarian ethic still lagged behind Protestants in the creation of modern science. It appears that secularized members of these other denominations preferred other types of worldly activities than science (or business), while Protestants on their way to a secular life preferred just these. Which means that the Calvinist background may still have been a decisive factor in the emergence of modern science, even if many of its practitioners were relatively secularized people.

This seems to leave the Jews in the same category as the Protestants, since secularized Jews—as Feuer correctly points out—were no less eminent in science than Protestants. Unfortunately, however, Jews appeared on the scientific scene so late that it is very difficult to learn from their case anything about the beginnings of modern science. I tend to think that Weber's distinction of the Jewish role in capitalism is valid also in the case of science. There was an immanent development in Protestantism paving the way to modern science, but there was no comparable development within Judaism: the scientific talent of the Jews was released only as a result of external influence. In any case the question of the difference between the conditions of starting the scientific movement and those of joining it successfully later seems to me essential for the evaluation of the Jewish case, and it is a pity that, in spite of his extensive use of this case, Feuer does not raise the question.

In addition to trying to refute Merton's hypothesis the author proposes a hypothesis of his own, namely, that the emergence of a hedonistic-libertarian ethic was the precondition for the rise of modern science. This ethic emerges under conditions of economic

growth, in places where people belonging to different cultures meet and co-operate peacefully, and where sexual activity is not considered sinful. These are partly sociological and partly psychological explanations, the latter couched in heavy psychoanalytic language. But there is no systematic attempt to check these explanations of the emergence of the hedonistic-libertarian ethic, only an attempt to verify the presence of this ethic among scientists. Since, however, the operational definition of the concept—especially of hedonism—is very broad, it is difficult to find anybody whom it does not fit. Besides, only in part is the concept used as an explanatory one; in part it serves as a criterion for evaluating different types of scientific work. Thus it is stated in the Epilogue that at the present "the 'new philosophy' of the seventeenth century scientists [has been] virtually done to death [and] an ethic of original sin, translated into the languages of decision-making, power-seeking, and electronic computation, has pervaded scientific circles. The scientist is no longer the scientific revolutionist but the laboratory managerialist." Whether one agrees or not, this takes us beyond sociology.

JOSEPH BEN-DAVID

Eliezer Kaplan School of Economics and Social Science
Hebrew University
Jerusalem, Israel

Modern Social Theories. By CHARLES P. LOOMIS and ZONA K. LOOMIS. New York: D. Van Nostrand Co., 1961. Pp. vii+720. $9.25.

In an earlier work, *Social Systems*, Charles P. Loomis developed what he called the "Processually Articulated Structural Model" or "PAS Model." At that time he built upon the ideas of leading social theorists. The current volume turns to an examination of the extent to which concepts in the PAS Model "fit" the conceptual framework of Howard Becker, Kingsley Davis, George C. Homans, Robert K. Merton, Talcott Parsons, Pitirim Sorokin, and Robin H. Williams, Jr. The authors were fortunate in eliciting and receiving the close co-operation of these theorists. The results of lengthy tape-recorded interviews are inserted in the text as a contribution to the viability of the final product.

The scheme used by the Loomises consists of elements, processes, structural-functional categories, and conditions of social action. Nine classes of elements (belief, sentiment, end, norm, status role, rank, power, sanction, facility) are matched by an equal number of elemental processes and combined into pairs of structural-functional categories. For example, "norming" designates "a closely connected bundle of phenomena" comprising norms as elements and evaluations as processes. In addition, there are comprehensive or master-processes which serve to articulate the others. These are communication, boundary maintenance, systemic linkage, social control, socialization, and institutionalization. Finally, concepts of space (territoriality), size, and time are taken as only partly structural within systems and thus viewed as conditions of social action.

The authors' principal purpose is to extract the central themes of each theorist and place them within their own classificatory scheme. In some cases they take pains to indicate that a particular theorist nowhere used the authors' chosen term but, in their lights, speaks to the same idea. Although there are passages where the effort seems strained the conceptual reordering is modestly and dispassionately done. If the unity found by the Loomises attests to consensus it also attests to the taxonomic vagueness of sociological terms. Without firm guides to conceptual distinctions, multiple similarities can be easily found. The need for concepts which place logical and technological constraints upon one another remains as a paramount task of sociological theory.

Despite these comments, the volume manifests the kind of intimacy with source materials that would try the patience of scholars less professionally dedicated. The authors' awareness of types of textual and contextual categories might have been the envy of medieval classicists. Quotations are extensively drawn from both published and oral transcriptions. The bibliographical citations from Becker alone number 270, and the documentation from other writers is equally thorough and extensive. Fortunately, the

overloading of abstract terms, inevitable in such a work, is partially offset by inclusion of common-sense analogies and descriptive vignettes.

Perhaps the most interesting material is found in the last chapter of the book. Here the authors deal with the conceptualization, analysis, and exploration of change as viewed by their group of theorists. Materials drawn from the verbatim reports are fresh and insightful, though more revealing of the thought of Williams, Merton, and Davis than of Parsons and Sorokin. On the whole the authors have done a serviceable job of searching out the main contributions of America's leading social theorists. There are occasions in every discipline when the main contributions of the main theorists in the mainstream can be profitably summarized and elucidated by one of their main representatives. But some readers will not comprehend Wilbert E. Moore's remarks in the editorial introduction where he writes: "I do not mean to issue a call to revolutionary action, but only to scholarly contemplation of the realities of large-scale and rapid change in social arrangements. It is comforting that the theoretical minds explored in this book are indeed turning to that contemplation."

LLEWELLYN GROSS

State University of New York at Buffalo

Philosophy of the Social Sciences: A Reader. Edited by MAURICE NATANSON. New York: Random House, 1963. Pp. xiv+560. $7.50.

Any attempt to make the unpardonably neglected philosophical substrate of the social sciences accessible to students should deserve praise. Unfortunately, Natanson's reader is a late echo of a venerable *Methodenstreit* about a lost farthing that may never have existed. Support for the idea of the volume gradually undergoes that "death by a thousand qualifications" which the editor explicitly sets out to avoid. But it should be clear that it is the organization and over-all intentions of the volume rather than individual chapters which form the subject of this review. The mind boggles at the problem of reviewing a symposium "briefly but fairly"—and limitations of space allow little more than a mere listing of contributors.

The book is organized to set up a dialectical argument between proponents of what are called the "objective" and "subjective" approaches to social ontology and methodology. Respectively, these conform to positivism-operationalism-physicalism and to intuitionism-phenomenology-mentalism—in short, sociology's traditional Rhine Maidens *Begreifen* and *Verstehen*, in slightly new garb perhaps, but still looking for that ontological gold. Thesis and antithesis find no resolution in this volume; but phenomenology is given the last say.

Part I, "Science and Society," brings two famous essays by Lundberg and Simmel into intriguing juxtaposition. Part II, "Theory and Practice," contains chapters by Winch and Jonas, with comments on the latter by Asch, Hulda, and Lowe. A final chapter by Machlup contains the statement: "Many sociology departments have been notorious for their role as refuge for mentally underprivileged undergraduates." This may well be true, but its relevance to the volume will escape all but the most sophisticated readers. Part 3, concerned with methodology, contains chapters by Nagel, Hempel, Schutz, Lavine, Natanson, and Goldstein. The last-mentioned author comes closest to the position taken in this review. Regrettably, he is not allowed to clinch the argument. Two more parts follow, on "Objectivity and Value" and on "Philosophical Perspectives"—Max Weber and Leo Strauss are featured in the former, Ayer and Merleau Ponty in the latter.

Natanson's introductions to the five parts lack crispness and punch. Many of the chapters are gems (though most of them are somewhat antique in cut), but their setting and the paste stones with which they are conjoined detract from their luster. Each part concludes with a useful annotated bibliography.

It is sad to have to be severe about a book that ought to fill a crying need, but the issues it purports to confront belong to a past generation and should be abandoned as gratuitous. Surely few remaining social scientists wander, lost, in a desert of "dust-bowl empiricism." Surely not many contemporaries are content to regard their "*Lebenswelt*" solely from the aspect of the

armchair. Our subject matter, to a large extent, *is* the phenomenology of the actors who populate it—and propositions about this subject matter (as about all others) should be subjected to empirical test, and that is that.

The time has come for philosophers interested in the social sciences to leave the junkshop of the antiquarian and to take a close look at what is going on in the workshops of living social scientists. If they could be persuaded to do this, they would find no dearth of problems to exercise their skills. The problem of analogic versus metaphoric reasoning, and the problem of the consequences for theory of alternative value orientations, to suggest only two, are appropriate places where they might begin.

GEORGE ZOLLSCHAN

Purdue University

The Nature of Scientific Thought. By MARSHALL WALKER. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. vii+184. $2.25 (paper).

The Preface to this book provides an admirably succinct description of its purposes and content. "This book is addressed to those who wish to learn the basic purpose and procedure common to all the sciences. The purpose is *prediction*, and the procedure consists in the construction and use of *conceptual* models. . . . The first six chapters introduce the concept of *model*, and discuss its relationship to science, mathematics, and philosophy. The next three chapters provide examples of the influence of physical, biological, and social factors upon models. Chapter X provides the mathematical vocabulary used in subsequent sections. Chapters XI through XIV illustrate how the concept of model may be used in four different fields. Finally, Chapter XV uses the vocabulary and point of view developed in this book to discuss the current and future problems of science."

The proposed scope of the book is immense, but the author, a practicing physicist, has all in all come remarkably close to covering it in so brief a volume. The intended audience ("This book is written for the general, educated reader, and no previous knowledge of science or mathematics is assumed") will find parts of it stiff going, and much of the pedagogical technical discussion is likely to miss its mark. But most of the treatment is exceptionally lucid and so well organized that the general reader will gain much from a careful reading, perhaps more than from any other volume of similar purpose.

There are a few sections of the book which the reviewer found unsatisfactory. One of these is a passage attempting to show a basic similarity between the scientific method and any learning by experience. The author characterizes the scientific method and learning by experience as survival techniques and illustrates this point by the particularly bad example of a burning fuse in an electrical circuit. The response of the fuse, we are informed, "is the circuit's defense against being destroyed by the overload." Bypassing the teleological issues raised, it seems to me that the basic problem with the formulation is the confusion of memory with reason. This is curious, since the author takes great pains to emphasize the role played by reason in the development of science, distinguishing scientific method very sharply from empirical generalization. The desire to exhibit the unity in science seems momentarily to have unsettled an otherwise sober development.

It may not be caviling to decry the author's mixed bag of examples of models in social science: theories of history by Hegel, Marx, Spengler, and Toynbee; the mathematical theory of games; linear programming and operations research; and what seems to the reviewer a completely gratuitous "model" of beauty which turns out to be a reinterpretation of some principles of conditioning under pleasurable reinforcement. I shall refrain from commenting on the treatment of Freudian thought in the chapter on models in biology. To be sure, the social sciences are not rich in fruitful models; nevertheless a serious study of their current literature would reveal more appropriate illustrations than the list above. Most literature on the philosophy of science concentrates on the experience of physics and chemistry and the lessons to be learned from that experience. From the great similarity of

examples and discussion in treatise after treatise, even a layman may conclude that the selections are made with good judgment, insofar as physics and chemistry are concerned. Much of this consensus derives from the fact that many philosophers of science by virtue of deliberate training are very familiar with these disciplines. The conversely bad judgment usually exhibited in the choice of social science material stems very likely from lack of familiarity with these variegated disciplines. One is grateful, however, for the Hemples, the Nagels, and the Kaplans.

Chapter XIV deals with science and ethics. The point of view is simple: Ethics is an instrument of survival and science can tell us best how to survive best. The spirit and naïvete with which this position is developed are refreshing.

The final chapter contains a diagnosis of the problems of science and the recruitment and training of scientists in the United States. The discussion is somewhat free-wheeling, but in the main seems sensible and agreeable to the reviewer. But the dramatic rearing of Malthus' warning failed to move him.

SANTO F. CAMILLERI

San Diego State College

A New Survey of the Social Sciences. Edited by BAIDYA NATH VARMA. London: Asia Publishing House, 1962. Pp. xi+248.

This is a collection of fourteen new essays by a number of well-known contributors in the various social sciences. The intention of the editor has been to provide the intelligent layman with the major concepts and theories of the disciplines of legal, political, economic, and behavioral analysis.

Nine of the contributions describe a field of study in social science. These, and the contributors, include law (William O. Douglas), political science (Lasswell), international relations (Padover), psychology (Gardner Murphy), social psychology (Klineberg), cultural anthropology (Morris Opler), sociology (Zetterberg), economics (Neisser), and statistics (Paul Neurath). Five essays on methodological and theoretical problems complete the volume: two chapters by the editor and one each by Barton and Lazarsfeld, William McEwen, and Radhakamal Mukerjee.

The authors interpret their mission in various ways. Some present a description of a field, some analyze a concept or portion of a discipline, some attempt theoretical innovations, and some present entirely new approaches. Hence, some of the essays are of interest to the professional social scientist as original contributions to theory. Among these are Zetterberg's attempt to reduce sociological findings to certain basic propositions, McEwen's synoptic model for social science inquiry and Varma's discussion of concepts. Other essays are to be judged primarily as ways of informing laymen about complex bodies of thought and findings.

Like many other such volumes, this one suffers from the uneven length and quality of its contributions. The essays on the political sciences are far too short to be of use. In five pages on law, Justice Douglas cannot rise much above oratory. Other contributors would have been better advised to write less and to descend to oratory. Thus McEwen finds twenty-five pages too few to enable him to climb out of the box of unclear jargon. On the other hand, some of the contributions are magnificent examples of clarity and precision of writing, a necessity for the pedagogical task of describing a field. Gardner Murphy and Paul Neurath are especially effective in their condensations of psychology and statistics. Zetterberg's essay is a clear and notable contribution to theory.

Professor Varma, of CCNY, has produced a useful collection of interest both to laymen and to professional social scientists. It is of value to anyone who is concerned with understanding the developing sciences.

JOSEPH R. GUSFIELD

University of Illinois

Primitive Classification. By ÉMILE DURKHEIM and MARCEL MAUSS. Translated and Edited with an Introduction by RODNEY NEEDHAM. Chicago: University of Chicago Press, 1963. Pp. xlviii+96. $3.00.

I wish that in the present instance the publisher had violated the conventional mode of presentation and had printed the Durkheim-

Mauss essay in front of the translator-editor's Introduction. This might have enticed the reader to examine the former before he delved into the latter. The essay itself will impress the reader with its quiet elegance, its direct, logical form, its clarity of style, its spirit of careful, yet bold, exploration, its suggestion of a new, exciting way to investigate a traditional problem, and its ingenuity in demonstrating, in Durkheim and Mauss's own words, "what light sociology throws on the genesis, and consequently the functioning, of logical operations." The reader is entitled to savor this sociological gem before being exposed to Needham's devastating, essentially ruthless, and to some extent bewildering and intellectually arrogant introduction.

Needham tells us that the essay is worth reading for its historical, methodological, and theoretical interest, but this viewpoint is expressed only after he has concluded that Durkheim and Mauss's argument is logically fallacious, that its methodology is unsound, and that "there are grave reasons, indeed, to deny it any validity whatever" (p. xxix). Moreover, he accuses the French sociologists of serious lapses from "the conventional requirements of scholarly publication" (p. xlvi), including misrendering of sources, citing a non-existent publication, misspelling names of persons, places and things, and other bibliographical errors. With great condescension, Needham assures us that we could not expect Durkheim and Mauss, "prescient though they may appear in some respects," to escape the limitations of the prevailing style of thought of their day which constrained scholars "to analyze human affairs causally and historically" (p. xxxiii). The arrogant haughtiness of Needham's approach is further revealed in such observations as that "very few academics have any ideas of their own" (p. xliv) and that Britain is the country where social anthropology has acquired most renown in recent decades (p. xlii).

Fortunately, the Durkheim-Mauss essay can be judged without reference to these latter pronouncements. Despite the tone and manner of the Introduction, we must all be deeply grateful to Needham for having produced a meticulously edited and, in general, excellently translated edition of this classic. Durkheim and Mauss themselves viewed their work as only a new way of approaching classificatory systems that at least deserved to be tried. Their emphasis on the heuristic value of exploring the sociological origins of the categories of thought is well expressed in the closing sentences of Durkheim's *Elementary Forms of the Religious Life*, for which, incidentally, this essay on primitive classification constituted an initial formulation: "What must be done is to try the hypothesis and submit it as methodically as possible to the control of facts. This is what we have tried to do."

In his brief account of the intellectual history of this essay, Needham might have mentioned the influence of the Durkheimian perspective upon the work of Piaget and his collaborators.

HARRY ALPERT

Center for Advanced Study in the Behavioral Sciences and University of Oregon

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World Revolution and Family Patterns. By W. J. GOODE. New York: Free Press of Glencoe, 1963. Pp. 432. $9.95.

As a field, "family sociology" is as much the butt of jokes as any, but (1) families are characteristic of all known societies; (2) until high levels of modernization were reached the vast majority of all people oriented most of their behavior most of the time to family considerations; (3) they still do so in the least modernized areas and often do so in the most modernized areas; (4) the family is the only known organization in terms of which the vast majority of all members of all societies have some roles at every stage of the life-cycle; (5) behavior in terms of those family roles ideally *and/or* actually affects their behabior in terms of all other roles; (6) despite all *soi-disant* "losses of function," for all societies—including the most modernized—the family remains the organization in terms of which the vast majority of individuals acquire learning on which all other learning is based (e.g., eating, sleeping, control of bodily functions, speech, role differentiation, solidarity, economic and political allocation, etc.).

One could go on; many of our firmest and most general propositions inhere in this field.

It is surprising that "family sociology" has not come to more. In the hands of W. J. Goode and a few others it finds its place in the sun.

Goode wants to put the state of knowledge about the relation between family patterns and modernization on as firm a basis as possible. Even devotees of the silliest form of the logic of misplaced dichotomies cannot *ad hominem* Goode away by calling him a theorist versus an empiricist or vice versa. He carefully distinguishes between "ideal" and "actual" patterns, thereby solving many paradoxes. He states the problem of the relation of changing family patterns to modernization with great clarity (and points out en route the terrible pit of tautology in which those stand who seek to make their definitions descriptively realistic).

After stating his problem and separating allegations of fact from fact in the "West" as best he can with the data, he gives coverage to Islam, sub-Saharan Africa, India, China, and Japan, leaving little good material uncited. (In Japan, for example, he has even been perceptive enough to recognize the special relevance of a scholar such as Kunio Yanagida.) No student could get a better bibliographical start than with his footnotes *and* his observations about the shortcomings of the extant data. (Readers will probably not heed those observations, but that is not Goode's problem.)

He may miss a trick or two now and then. Along with all other scholars he asserts that samurai wives had less authority and scope than rural ones. Some of the very kind of reasoning at which he is so good should have suggested dissent. And at the end he proudly and forthrightly tells us where he stands as a moralist on his findings—a compunction that has passed out of the "natural sciences" as far as technical work is concerned—but what the hell; it's a grand book.

MARION J. LEVY, JR.

Princeton University

Workers, Factories, and Social Change in India. By RICHARD LAMBERT. Princeton, N.J.: Princeton University Press, 1963. Pp. xiii+247. $5.50.

This is a significant and able addition to the comparative study of industrialization and the literature of developing areas. Based on interviews with a sample of 821 factory workers in Poona, Lambert has collected information on the social characteristics of the labor force in five large, privately owned factories. He has analyzed differences between old and new factories and between those with work processes involving different skill levels and supervisory types.

The importance of the study lies in demonstrating the lack of fit of earlier theories of labor commitment which point to great difficulties in securing the co-operation of traditional populations to industrial work. There is little evidence of reluctance or ambivalence in worker commitment in Poona. The factory is neither a catalyst producing modernization nor is its development dependent upon workers who represent a sharp break with the past.

These conclusions emerge from a group of findings which show that the factory work force was not different from the Poona population but that, where differences were found, they were in the opposite direction from that predicted by labor commitment theories. For example, the work force is somewhat more highly educated, earns more money, and contributes to larger families than is true of the average Poona inhabitant. Despite the general underemployment of Indian society, factory workers have a high sense of security, have experienced several factory jobs, are urbanized, and represent an already socialized work force. They are not peasants new to both city and industry. Caste and education show a clear relation to each other and to factory status, especially as between clerical, supervisory, and line jobs. In general, and more so in the newer factories, education (above the primary level) is closely related to high factory status. The relation of wages to status is not as close as is expected, however.

For Lambert, these findings add up to a theory of work-force "overcommitment" which stresses the relatively high wages of factory work and the security which legal and customary norms enforce. Factory work in Poona is characterized by a lack of relation between job security, wages and status, and productive performance. As is true of the traditional caste system, jobs have become infused with property rights; hence management can only

try to govern job entry, not performance. "Workers are still oriented . . . to a village-based set of inter-personal norms which . . . are carried over into the society" (p. 93).

This theory is the rich pay dirt of excellent comparative work. It suggests that traditional culture is by no means inconsistent with industrialization. In its specific relation to Lambert's data, I find this view somewhat *ad hoc*. The writer uses legal and historical material to make sense of his interview findings, rather than the alleged values being demonstrated by the data. But this is necessarily the case in comparative work. Having shown that one way of seeing is not reliable is to have done a great deal. The suggested new way is a significant view around which to continue study. The more we peel the onion of the developing societies the more we realize that the dualities of tradition versus modernization are not sharp enough knives for the task.

JOSEPH R. GUSFIELD

University of Illinois

Process of Independence. By FATMA MANSUR. New York: Humanities Press, 1962. Pp. xvi+192. $5.00.

The author of this book is literally a (young?) Turk, and the book is undoubtedly enhanced, as A. H. Hanson says in his Foreword, by the fact that the author has experienced in her own country events which were similar to the political evolutions of the new nations that she describes. The book is a revised version of a doctoral dissertation completed at Harvard University in 1960, and its subject is comparative politics. It contains an analysis of the political transition from politically dependent colonies to politically independent nation-states, based on available documents and secondary sources for India, Pakistan, Indonesia, and Ghana. Occasional examples are drawn from other new nations as well. The purpose is ambitious and worthy, and in general the author carries it out well with careful scholarship, thoughtful insights, and sympathetic understanding.

The book begins with a general discussion of the concept of elites, but soon moves to a review of the impact of colonialism on the colonies, including an examination of some similarities and differences in the British, French, and Dutch colonial situations. The effect of colonialism on the authority system is stressed, and the role of elites as nation-builders takes the center of the stage.

In chapter ii, the author shows how Western education created an emergent indigenous elite dedicated to a rationalistic frame of mind and a belief in equality, progress, and organization. Both the traditional way of life and the colonial power came to be viewed as obstacles to social change by such equalitarian and liberal Western-educated elites. Thus, the desire for nationhood was created and nationalist movements, linked to the goal of modernization, were born.

After describing in chapter iii the competition and replacement of different elite groups during the transition to political independence, the author turns to the political processes of constitution-making and the development of political parties, to which she devotes a chapter each. A summary chapter concludes the book.

The chief strengths of the book, I think, are to be found in the combination of the comparative approach, the constant drive toward generalizations, and the rare ability to keep the discussion centered on dynamics. Change, development, and transitional stages remain in sustained focus.

Not altogether picayune criticisms include the following: (1) the attempt to elaborate the theory of elites is weak; (2) relevant theoretical and empirical studies by many writers are ignored or insufficiently utilized—Almond, Apter, Deutsch, Pye, Shils, and Weiner to mention a few; (3) the method often verges on selective illustrations—especially when countries other than the main four are brought in—rather than a systematic review of a random sample of all the evidence bearing on a given proposition; and (4) occasionally, the author makes false comparisons between the new states and the Western nations.

One example of the latter is particularly troublesome, although certainly not unique to this author. Mansur is to be commended for not stretching the definition of political democracy to the point where the new states she discusses are all *in some sense* democracies, but she does compare the *realities* of political life in the new states, not with the *realities*

of political life in the Western democracies, but with the *ideal* of democracy in Western political thought. To conclude from such a comparison that Western political institutions do not work in the new states because of some special conditions in the new states is, in my opinion, premature. One might have to conclude from such a stringent test that Western political institutions do not work anywhere, even in the Western democracies, because the democratic ideal is so imperfectly achieved. In the United States alone, it is not difficult to enumerate a few imperfections: gerrymandering, the one-party South, bribery and blackmail of public officials, mattress voting and repeating in Boston slums or Chicago River wards, etc. Yet, giving up the democratic ideal could create conditions far worse than the present imperfect (but improving?) effort to achieve it. The same is true in the new states. No one, including Mansur, has *proved* that democracy will not work there—at least as well as it does in the Western democracies—even though the arguments in favor of such a conclusion are seductively plausible, apparently reasonable, and widely believed. However, the consequences of such a conclusion can further erode democratic institutions in the new states by supporting a common alibi for not *trying* to make political democracy work at all.

But these criticisms do not alter the fact that the book is a genuine contribution to our growing understanding of the political transition to nationhood.

WENDELL BELL

Yale University

The Black Man's Portion: History, Demography and Living Conditions in the Native Locations of East London, Cape Province. By D. H. READER. Cape Town: Oxford University Press, 1961. Pp. xv+180. $5.60.

Initially, this first of three volumes of a series called "Xhosa in Town," which reports the results of a survey of demography, employment, and housing of the African ("Bantu" in the current Union of South Africa terminology) population of a harbor city, appears to be of interest only to social scientists who are concerned with the study of Africa. Published under the general editorship of Philip Mayer (who is also co-author of the second volume, *Townsmen or Tribesmen*), Reader's study is soundly empirical. By making use of well-controlled statistical sample surveys, he is able to demonstrate, among other things, that the 1951 Census of East London underestimated the African population by half; as a clue to the situation in the entire country, this finding is fraught with political significance. But although the statistical materials are supplemented by "anthropological interviewing of the conventional open-ended type" which provides apt illustrations of the human situation behind the statistics, one seeks in vain any sort of sophisticated conceptual analysis. The book ends abruptly without any conclusions.

Given the contemporary context of South African society and politics, however, Reader's work becomes immensely relevant to anyone concerned with the comparative study of social change, as may be suggested by one question which it raised in the mind of this reviewer: What would have happened if the socioeconomic differentiations created by the industrial revolution in Europe had coincided with a profound racial cleavage? Or, to put it another way, what happens when a society based on caste enters the industrial age? This is really what the book is about. The "Bantu locations" of East London become a microcosm of a society on the verge of explosion. Along with the other books in the series, *The Black Man's Portion* stands as a tribute to the social scientists who have the courage to look in the face of this desperate situation in their own country and the integrity to report on it without flinching.

Three years before Reader began his survey, East London was the scene of the latest in a long series of attempts by Africans to modify the official policy of tight controls over physical movement and residence which has long been the Union's way of resolving the conflict between the need to have a large source of labor near the areas inhabited by the whites and the frontiersmen's fears of being swamped by a potentially hostile population subdued in war. The author devotes part of a historical introduction to an analysis of the violence which erupted when the campaign for passive resistance (or "general defiance," as the Union puts it) was prohibited and its leaders arrested. Although he

does not return to political analysis, the author clearly establishes that the social conditions underlying the "incomplete insurrection" of 1952 have, if anything, become more acute. It is not difficult, therefore, to provide one's own concluding chapter.

ARISTIDE R. ZOLBERG

University of Chicago

Factors Affecting Human Fertility in Nonindustrial Societies: A Cross-cultural Study. By MONI NAG. ("Yale University Publications in Anthropology," No. 66.) New Haven, Conn.: Department of Anthropology, Yale University, 1962. Pp. 227. $2.50 (paper).

This monograph, based on the author's Ph.D. dissertation at Yale University, seeks "to evaluate the fertility levels of a number of nonindustrial societies . . . , to learn the significance of various factors in explaining the main variations in the fertility levels of these societies, and to indicate the relationship, if any, between elements of social structure and factors directly affecting fertility." With these ends in mind, the author reviewed published and unpublished reports and selected for analysis the sixty-one societies, representing several geographic and cultural areas, for which there exists reasonably adequate information on fertility and at least one of ten other variables

By his efforts, Nag, who is associated with the Anthropological Survey of India, has added substantially to our understanding of reproductive patterns in various cultures. The statistical measures, many of which he had to compute, do not support the view that non-industrial societies have rates of fertility or population growth which are uniform and which clearly distinguish them from industrial societies. Further, as the detailed case histories of eight societies show, they have quite different configurations of attitudes and norms pertaining to coitus, child care, family size, and birth control. In addition to these findings, the study establishes the relationships between several physical and sociocultural variables and fertility levels and thereby confirms or refutes a number of hypotheses developed by Carr-Saunders, Davis and Blake, Ford, Krzwicki, Kuczynski, Lorimer, Notestein, and other scholars. Although the analyses demonstrate how fertility is depressed by prolonged *post partum* abstinence; high rates of venereal disease, sterility, and other single factors, they fail to show how particular levels of fertility result from the simultaneous interaction of several factors. This weakness of the study is frankly admitted by the author, who correctly attributes it to his reliance on data in which the omissions were too numerous for purposes of multivariate analysis. For the benefit of his readers, Nag provides sixty pages of appendixes which list the variate values that he was able to obtain for each society and the sources of every value.

There are other respects in which this monograph is especially significant It suggests to demographers that, in the absence of extensive and costly cross-cultural surveys, ethnographic data carefully selected and skilfully manipulated can answer uncomplicated but important questions. It emphasizes for anthropologists the importance of systematically collecting population data during future field studies, and it provides them with a sound introduction to basic demographic concepts and methods and the excellent analytical framework of Davis and Blake. Last, the study is significant because it represents one of the few efforts by an anthropologist to analyze and explain population dynamics.

EDWIN D. DRIVER

University of Massachusetts

A Communications Theory of Urban Growth. By RICHARD L. MEIER. Cambridge, Mass.: M.I.T. Press, 1962. Pp. viii+184.

This book attempts to rephrase some ideas concerning urban growth suggested earlier by Durkheim and Cooley—namely, that the rise of modern urban civilization was dependent upon the development of communications techniques. However, the role that Meier assigns to communications is not altogether clear although he seems to claim that the development of communications is the consequence of increases in population density and commerce. Meier believes that the characteristics of modern cities are that they have destroyed their natural environment, become dependent upon rural areas for a food supply, and

developed communications channels which permit the growth of culture.

It is these communications systems of modern cities that Meier proposes to study, claiming that the analysis of urban communications and of the time budgets of city dwellers offers more rewards than does the study of such topics as human ecology, geography, land economics, or traffic flow. However, he does not satisfactorily explain how to study urban communications, and he fails to demonstrate the usefulness of his suggestions. Rather he notes that once communications channels have been developed and their worth has been proved they tend to be overworked, and this impedes the cultural growth that should take place in cities. Communications channels may now be overloaded with redundant advertisements and unimaginative television programs. To rid communications media of this trivia Meier proposes a tax on advertising which would get progressively steeper as the number of exposures increases and a tax on popular television programs so that the viewing of television would drop either by ten per cent or to fifty hours per household per week. If this worthless material were removed from communications channels, they would be free, Meier believes, for transmissions leading to sociocultural improvements. Suggestions such as these fail to prove the value of a study of urban communications or the obsolescence of the traditional ecological approach to urban analysis.

Although Meier presents only a few basic data in this book, it would be difficult for a reader to discover exactly where he obtained his figures since he has an aversion to using clear and detailed footnotes, Meier also has a tendency to lapse into strained analogies in the physical sciences and elaborate explanations of natural science phenomena only remotely related to the study of urbanization.

REYNOLDS FARLEY

Population Research and Training Center
University of Chicago

The Urbanization of America, 1860–1915. By BLAKE MCKELVEY. New Brunswick, N.J.: Rutgers University Press, 1963. Pp. xi+370. $10.00.

The title of this volume does not adequately reflect the author's broad interests in cities and the many ways in which they have contributed to social change and the development of national institutions. In the course of fewer than 300 text pages, McKelvey, city historian of Rochester, New York, and author of a four-volume history of that city, ranges over an enormous amount of American social history.

Diffusion of urban sites, emergence of industrial cities, and their gradual transformation into a network of interlocking metropolitan regions are reviewed in brief chapters which draw on and supplement recent concern with a national system of cities. The focus then shifts to political evolution of cities, "the shame of the cities," increasingly successful reform movements, and development of a coherent city-planning movement. In a series of chapters, the author traces the role of the city in initiating new ideas and new organizations in the realms of labor, community welfare, religion, education, recreation, entertainment, science, and literature. Concluding chapters on "The Triumph of Metropolitan Regionalism," "Civic and Social Armageddon," and "Cultural Fulfillment and Disenchantment" reiterate the author's thesis: "it was because their constant adaption to . . . changes afforded leadership in the broader currents of American life that the growth of cities acquired historical significance."

Seventy pages of notes and bibliography reflect McKelvey's immense reading and should reward perusal by any student of American social history during the period covered. Sociologists will appreciate the wise use the author has made of their writings in developing his perspective on urbanization and the impact of the city on American society. The book remains, however, a historical interpretation on a broad scale, providing neither the mine of detailed information contained in works such as the author's history of Rochester, nor the systematic marshaling of evidence that could elucidate the empirical applicability of his numerous generalizations. In his zeal to relate so many developing social movements and processes of social change to cities, the author emerges with endless paragraphs crammed with names, places, and dates illustrating his points.

Despite the fact that urbanization may not have been in every case the prime mover McKelvey portrays, sociologists of many specialities can find in his work many facts and ideas to stimulate their own investigations of American social organization.

KARL E. TAEUBER

University of California, Berkeley

Beyond the Melting Pot: The Negroes, Puerto Ricans, Jews, Italians, and Irish of New York City. By NATHAN GLAZER and DANIEL PATRICK MOYNIHAN. Cambridge, Mass.: Harvard University Press, 1963. Pp. 360. $5.95.

It must be confessed that American sociologists have been slow to admit that ethnicity is an important factor in American society. Despite the recent publication of Herbert Gans and the periodic comments of Bernard Rosen, ethnicity is usually treated like something that happened back in the time of Thomas and Znaniecki, but can hardly be expected to be taken seriously today. Surely this refusal to think about ethnicity can only be construed as a major failure, since it is perfectly clear to anyone who spends even five minutes considering a city like New York that ethnicity is of immense importance.

Glazer and Moynihan, in a work which is frankly exploratory, try to focus on the meaning of ethnicity in an American metropolis. Their basic contention is that "the ethnic group in American society became not a survival from the age of mass immigration but a new social form." Indeed the immigrants were not Irish but Kerrymen, not Italians but Baresi before they came to New York. They continue to be Irish or Italians long after the cultural content of the Old World heritage has vanished. Thus, the authors suspect, the ethnic groups are not cultural remnants, but interest groups (and perhaps identity-bestowing groups) which are likely to remain for a long, long time.

To the present reviewer this is an extraordinarily exciting suggestion and one which may make a major contribution to the understanding of the social structure of great American cities. Much more rigorous investigation will be necessary before it can be considered to be proven, but Glazer and Moynihan have pointed the way to a very important research question. One wonders if they are not perhaps too hasty to think that all the Old World cultural content has gone from the ethnic groups (although Gans makes the same suggestion). Peter Rossi has recently observed that there may still be many subtle modes of interpersonal behavior which differentiate the ethnic groups. Such a hypothesis would also be worth further investigation.

Glazer's chapters on the Negroes and the Puerto Ricans are especially interesting, although his comments on the failure of integration to produce the desired effects in the New York schools and on the need for self-improvement within the Negro community are hardly likely to be favorably received in many circles; this is not to suggest that they are not valid, however. In addition, Glazer's point that as the Italians become more middle-class they become better Catholics (even to the extent of sending their children to parochial schools) is an insight that may give pause to non-Catholics and Catholics alike—although perhaps for different reasons.

Moynihan's chapter on the Irish—principally concerned with the decline of Irish political power in New York—is, to the present admittedly bigoted reviewer, charming and gently ironic. The irony comes from the fact that the account of the eclipse of Irish political power is presented by an assistant secretary of labor who surely must be considered a dues-paying member of the Irish Mafia. As Moynihan observes, "a new era began on the day when the President of the United States and the Prime Minister of Great Britain were photographed at the rail of the presidential yacht, standing above a large life preserver on which was emblazoned, 'Honey Fitz—Washington D.C.'" Indeed, ethnicity is not without some residual importance in American society.

ANDREW M. GREELEY

National Opinion Research Center

Intermarriage and Jewish Life: A Symposium. Edited by WERNER J. CAHNMAN. New York: Herzl Press and Jewish Reconstructionist Press, 1963. Pp. 212. $5.00.

For those concerned about Jewish survival, this book will be of interest, though the symposium format at times necessitates sheer will power for the reader to keep going. For others, including those interested in the theoretical sociological-cultural problems of subcultural survival and intermarriage, it is a book that may well be bypassed.

There are two strands in the book: the state of knowledge about marriage of American Jews with those of other religious identifications and a proposed moral orientation toward such intermarriage. Rabbis, educators, and social scientists—all of whom, with one exception, are strong Jewish survivalists—were assembled in 1960 to have their individual say on these two themes.

Part I, "Twofold Guidance," wastes thirty-two pages. R. J. R. Kennedy does our profession a disservice by repeating, essentially, what Park, Wirth, and others said more coherently many years ago on "What Has Social Science To Say about Intermarriage?" The educator Ben-Horin is shriekingly and confusedly against intermarriage. Period.

Part II should be entitled "Facts as We Don't Know Them." Baar and Cahnman, in a not very useful paper, and Rosenberg, in a very solid, cohesive summary, present intermarriage data for Switzerland and Canada, respectively, based on official sources. Rosenthal brings together the few local studies which report intermarriage data, but pays most attention to the 1957 national census survey, which reported that 7.2 per cent of the married couples involving Jews had one non-Jewish spouse. Maier, and Zuckerman and Chenkin, in two papers which should have been one, present a good, long list of disparate questions that need to be researched: What are the intermarriage rates? What are the variables influencing rates? What are the results of intermarriage?

Strand two appears in the two concluding and most interesting papers by sociologist Cahnman and Rabbi Cohen. As humanists, democrats, Americans, and realists, they reject the traditional "building-a-fence" means to the goal of Jewish survival. Since survival is nonetheless profoundly desirable (though neither has room to spell out why he thinks and feels so), Jewish life must be made attractive. Intermarriage, then, will not only cease to be a threat, but become a recruitment source. The logical conclusion is accepted by both: the dominant hostile attitude to conversion should be dropped. Eichorn's review of official attitudes to conversion, and Rubenstein's psychoanalytically oriented case histories of conversions which came to his attention as campus rabbi, both of which appear earlier in the book, are, in this light, relevant.

AARON ANTONOVSKY

Israel Institute of Applied Social Research

Cities and Civilization. By G. S. GHURYE. Bombay: Popular Prakashan, 1962. Pp. vi+306. Rs. 20.

Cities and Civilization is peripatetic, with a perspective ranging from demography to history to sociology to common sense. Such breadth does not limit its value to the lay reader for many specific points are made for the specialist as well.

Dr. Ghurye first sets up a framework of the natural history of cities with sources ranging from Weber to Mumford to Eliel Saarinen. Although only the United States, the United Kingdom, and India are analyzed in great detail, data on the growth, number, and location of "great" cities for fourteen countries are presented. While Ghurye's use of population data is at times somewhat perfunctory and his conclusions such as that "million mark cities are presently drawing to themselves a smaller share of the total population than smaller cities" are not startling, Ghurye must be commended for taking a definite comparative approach to urbanism.

Second, Ghuyre examines Toynbee's "law" that the location of capital cities will be in the "marches" of civilization and finds it inadequate. Whether surveying a myriad of historical materials or reconstructing the population of Memphis, Ghurye here has succeeded in a real synthesis of data rather than piecing together summaries of scattered data. Using Egyptian, Chinese, and Indian evidence, he points out that the economy, religion, and culture—as well as frontiers—determine city size and location. The beginning student is offered a glimpse into the controversies that beset researchers of primate cities, and the specialist is given a sound reinterpretation of city location.

Last, the scholarly meanderings of Ghurye are brought to an examination of Bombay. He discusses its position, from both historical and demographic evidence, as the unofficial metropolis of India. Since he dwells on specifics, his solutions for current problems are probably of interest only to Bombay city planners. This is a disappointment, as the breadth of generalizations is a high point of the earlier sections of the book.

The book is admittedly a search for the meaning of the city so that the future can be shaped by that knowledge. The search is by no means ended, but we are farther along the way.

FRANK J. ZULKE

Community and Family Study Center
University of Chicago

Education and the Cult of Efficiency. By RAYMOND E. CALLAHAN. Chicago: University of Chicago Press, 1962. Pp. xii+273. $5.50.

Educators pressing for an enlarged professional mandate over the affairs of the public schools keep stumbling over the hard fact that, although their hearts are pure, their claims to a monopoly of expert knowledge are weak. Thus, while it is clear that the intrusions into the schools made by businessmen, veterans' groups, and other outside interests have often been detrimental to American education, it is also apparent that educators have had little in the way of a solid defense against these intrusions. Moreover, in the search for either an esoteric or popular claim to legitimacy, educators frequently have been ready to jump on the bandwagon of anyone who appears to provide them with some new ideology which could secure and improve their status and power.

A new and significant case in point is provided by Callahan, who presents a detailed study of the movement to bring efficiency and scientific management into the schools during the period roughly bounded by 1900 and 1930. In these decades Taylorism was in vogue, the schools were under strong public attack, there was great pressure from outside to cut costs by adopting the presumably efficient new methods of business, and school administrators, insecure in their jobs, eagerly took up the cudgels of scientific management. The result, Callahan shows, was to divert attention from educational quality to educational cost-accounting and cost-cutting; to increase the amount of pseudoscience in education; and to weaken educational quality by furnishing short-sighted rationales for such practices as the double and triple shift. Worst of all, the author concludes, the legacy of this movement continues: the cult of efficiency was warmly accepted in university schools of education and it provided a conceptual basis for professionalizing the superintendent as an expert economizer. As a consequence, Callahan says, the educational establishment still contains many influential administrators and professors who wrote dissertations on plumbing and pupil accounting and who know more about the manipulation of teaching loads than about the development of good practice with children.

As a piece of research and analysis, despite the many interesting materials and interpretations, the book has serveral deficiences. First, there is a tendency toward gleeful muckraking, relying frequently on streams of absurd quotations interspersed with the author's condemnations of the quoted. Second, the post-1930 materials are weak for the assertions made. Third, as the author recognizes, his attempt to ascribe the rise of the cult of efficiency mainly to the exposed position of the administrator breaks down when he discusses its acceptance in the professional schools, and his descriptions of the biographies of the professors important to the cult do little to rebuild an adequate explanation. Such an explanation would also require a fuller recognition of the educators' search for a publicly accepted basis for professional claims and, self-interest aside, the power that a popular concept like "scientific management" can have in the culture generally.

Finally, the author's line of argument itself reflects to some extent the weakness of the education profession's ideological position. He is certain that the efficiency-seekers were wrong, but his reply is only the oblique one that quality should be more important. The analysis and the educational philosophy underlying it do not seem to come to terms with the realities that education is involved in the local political process, that resources are never what the educator "needs," that the superintendent must act as a mediator between values of education and economy, and that

he must use whatever managerial knowledge he can find. There can be no utopian concern with quality alone.

Whatever its shortcomings, the book is important. It may spur thoughtful controversy in education, and it should stimulate sociologists to turn their attention to the broad problem of the interrelations of school, community, and university.

DAVID STREET

University of Chicago

Education in Depressed Areas. Edited by A. HARRY PASSOW. New York: Bureau of Publications, Teachers College, Columbia University, 1963. Pp. xiv+359. $4.75.

The papers in this volume were given at a conference of educators, psychologists, sociologists, and others interested in the problems of educating lower-class children in an urban environment, held at Teachers College, Columbia, in 1962. The book gives attention to the general role of the urban school, the sociological and psychological problems of lower-class children and their educational effects, teacher recruitment and training, and school programs. As editor, Passow makes useful statements throughout, reviewing the papers and the literature in general, posing thoughtful questions, and, in a concluding chapter, summarizing the major dimensions of the problem and citing current action programs across the country.

Many of the contributors spend a great deal of time describing the now familiar slum environmental with all of its horrors. In addition to giving this descriptive material, each contributor has his or her) special slant. Mel Ravitz sees the problem as basically one of assimilation. Sloan Wayland leaves one with the impression that things really are not so bad when he attributes the concern over the educational plight of lower-class children to the generally higher aspirations of American society. Miriam Goldberg sees the problem in somewhat more complex terms. She cites the newer conditions of the urban environment (larger population, greater heterogeneity, and new mobility patterns) and outlines their consequences for academic achievement in depressed areas. Similarly, Martin Deutsch rerelates environmental circumstances to the dynamics of learning, and in so doing delineates the reasons for the academic disabilities of slum children.

The papers by Robert Havighurst and Alan Wilson, along with the one by Goldberg, give impressive evidence as to the durability of mixed-class schools. David and Pearl Ausubel do an effective job of outlining the social and economic conditions conducive to the development of weak ego-structures among Negro children and the consequences of such for their education. In a less imaginative way, Kenneth Clark concerns himself with the age-old dilemmas of I.Q. tests, homogeneous grouping, and the middle-class-oriented school. His stab at Conant's *Slums and Suburbs* is worth considering, however. The finding reported by Richard Cloward and James Jones, that increased exposure to the school on the part of adults may lead to less favorable impressions of it, has important implications for the school in the move for greater involvement of lower-class parents.

The papers on teachers, by Vernon Haubrich and Leonard Kornberg, both stress the importance of revamping teacher training to acquaint prospective teachers with the problems of the slums. It seems that more should have been said about in-service training for the thousands of teachers already in the schools. The book concludes with papers on school programs. John Fischer gives broad guidelines to desegregating schools, while Carl Marburger describes his "Great Cities Program" in Detroit. Finally, Henry Saltzman urges the use of the community school model to integrate the formal school and after-school activities of the lower-class child.

This book is an excellent introduction to the existing knowledge on this problem. Perhaps its greatest asset is heuristic. It suggests a multitude of compelling researches linked to the approaches of various disciplines.

MARY A. QUEELEY

University of Chicago

Voice of American Medicine. By JAMES G. BURROW. Baltimore: Johns Hopkins Press, 1963. Pp. xii+430. $7.50.

One of the important topics in the sociology of professions is the function of the professional association for both the occupational

community and for the larger society, but hardly any sociologist has done research in the area. Rather, our knowledge about particular associations comes from official histories or from infrequent monographs by scholars in other disciplines. Because it is the spokesman for the most developed profession and because it is frequently involved in public controversies, the American Medical Association has attracted more attention than any other: Oliver Garceau has written about its internal politics, the A.M.A. itself has published an official history of its scientific and professional activities, and now James Burrow presents the history of the A.M.A.'s political demands and relationships with government.

Burrow writes with the techniques and viewpoint of a historian. His purpose is a thorough summary of the facts from the founding of the A.M.A. to the present. The result is an invaluable codification of the literature, and the thorough documentation makes it the basic sourcebook for any future scholar seeking to probe more deeply into particular topics in the A.M.A.'s political history. Popular and scholarly discussions of the A.M.A. are often marred by bias, but one of the virtues of Burrow's book is its dispassionate reporting of facts, an indispensable quality in any historical sourcebook. Because he covers the A.M.A.'s entire history, he summarizes much unfamiliar information about the political aims, legislative victories, and defeats during the association's first century. Few of us are familiar with more than recent events.

Because Burrow's aims are quite enough to fill one book, there are several things that he does not attempt to do. Since the book concentrates on the A.M.A.'s political policies and pressures, it says little about the association's other functions or about the state of medical practice. Since it focuses on the national office of the A.M.A., it says little about the state medical societies and the important political events at state and local levels. Being a thoroughly documented and detailed historical sourcebook, it lacks the literary grace for informal reading. As it must cover many events extending over nearly a century and a quarter, the book has space for no more than a familiar summary of the recent controversies over national health insurance.

The author's exclusive reliance on published documents may cause the book to fall short of its potentialities. The best studies of pressure-group influences on legislatures and on administrative agencies are based in large part on informal interviews with participants and research in unpublished files, but such "inside dope" seems absent from Burrow's book. Concern with the A.M.A.'s public statements and legislative petitions may lead to an undue neglect of its important role in some election campaigns, such as the 1950 campaigns in some western states. The sociologist might wonder how the various divisions within medicine and within the A.M.A. affect the association's political stance and how turnover in leadership affects policies and tactics, but Burrow's concentration on the A.M.A.'s official statements leads him to describe it as a relatively monolithic and only slowly changing body.

William A. Glaser

Bureau of Applied Social Research
Columbia University

The Judicial Process: An Introductory Analysis of the Courts of the United States, England and France. By Henry J. Abraham. New York: Oxford University Press, 1962. Pp. xii+381.

Described by the author as a "selective comparative introduction to the judicial process" which "seeks to analyze and evaluate the main institutions and considerations affecting the administration of justice under law," *The Judicial Process* offers the social scientist a literate, non-jargonized link between the social and the legal dimensions of human interactions, tensions, and conflicts.

Beginning with the postulate that respect for the law is essential to the effective and equitable operation of popular government, Abraham proceeds to probe the complexities of law as concept, system, and process. He describes thoroughly yet succinctly the structures through which law emerges and evolves in the United States, England, France, and the Soviet Union; and he refers at relevant points to other legal systems of Europe. A basic recurring theme is the relationship between policy views of the courts and judges

and policy views of the larger society. He suggests that, while judges need not literally follow election returns, their views do not long remain out of joint with those of the majority. Self-imposed maxims of judicial restraint modify if not mollify the polarities of ideology that might otherwise produce stalemate or revolution in societies that allow judicial review.

Abraham focuses in greatest depth on the role and functions of the United States Supreme Court as a policy-making institution. That he does this with balance, precision, wit, and an absolute minimum of pedantry distinguishes his volume from the unleavened literature on the judiciary that has thudded forth in the last few years.

If pedantry is avoided in the volume, there may still be room for cavil in the review. Only five pages are devoted to the problem of compliance, and even there the focus is on compliance by legal structures with the norms prescribed by those at the apex of the judicial hierarchy. Only indirectly, in the chapter on juries where "the intuitive part of us" is examined as counterbalance to "the intellectual part of us" and jury proclivities for compromise are probed, does Abraham deal with relationships between formal legal compliance by court opinions and actual social compliance as embodied in behavior. Social problems that enter the legal process are not transmuted permanently and irrevocably into legal problems. One might wish that an analysis of the judicial process would highlight judicial linkages both with pre-decisional social factors out of which the action at law arose and post-decisional social factors that, as responses to judges' decisions, determine the efficacy and help to mold the subsequent contours of the judiciary's role.

Lack of fulfilment of a hypothetical wish places no blemish on what is otherwise the most lucid and perceptive introduction to the judicial process since Cardozo's day.

VICTOR G. ROSENBLUM

Northwestern University

Mental Health in the United States: A Fifty-Year History. By NINA RIDENOUR. Cambridge, Mass.: Harvard University Press, 1961. Pp. viii+146. $3.50.

Like much that is worth attention, "social problems" are not born fully grown. They are nurtured and cultivated. In felicitous style *Mental Health in the United States* presents a brief, competent overview of a social movement which began by focusing attention on the special problems of the approximately two-tenths of one per cent of the national population who were in mental hospitals in 1908. In only fifty years this movement made the care and prevention of emotional disturbance a prominent social problem. Nowhere else and never before has emotional imbalance been a subject of national concern.

To be sure, no social movement is altogether successful, for then it ceases to be a movement. Yet "thousands of mental patients in the United States are getting better care than they ever had before. . . . More funds for research are becoming available. . . . In 1958, an opinion poll indicated that next to education, the American people are more willing to be taxed for the care of the mentally ill than for any other major public service." Compare this with the world of "lunatic asylums" and "alienists" of the year 1908, when Clifford Beers, an ex-mental-hospital patient, organized the Connecticut Society for Mental Hygiene—"the beginning of the organized mental health movement in America."

This history illustrates what I believe to be five necessary, if not sufficient, conditions for a personal tragedy like mental illness to become a social problem. First, I think, to be a social problem a situation must be dysfunctional to some or all of a society. Thus Ridenour shows that inefficiences attributable to mental disturbance became most noticeable during our times of maximum collective effort—World Wars I and II. Second, a true social problem offends the moral sensibilities of a good part of the population. Indeed, muckraked America was ready to be moved by the early systematic surveys of mental hospitals. To effect this second condition and to make dysfunction more than an analytic idea, a third condition, the lifting of pluralistic ignorance, must be met. In this book, problems of public education and information as they bear on the moral, financial, and technological aspects of mental disturbance are given a balanced review. Fourth, to move through the gateway of public interest and emerge as a moral issue, a social problem must

acquire a name. The name is a value concept, an idea defined only in action, which rallies social forces to wrestle with the issue thus delineated. In a thoughtful section, Ridenour discusses the troublesome words "mental hygiene" and "mental health," and relates these terms to some structural imperatives of the movement. Finally, a social problem must be instituted in the form of organizations, professions, and publics. Not even "crime" is a social problem unless there are police. Much of the book is therefore devoted to the difficulties of instituting a new value concept. Problems raised by relationships between the movement's elite and its mass, by relationships between organizations both within and without the movement, and by comparisons between the movement's stated goals and their actual realization are all touched upon, though not in these terms, in Ridenour's laudatory yet occasionally critical account. Students of organizations may be stimulated to review these issues in greater detail. Comparisons between the mental health units and other voluntary organizations should be rewarding. Why in the twentieth-century United States mental disturbance has met the five conditions which make for a social problem is also a question deserving of further study.

The book's non-academic orientation is responsible for its lack of either an index or footnotes Nonetheless, the basic facts about the origins and development of the mental health movement are made available to those in any way affected or concerned—and this should include a number of sociologists.

CHARLES KADUSHIN

Columbia University

Troublemakers: Rebellious Youth in an Affluent Society. By T. R. FYVEL. New York: Shocken Books, 1962. Pp. 347. $4.95.

It is the thesis of this "British social and political analyst" that "Teddy boys" and their like in other countries are the "also rans" of the affluent society. The demons of Fyvel's analysis are commercial entertainment and the advertising agencies, the British systems of education and youth services, and "an alarming drive towards purposelessness" in British society. The former stimulate young appetites for cheap goods and entertainment and create much of the form of "commercial youth culture." Fyvel observes that even in the midst of the blaring pleasures of commercial youth culture, the youngsters seem "bored nearly all the time," shifting from one temporary alliance to another without real friendship. The culture lacks purpose for these young people, and because it leaves them unsatisfied "they are pushed to go too far and get themselves in trouble."

Fyvel's analysis includes the entire sweep of recent British history. One cannot help but admire such penterating critique as is leveled at the mass media, advertising, and commercial entertainment, at the British educational system which he accuses of perpetuating invidious class distinctions, and at youth services which retain an antiquated and inappropriate "charity" atmosphere in the affluent society, being virtually ignored in Britain's tremendous new housing developments. Fyvel charges that working-class youngsters find schooling unchallenging and unrelated to life as they understand and anticipate it. Many are left behind in the competitive struggle for technical and cultural advance. Commercial youth culture lures them from school at the earliest possible moment (fifteen years for most), easy-come, easy-go jobs put money in their pockets and undermine the older apprentice system.

Fyvel looks to Europe—east and west—and to the United States for confirmation of his thesis and, by and large, finds it. The analysis of conditions in eastern Europe suggests a different basis for youthful rebellion, however—here "the young rebelled against drabness, against regimentation, against political unfreedom and oppression." In every country examined the age of "14 or 15 upwards" is found to be bored, dissatisfied, and therefore in rebellion.

Fyvel has availed himself of governmental reports and more popular sources of information in Britain and other countries, interviews with educators, employers, police, youth workers, and young people, and visits to many countries. One cringes at the ease with which he generalizes impressions and advances solutions, but the book abounds with hypotheses worthy of inquiry. The most disquieting experience in reading the book for me was his discussion of the state of delinquency and

knowledge thereof in the United States. Here his sources tend to be either dated or overly sensational. Unfortunately, research is virtually non-existent which would test his basic proposition that "the voice of advertising has become that of the super-ego" and that this, in turn, reflects and is a part of "the growing social unbalance in the affluent society."

JAMES F. SHORT, JR.

Washington State University

Persistent Criminals. By W. H. HAMMOND and EDNA CHAYEN. London: Her Majesty's Stationery Office, 1963. Pp. ix+237. 25*s*.

To sociologists in general, as well as to criminologists, this book is an important contribution to the literature on prediction. Methodologically it is an equal—and in one sense a sequel—to the Mannheim and Wilkins study. Hammond and Chayen use and fully describe what appear to be the best available statistical tools to analyze their data. They are quite aware of the limitations of some techniques, point them out to the reader, show reasons for choosing one tool instead of another, explain their multiple regressions, scoring devices, and how and why they made certain intricate calculations. The book is a model of design and evaluative research. Moreover, the authors are willing to make recommendations but do not presume to make policy. If the goals of society or the courts or some agency are thus and so, these authors give substantial empirical evidence to show the deficiencies of some current operations and the most efficient means of achieving those goals. They appear, however, to refrain from making the ethical decisions; and this is perhaps as it should be.

Substantively, the book is about offenders in England who become liable to preventive detention, which loosely refers to a sentence given to a person thirty years of age or older, convicted of an offense punishable by imprisonment of two years or more, and who has been convicted on at least three previous occasions since age seventeen, with at least two of these convictions resulting in a sentence to Borstal training, imprisonment, or corrective training. Males sentenced to preventive detention (178) in 1956 are compared to male offenders liable to preventive detention (1,384) in that year, and to all males aged thirty or over convicted (6,171).

The study is divided into two main parts, the first of which compares offenders sentenced to preventive detention with those given other sentences, including analysis of factors influencing the kind of penalty, the effectiveness of preventive detention, or other sentences given to persistent offenders, the most important factors in the criminal histories of the 1956 "liable" offenders given sentences other than preventive detention in relation to subsequent reconviction, and a similar analysis of reconvictions of offenders released from preventive detention from 1952 to 1957. An effort is made to determine how serious were the offenses of "liable" offenders and the danger these men present to the community when free. The cost value alone is used to judge property offenses.

The second part deals with more subjective social and psychological data and makes comparisons with Norval Morris' earlier work, *The Habitual Offender* (1951). Some of the most interesting methodological exercises appear here: predicting the probability of recidivism, comparison of statistical estimates with the advisory board's allocation and with the prison staff's forecasts of reconviction.

Space precludes presenting findings, but they are enlightening and discussed analytically. Reliability of data is not overlooked and an interesting validation study is reported. In sum, this book is another fine product of the Home Office Research Unit and adds to an increasing sophistication of criminological analyses.

MARVIN E. WOLFGANG

University of Pennsylvania

The Structure and Dynamics of Organizations and Groups. By ERIC BERNE. Philadelphia: J. B. Lippincott Co., 1963. Pp. x+260. $7.50.

The author of this book with its ambitious title is a practicing psychiatrist who, in addition to participating in "about 5000 shifting

situations" in different types of groups, has taught and supervised group therapists and served as a consultant to numerous group leaders. The book is not designed as a "handy manual" for leaders but is intended to offer a "systematic framework for the therapy of ailing groups and organizations."

All groups—organizations are merely more complicated types of groups—may be dissected, according to Berne, into the following analytical components: (1) the "public structure" composed of the formal charts of positions and the personnel who occupy the organizational slots; (2) the "group authority," which includes the leadership and the group canon; (3) the "private structure" consisting of the "group images"—the ideas held by individual members concerning what the group is or should be like; (4) the "group dynamics," which consist of the interplay of three basic forces—cohesion, internal disorganization, and external disruption; and (5) the "group process" composed of the transactions which take place among individual members, analyzed according to whether the persons are acting as "parents," "adults," or "children."

The major analytical device employed is the topological diagram in which structures and forces of the above type are presented and discussed as a means of "explaining" events occurring in particular groups. A spiritualist meeting, a psychiatric clinic, and a psychotherapy group are among the groups examined. An imaginative lexicon is developed—for example, a primal leader of the past is labeled an "euhemerus"; a "Turgot group" is one that devotes all its energy to one kind of work. The language is so special, in fact, that the author has felt the necessity of appending a fourteen-page glossary to aid the reader.

The major product of this book is a set of descriptive categories which although sometimes novel seem neither particularly precise nor especially productive of new insights or hypotheses. The hypotheses which Berne himself proposes are more in the nature of definitions—for example, "the proceedings of any group are regulated by two authorities: the leadership and the group canon" (p. 113).

In an appendix devoted to "suggested reading," references are cited dating from 3000 B.C. to the present. Berne, however, indicates little interest in the research of the twentieth century, which because it consists of operations that "can be performed by machines" contains "few creative ideas as compared with the older writings." Berne's own formulations seem, to this reviewer, not so much creative as eccentric.

W. Richard Scott
Stanford University

Behavior in Public Places. By Erving Goffman. New York: Free Press of Glencoe, 1963. Pp. vii+248. $5.95.

This book is directed at developing a conceptual scheme for describing the normative structure which governs face-to-face interaction, especially in situations in which the participants are not fully free to be themselves (hence the reference to "public places" in the title). It is, of course, possible for any moderately ingenious man to multiply distinctions and develop concepts. But to be useful a conceptual scheme must be informed by an idea of the motors of social action; the only justification for classifying two phenomena together and distinguishing them from others is that we think the two phenomena classed together play the same role in a scheme of explanation, that their differences are irrelevant and their similarities crucial.

Evaluating Goffman's conceptual developments dispassionately in relation to a conception of the motors of social activity is difficult, because his examples are too interesting in themselves and he is too illuminating in discussing them. But it seems to me that Goffman has launched a fundamental attack on the main conceptions of social motors held by sociologists, and that this book must therefore be evaluated in more pretentious terms than it presents itself in.

The implicit justification for Goffman's conceptual work here, as I see it, is that many of the connections between social structure and individual behavior are not made through the mobilization of powerful personality drives to secure the performance of social roles, but rather through normative definitions of situations in which relatively weak and shifting motives are mobilized for the purpose at hand.

Or another way of stating it is: people's social behavior varies more with differences in the norms governing action at particular times and places than it varies with the organization of their personality into social role behavior. For instance, the degree of aloofness or social distance of a professor from a student varies more between the time during lecture and the time after lecture than it does between professors or between students. Society keeps going not because it has built a deep self-conception as student into those who take the role, but because it uses relatively minor motives to keep people quiet while others are lecturing.

It seems to me that this notion is fundamentally right for most social phenomena. Only for a few phenomena, though some of these (like occupational choice) are crucial, are fundamental motives, or personality characteristics, or role commitments the explanation for variation in social behavior. Most of the social world is too mundane to mobilize whole personalities. It seems therefore that the direction of the thrust of Goffman's theorizing is correct. Furthermore, the concepts developed here, along with some others from previous work, seem to me to be a fruitful start in ordering such variables for explanatory purposes.

What worries me about the book is a characteristic it shares with quite a lot of theoretical work in the status-role tradition, which I would call a failure in explanatory commitment. In Goffman, as in Parsons or Simmel, it is relatively rare to come across statements of the general form of "I think the explanation of . . . is . . . ," or of the form "The greater the degree of . . . , then, the greater the degree of . . . that we will expect." Empirical materials play the role of illustrations of a distinction, rather than evidence for an explanation.

As is usual with Goffman's work, the writing has grace, wit, clarity, and modesty. The book can be read for pleasure by those who do not care about the future of sociology. And it ought to be read carefully and seriously by all who do because it is good evidence that the fundamental theoretical presuppositions of most sociology are precarious.

ARTHUR L. STINCHCOMBE

Johns Hopkins University

The Small Group: An Analysis of Research Concepts and Operations. By ROBERT T. GOLEMBIEWSKI. Chicago: University of Chicago Press, 1962. Pp. xii+303. $6.00.

Mr. Golembiewski suggests that there have been two methods used by social scientists in attempting to develop exhaustive summaries of small-group phenomena: factor analyses and surveys of the published reports. His own book is of the latter type. The basic variables by which small groups are described are, he thinks, of three types: *structure, style,* and *population.* The distinctions between the three are not entirely clear. The first two include attributes of the collectivity; the third consists of characteristics of the individual members. The ambiguity seems to lie in the difference between structural variables and style variables. For example, why are the group-dynamics experiments with authoritarian versus democratic groups discussed under *style,* while leadership comes under *structural?* However, he gives a number of examples of dimensions under each heading.

A particularly interesting subject, referred to at six different points in the book, is the discussion of differentiation in leadership roles. Three quite distinct roles emerge: a person who is most expert at handling whatever collective enterprise the group has undertaken, a person who makes the interaction enjoyable to the members, and a person who is more active than anyone else. This small theory "covers" a large number of research studies. The roles tend to be separated but, under some leaders, may be combined. Operationally, the three roles are quite different. Whereas both expertness at the group task and activity can be measured by observation, the social role is a matter of subjective evaluation by the individual members and is fundamentally sociometric, in Moreno's original meaning. In some of the main experiments from which this generalization was derived, ratings by members were used to measure both expertness (best ideas, guidance) and sociability (liking). The former are actually indirect behavioral measures, although the observation was by the members, while the latter concept calls for a psychological measure.

The author gives considerable importance to the basic dimension, cohesiveness. The attractiveness of the group for its members, the sociometric structure and the status ratings of

other members are apparently all related features of groups and derive from identification between members. The significance of the sociometric structure and of cohesiveness for the group is not always noted by the outside observer unless he is able to adopt the perspective of the members, for he may tend to see the group in terms of overt interaction while they perceive it in terms of motives and feelings. Golembiewski says, "theoretically, cohesiveness is the essential small-group characteristic" (p. 149).

The problem of defining "small group" is discussed at the outset but perhaps not followed up to the extent it might profitably have been. It is not always clear which findings hold true for all types of groups and which do not. The author lists three usages: (1) a small set of persons who have interdependent relationships and a culture, (2) a set of persons, few enough that they can identify one another, who meet together (Robert F. Bales's definition), and (3) any set of persons, regardless of the criterion by which the scientist classified them together. There is no question of the value of developing a typology of groups. However, there are many people whose writings are included under the heading of "small groups" who are primarily interested in face-to-face interaction and simply chose the small group as a convenient but significant example of it. Definition 2 follows the lines of formal sociology.

This question partly extends into the widely discussed distinction between "real" or "natural" groups and "laboratory" or "experimental" groups, a distinction which has never been clear. In the future it will no doubt be an advantage to carry out more observation of established groups, as the author suggests. However, if a group wants changes, and many groups do, there is no reason to abandon experimentation.

CHARLES H. HAWKINS

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Social Influence and Power in Two-Person Groups. By ANTTI ESKOLA. ("Transactions of the Westermarck Society," Vol. VI.) Copenhagen: Munksgaard, 1961. Pp. 153 (paper).

Two related studies of power and relative power among dyads are described in this short volume. The research objectives focus on the evaluation of some measures of relative power and the personal and interpersonal characteristics which are related to the exercise of power.

Nearly three hundred foremen and college students were given nine evaluative tasks, some with objectively correct answers, others subjective in nature. A typical task called for the subjects to rank candidates for prime minister of Finland. Each subject responded to each task three times: once alone; once with a partner with whom he was obliged to reach a joint evaluation; and finally, alone once more. Power is measured by the extent of convergence of either or both toward the other's initial stand on each task. The author's presentation of his own measures of power, including one which incorporates shifts in both "public" and "private" opinion, is a model of clarity and order.

His analysis of the "group" phase, by correlation and factor analyses, gives extensive attention to the ways in which subjects reconcile originally diverse views. In connection with this, the analysis of compromise and conversion reactions is of special interest. It is one of the few systematic and empirical treatments of these social processes in the literature.

The findings are related to a broad set of problems and hypotheses in social psychology and sociology, including the measurement of power, Festinger's analysis of compliant behavior, the effects of initial degrees of acquaintance and friendship upon one's relative power, and Bales's three orthogonal factors in leadership.

As is so often the case, the author's allocation of space to interpretation might be questioned. Further, there are questions concerning the design and analysis. Since the subject's level of involvement in the outcome of tasks was relatively low, I wonder what more general significance can be attached to evidence of power among them, the tendency to compromise, and the stability of the joint decisions when the subjects were re-examined alone. What effect might allowing the subjects to remain in disagreement have upon the findings? What were the mechanisms used to achieve agreement when subjects initially dif-

fered little from one another as distinct from when they disagreed widely? So much is condensed into this short volume that the author could not comment upon these and many other questions, nor was there space for occasional summaries of analyses or discussions of the implications of his rather provocative evidence. Despite these reservations, the author has demonstrated considerable competence and ingenuity in the design and execution of his research into this area of the analysis of power.

DONALD P. HAYES

Cornell University

Religion and Economic Action: The Protestant Ethic, the Rise of Capitalism, and the Abuses of Scholarship. By KURT SAMUELSSON. Edited and with an Introduction by D. C. COLEMAN. Translated by E. GEOFFREY FRENCH. New York: Basic Books, 1961. Pp. xii+157. $3.75.

Keynes said that "practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist." Armed with a vigorous skepticism and a devotion to the historical method, Samuelsson has set out to free us from Max Weber—or at least from his thesis that Protestantism created the preconditions for a "spirit of capitalism."

Unlike many revolutionaries who challenge revered figures in the scholarly world, Samuelsson is neither cheap nor shrill. He believes that several people have been wrong: Sombart, Brentano, Fanfani, Tawney, Parsons, and Weber. But there are no *ad hominem* attacks. Samuelsson treats their hypotheses respectfully but not reverently. He organizes an enormous range of data with care and precision to test the hypothesis that there is a correlation between religion and economic action, and he rejects the hypothesis. He makes a persuasive case.

The starting point for Weber's work was Martin Offenbacher's study, published in 1901, of the occupations of Protestants and Catholics in Baden. Samuelsson shows that there were errors in Offenbacher's figures and that Weber used these erroneous data.

The Weberian correlation between religion and schooling, based upon enrolments in Baden, turns out to hinge upon the fact that in certain towns with a large Protestant majority in an area otherwise predominantly Catholic there were more Protestants than Catholics in the secondary schools. When the religious denominations of the schoolchildren are put against the demographic distributions in each district, the difference between Catholics and Protestants in propensity for schooling vanishes.

Similarly, differences in capital assets between the denominations dwindle when rural-urban differences are brought into the computations. People who live in towns tend to invest their money differently from rural landholders, and the Catholics in Germany were more rural than the Protestants. Indeed, without an urban-rural control, the Jews were the most "Protestant" of all.

There is more, much more, ranging from a discussion of Sumner as the St. Paul of the capitalist gospel to an analysis of Protestantism and progress country by country, from Belgium and the Netherlands to the American South. Samuelsson treats us to a model of historical sociology in the classical tradition; we shall not be able to ignore this work. As Homans says, *Religion and Economic Action* "does not just tinker with Weber's hypothesis but leaves it in ruins."

RAYMOND W. MACK

Northwestern University

The Behavioral Persuasion in Politics. By HEINZ EULAU. New York: Random House, 1963. Pp. 141. $1.95.

This book will not stimulate empirically oriented political sociologists, but may be useful to some social scientists as a fresh statement of the importance of behavioral research to understanding political life. The core chapters are taken from lectures delivered to undergraduates several years ago. They contain an interesting outline of role theory, a terribly vague discussion of culture, and a chapter on the importance of personality to the study of political behavior. Social scientists who are tired of tedious presentation of facts to support an author's argument may welcome this book. It is unencumbered by empirical data and aims instead for a lyrical statement of the importance of all things, the interdependence

of man to man, role to role, culture to politics, institutions to political behavior, personality to culture, and so on. The task facing the political scientist with a behavioral persuasion seems so vast that those who have not already experienced the joy of testing a specific hypothesis (as Eulau has himself done with distinction) cannot be blamed if they avoid a behavioral perspective a while longer.

At the risk of being unfair I believe that the tone of the book is conveyed by a short quote: "At the risk of ambiguity, I shall continue to use the term 'culture' without defining it for the simple reason that, among the host of over 250 available definitions, I know of none that is neither too narrow nor too inclusive. . . . As a concept, culture is a synthetic rather than an analytic construct, and I wish to use it to stimulate investigation, not to conduct it. In an essay of this sort, conceptual refinement may be more of a handicap than an advantage" (p. 63). It is my impression that essays of this sort are abundant and do little to convert the heathen to a just cause.

PHILLIPS CUTRIGHT

Social Security Administration

City Politics. By EDWARD C. BANFIELD and JAMES Q. WILSON. Cambridge, Mass.: Harvard University Press, 1963. Pp. 364. $6.95.

The city is not an abstraction for Banfield and Wilson; it is New York, Chicago, Los Angeles, Philadelphia, and every other major representative of the variegated American urban scene. They do justice to the richness and complexity of their raw material. The categories used are empirically rather than analytically drawn and, consequently, they can be clearly illustrated with one or more actual cities.

This verisimilitude is a considerable virtue. It gives the analysis political sophistication and vitality. At the same time, city politics is clearly viewed in a social organizational context. The city must cope both with the achievement of collective goals (the "service function") and with the management of conflicts of interest and values. "The explicitly political elements in the life of the city—especially governments, parties, and interest groups," the authors write, "although only a part of the complicated mix of integrative and disintegrative forces, are of principal importance in a study of city politics."

One recurring cleavage—a combination of ethos and ethnicity—is given a central focus. One side of the cleavage involves a set of beliefs which Banfield and Wilson call "good government" (the quotation marks are theirs and are used throughout). Those who hold such beliefs view the problems of city government as primarily technical; the emphasis is on the most efficient mobilization of resources for collective goals. It tends to be the ethos of middle-class Anglo-Saxons and Jews. On the other side of the cleavage, politics is a struggle for special advantage and the achievement of private ends. This is the politics of immigrant groups and Negroes.

While the authors see the "good government" forces as the wave of the future, it is with considerable misgiving. They see many important functions served by the old politics of personal obligations and private ends—an avenue of mobility for immigrants, the mobilization of influence in a system in which authority is widely dispersed, the integration of lower-class groups into the urban and the national community, and others. Politics is no dirty word in their dictionary.

Many differences between cities emerge—for example, the centralization of influence in Chicago and the decentralization of influence in Los Angeles. It is less clear what the social conditions are which produce one outcome rather than another. But no book can answer all questions about urban social structure, and it is enough that this one provides an important synthesis of the operation and function of politics in the American city.

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